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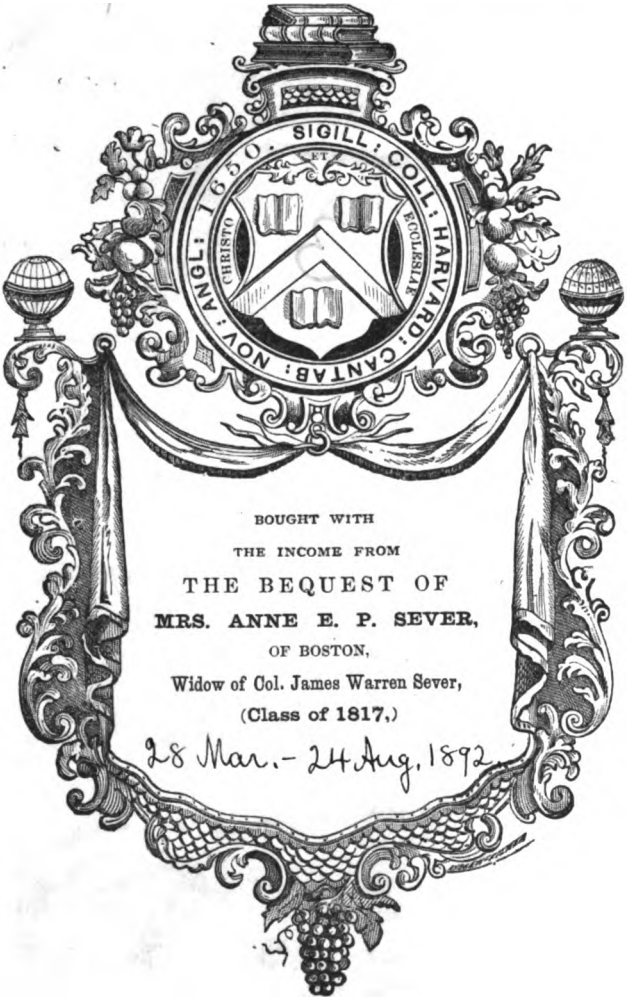

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THE IRON WORK FOR THE DOME OF THE PROPOSED GOVERNMENT BUILDING, WORLD'S COLUMBIAN EXPOSITION, CHICAGO, ILL.*

By JAMES C. MCGUIRE, Jun. Am. Soc. C. E.

It is the purpose of the author of this paper to outline as briefly as possible some of the characteristic features of the work, which may be of interest to engineers because of its size and method of construction. It is among the tallest domes in the world, which are built of any of the malleable metals. That at Vienna, Austria, exceeds it, being 279 feet high, but is entirely different in construction, being built like the frustrum of a pyramid instead of circular. This dome has been designed as only a temporary structure, and probably will not remain in position longer than one year.

The Foundations.—The soil in the part of Jackson Park, where the dome is to be built, has, by borings, been ascertained to consist for a depth of about 3 feet on top, of black soil; then for 11½ feet of quicksand; under this, 14½ feet of soft clay; then hard pan was penetrated for a distance of 13 feet. The dome rests on sixteen columns, and under each column

*Designed by the author, in the office of the Supervising Architect, Treasury Department, Washington, D. C.

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there are fifteen piles. The specifications require that, before getting out the piles for the work, the contractor must drive test piles in order to determine the actual length of pile required; each pile to be driven until a hammer weighing 2 500 pounds, falling 25 feet, will not sink the pile more than 1 inch, or equivalent thereto, and the head of the pile to remain whole, sound, and without split or flaw. As there is a load of about 418 000 pounds on each column, if this be equally distributed each pile will be required to carry 27 860 pounds; and, as nearly 90 per cent. of this is wind load, there will be no difficulty in securing a good foundation. These piles have been driven, and are from 20 to 30 feet in length. On top of the piles are three courses of cribbing for securely fastening together the heads of the different piles, and rendering the whole rigid and firm; this cribbing is securely bolted to the piles, and on it rests the shoe of the column, which is built of eye-beams and plates for the purpose of distributing the pressure over a larger area than is occupied by the base of the column.

The Dome.—The dome stands in the center of a rectangular building, 420 x 350 feet (see Plates I and II); but it is built entirely independent of the main building. At the floor level the dome proper (not including the outside sheathing) has an external long diameter of 118 feet, and it rises vertically 115 feet above the floor level of the rotunda, to the spring line. The ironwork finish is sixteen sided, but is architecturally treated, as seen from the interior of the main building, as an octagon (that is, the outside of the dome below the roof of the main building). After the dome emerges above the roof, the finish is sixteen sided until the cornice below the windows which extend around the dome is reached, and from thence it is cylindrical to the springing line (see Plate I). From the springing line the exterior is the surface of a sphere, very much on the order of the dome of the United States Capitol at Washington, D. C. The finish of the inside is sixteen sided throughout the entire height.

The columns supporting the dome have a depth of 6 feet from back to back of angles, and are composed of four angles 4 x 6½ inches, extending to the springing line, with two 14 x ½ inch cover plates which extend about 56 feet above the floor; the angles of the columns are latticed with double latticing at about 45 degrees inclination, the latticing being composed of a 3 x 5 inch angle. These columns are braced horizontally by struts in pairs, which form continuous

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PLATE I.
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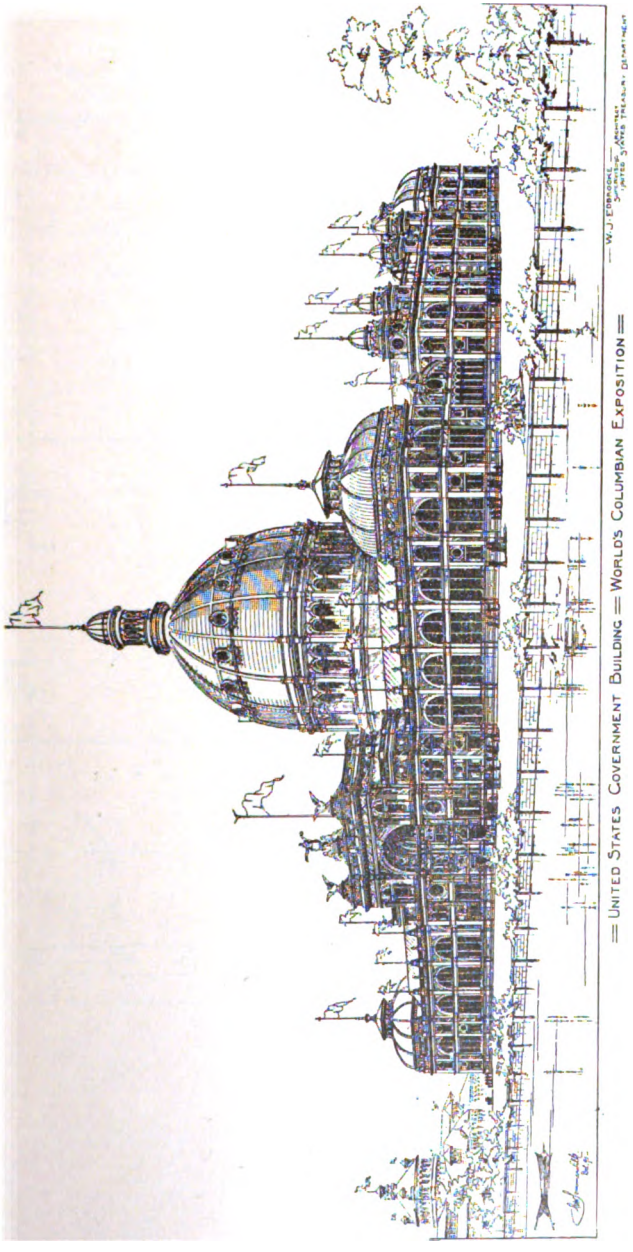

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PLATE II.
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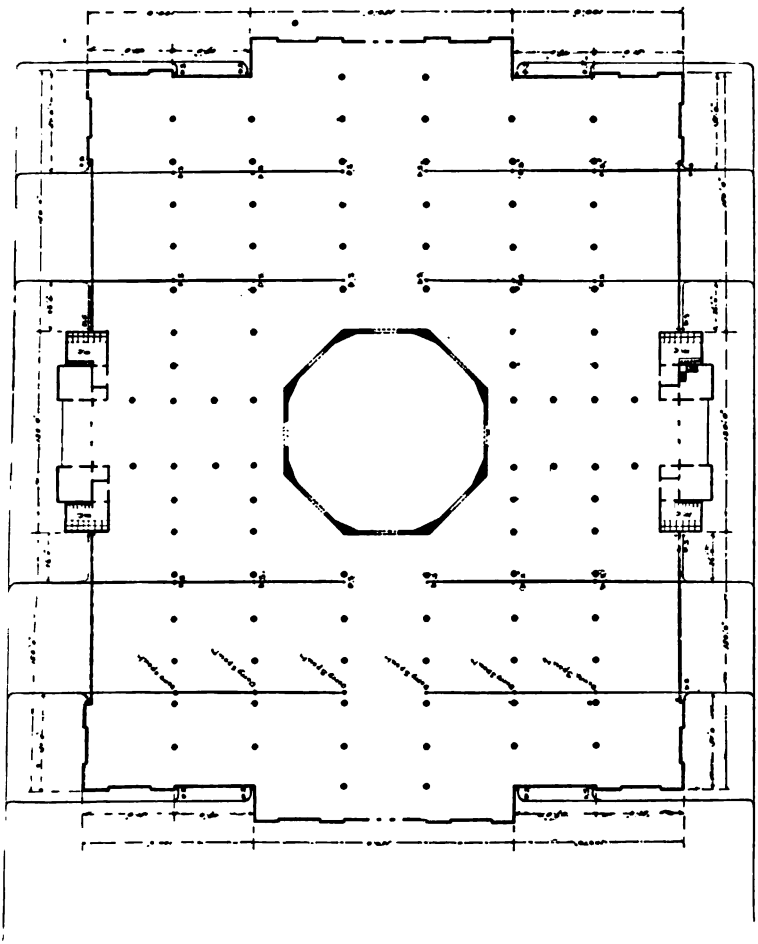
FIRST FLOOR PLAN.



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rings around the dome, each pair being from 20 to 26 feet apart; there is one pair of struts just below the springing line, and another under the floor, with four intermediate pairs. The outside one of these struts is as near the outside flange of the column as was possible to place it; while the inside strut is moved outwards a distance of 1 foot 10½ inches, because the rods which are attached to the struts and are in every other panel, would (if the struts were set against the inside flange of the columns) show after the interior finish is in place, as there is a recess between the columns.

These struts are composed of four 3 x 4 inch angles, double latticed, with 2 x $\frac{5}{16}$ inch lattice bars, and are 18 inches deep; except the first strut above the floor, which is 24 inches deep, and is latticed with double latticing of 2½ x 3 inch angles. These latter struts carry a basket balcony, the floor of which is 28 feet 6 inches above the first floor level; there being one of these balconies in every other panel over the eight entrances to the rotunda of the dome (see Plate III). The entrances, also, are in every other panel, as the bracing between the struts occupies the alternate ones. Two of the balconies are not accessible, the others are entered by a stair which leads up between the columns of the dome and between the struts. There are four stairways leading from the first floor to four of the balconies, and two of the balconies are entered by going from these to a gallery in the main building outside of the dome, and from this gallery into the balconies in question.

The balconies extend back between the columns, and four of them, as previously stated, have a stairway entering on one side, which comes up through the columns. Directly opposite the landing of the stairs, and on the opposite side of the balconies, there are four stairways starting, which extend up to the gallery, the floor of which is 75 feet above the floor of the rotunda. This gallery extends entirely around on the inside of the dome, and is supported on brackets fastened to the inside of the columns; eyebeams extend from one bracket to the other, upon which the floor joists are fastened, which support the floor.

It is not the intention to admit the general public above this level, but there is provided a single stairway which leads from the gallery to the lantern; which continues on through the ribs of the dome and between the struts, until it is above the springing line of the dome, when it turns and goes up parallel to one of the ribs, it being suspended from

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this rib by 1-inch round rods. The stairs are braced at each suspension point by five-eighth-inch guy rods, which extend out on either side and are fastened to the struts of the dome. The stairs are composed of two 6-inch channels, which form the stringers, with pine treads bolted to angle brackets, which are fastened to the stringers after the usual manner; and a 1½-inch gas pipe railing on either side of the stairway, with the balusters of the same screwed in cast iron lugs which are bolted to the stringers.

At the gallery level there are windows in every panel, extending around the dome, which made it necessary to do away with the rod bracing entirely at this level. In order to strengthen the columns at these points, large ⅜-inch thick plates, as knee-braces, were used, both at the outside and inside strut; these plates being stiffened by 3 x 3 inch angles. These plates were made as large as possible without interfering with the window openings, the plates being on the struts, both above and below the windows (see Plate VII).

From the springing line the ribs converge towards the center of the dome. The depth of the ribs is 6 feet at the springing line from back to back of angles, and at the top of the crown of the dome, it is 3 feet from back to back of the angles, with a circular opening at this point of 17 feet diameter, over which the lantern is placed. The ribs above the springing line consist of two 6 x 6 inch angles for each flange; the ribs are divided into fifteen panels, by two 3 x 5 inch angles, which are normal to the exterior flange of the rib, making each panel a little less than 6 feet long; and in each panel there is double latticing composed of 5 x 3 inch angles. Strut No. 5, at the springing line, takes the outward thrust of the ribs of the dome; and the ring at the top of the dome, which is 3 feet deep and has a web ⅜-inch thick, with chord angles 3 x 5 inches, takes up the corresponding inward thrust. On top of this ring, and at a point 180 feet 10 inches above the floor of the rotunda, the lantern starts, and there is a rib of the lantern for each rib of the dome, to which latter the ribs of the lantern are connected. At every third panel point of the ribs above the springing line there is a strut, which is the full depth of the rib, composed of four 3 x 3½ inch angles, double latticed, with 3 x 3 inch angles; there being four rings of such struts, with rod bracing between them, the rods continuing up in every other panel between the ribs; there being only one set of these rods above the springing line and fastened to the inside flange of the struts.

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At panel points where there are no struts the entire depth of the rib, there are struts 18 inches deep, which form continuous rings around the dome, and act as purlins. These purlins are composed of four chord angles, 2½ x 3½ inch, with double latticing of 2 x $\frac{5}{16}$ inch bars. To these struts and the deep struts, at intervals of 2 feet, are bolted 2 x 4 inch timbers which run vertically, and to which the 1-inch sheathing is nailed, the sheathing being nailed diagonally.

The ribs of the lantern are composed of four 3 x 3 inch angles, latticed with 2 x $\frac{5}{16}$ inch lattice bars; the ribs are braced by horizontal rings, which extend around the lantern, and are composed of a $\frac{5}{16}$-inch plate, with a 3 x 3 inch angle, riveted to the top and bottom of this plate; these plates being used in pairs, one on the outside and one on the inside flange of the ribs of the lantern. Great difficulty was found in getting suitable bracing between these rings, as the columns of the lantern are so close together that if there are rods between all columns, the angle of inclination becomes very acute; so the method was adopted as shown on the drawing of the lantern. This lantern is 47 feet 1½ inches high, making the total height from the floor of the rotunda, to the top of iron-work, 227 feet 11½ inches. As there is a lot of ornamental work on the outside of this, the height to the top of the lantern may be said to be 235 feet 5½ inches, and to the top of flag pole is about 275 feet.

At about the springing line on the interior of the dome, the diaphragm starts, with a radius of 53 feet 4 inches. It is composed of vertical ribs of two 2½ x 3 inch angles, suspended by ⅝ inch rods from the main ribs of the dome. Upon these ribs, at intervals of 2 feet 4½ inches, are the purlins, composed of 3 x 3 inch angles, to which the galvanized covering of the diaphragm is fastened. There is a circular opening in the top of the diaphragm 16 feet diameter, and over this a canopy, which is a portion of the surface of a sphere with a radius of 11 feet 8½ inches.

Calculations.—It is not the intention of the author to go fully into the calculations embraced in the design of the proposed structure, as it would be both tedious and long, and therefore only the general method which was adopted will be given.

First, the stresses in the dome proper were found and then in the columns. The assumption to start with was that the dome is a braced arch hinged at the crown and ends, the ends being at the springing line; as a matter of fact it is all riveted up tight. The vertical loads were determined on the assumption that, including wind, snow, and

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the dead weight of the material, the vertical load would be 50 pounds to the square foot on the horizontal projection of the surface of the dome.

The braced arch was first considered, of course, as of single intersection for the calculations, and then as of double intersection, considering one-half of the stress to be carried by one member of a panel in tension, and by the other member of the same panel in compression.

The diagram for these stresses is the one marked "Vertical Loads" (see Plate XI). One member of this diagram had to be computed, the simplest of which was the outward thrust. This was found by taking the panel loads on the horizontal projection of the surface of the dome and considering them as acting at the panel points, and taking the sum of all the moments of the forces around the crown. Since the structure is in equilibrium, this is equal to the moment of the outward thrust by the depth; hence dividing the sum of the moments of these forces as previously found, by the depth, we have the outward thrust; then by the diagram of forces it is seen that a normal force of unity, puts a stress of tension in the struts at the springing line of 2.55; and hence we get the tension in these struts by multiplying this normal thrust by 2.55.

The sum of the panel loads between the crown and springing line is equal to the reaction, and having this and the horizontal thrust, which we will call "T," the diagram was constructed from which the stresses in all the members of the dome were easily found. The vertical loads are given in the table and are marked "V." It is to be noted at panel one, that we have the dead and live load of the lantern acting at this point. The balcony around the lantern has been considered filled with people, the load being at 80 pounds per square foot.

The next forces to be considered are the horizontal wind forces acting on the lantern. As there are the same number of ribs in the lantern as in the dome, the author has taken the vertical projection of one panel of the lantern, assuming the wind to blow perpendicularly to this panel with a force of 50 pounds per square foot (this is a very high value, but considering the exposed position which the lantern occupies, it is not far on the safe side), this force on the lantern gives us the shear at the base; and to find the moment which this force exerts at the foot of the lantern, we have the shear multiplied by the distance to the center of pressure, which is at one-half the height of the lantern.

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Stresses in Members of Truss.—(See Plate XI and Fig. 1.)

Top Chord.

No.	2	4	10	12	18	19	26	28	34	35	41	42	49	50	57	60
V	—11 100	—9 100	+1 100	+2 650	+4 600	+7 000	+3 750	+6 250	+200	+2 400	—3 750	—2 200	—6 100	—5 250	—6 350	—5 350
L	.....	—31 500	+32 200	—32 200	—33 700	—33 700	—36 500	—36 500	—40 700	—40 700	—46 500	—46 500	—56 000	—56 000	—63 800	.....
H	.....	—28 00[illegible]	—66 20[illegible]	—67 700	—86 500	—88 700	—90 700	—93 200	—82 500	—84 600	—64 000	—65 500	—37 700	—38 000	—11 500	.....

Bottom Chord.

No.	1	6–8	14–15	22–23	30–31	37–38	45–46	53–54	59
V	—30 500	—32 700	—36 800	—33 200	—25 200	—16 000	—8 900	—5 400	—5 000
L	.....	+29 200	+30 100	+32 000	+34 800	+38 700	+44 500	+54 300	.....
H	.....	+51 300	+79 300	+86 200	+77 500	+57 600	+30 000	+1 000	—14 600

Diagonals.

No.	C	5	9	13	17	21	25	29	33	36*a*	40	44	48	52	56	
V	.....	+7 000	—5 800	—600	—800	—5 350	+2 800	—7 350	+4 400	—6 850	+3 750	—4 500	+2 000	—2 000	+100	.....
L	+2 600	+3 000	+4 300	+2 800	+5 100	+2 500	+6 200	+2 300	+7 800	+2 500	+9 500	+2 300	+13 600	+2 000	+1 100	.....
H	.....	—29 500	+26 700	—12 000	+13 400	+3 000	+2 500	+12 500	—6 000	+18 000	—11 500	+19 700	+16 006	+17 500	—15 600	.....

Radials.

No.	3		11		20		27		36		43		51		58	
V	+5 800	.....	—600	.....	—1 600	.....	—2 200	.....	—2 450	.....	—2 000	.....	—1 400	.....	—1 350	.....
H	—4 500	.....	+200	.....	+2 500	.....	—4 000	.....	+5 000	.....	+4 200	.....	+3 000	.....	+6 000	.....

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The stresses in the various members from this force are marked "L" and are found by the diagram of forces (Fig. 1); having calculated first the stress in one member, No. 57, which is equal to the moment previously found divided by the perpendicular depth of the rib at this point, from this the diagram was easily constructed; we could have found the stress in any other member by simply considering the truss at any point as having to resist this moment. It will be noticed from the table that on approaching the spring line where the depth of truss becomes greater, the chord stress decreases. The chord stresses may be found at any

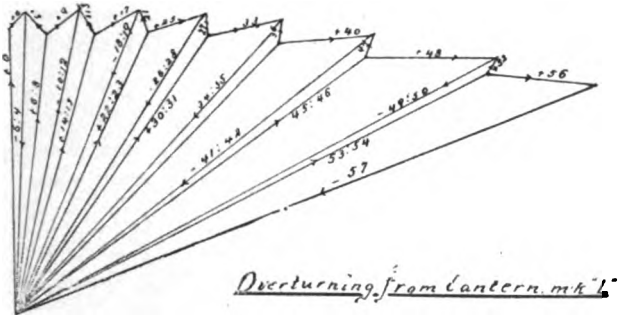

FIG. 1.

point by simply dividing the moment previously found by the perpendicular depth of the rib at the desired point.

Next to be considered is the effect of the horizontal wind force on the dome itself. This has been done in the same way, practically, that the stresses from the vertical loads were found. The wind has been taken at 50 pounds per square foot and considered as acting on the vertical projection of one panel, and the panel loads have been taken as acting at the panel points; from this the reaction was found by the method of moments both at the top and bottom of the rib. The stresses thus found are marked "H" (Fig. 2) and were taken for the right hand portion of the rib, because of convenience, and only the left half of the arch has been shown in the plates.

Stresses in Rod Bracing.—Rod bracing has been used in every other panel, extending from the ground to the lantern. Since we wish all the rods around the dome to bear equal stresses when the wind is blowing, the component borne by each will be in proportion to the projection of their lengths on a plane parallel to the direction of the wind. By inspection it was first found in what direction in reference to a braced

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panel the wind would have to blow, in order that this projection might be a minimum. The wind can blow perpendicularly to a braced panel, and perpendicularly to an unbraced panel, and only at points in between. Now the projection of the rods in the direction of the wind when the

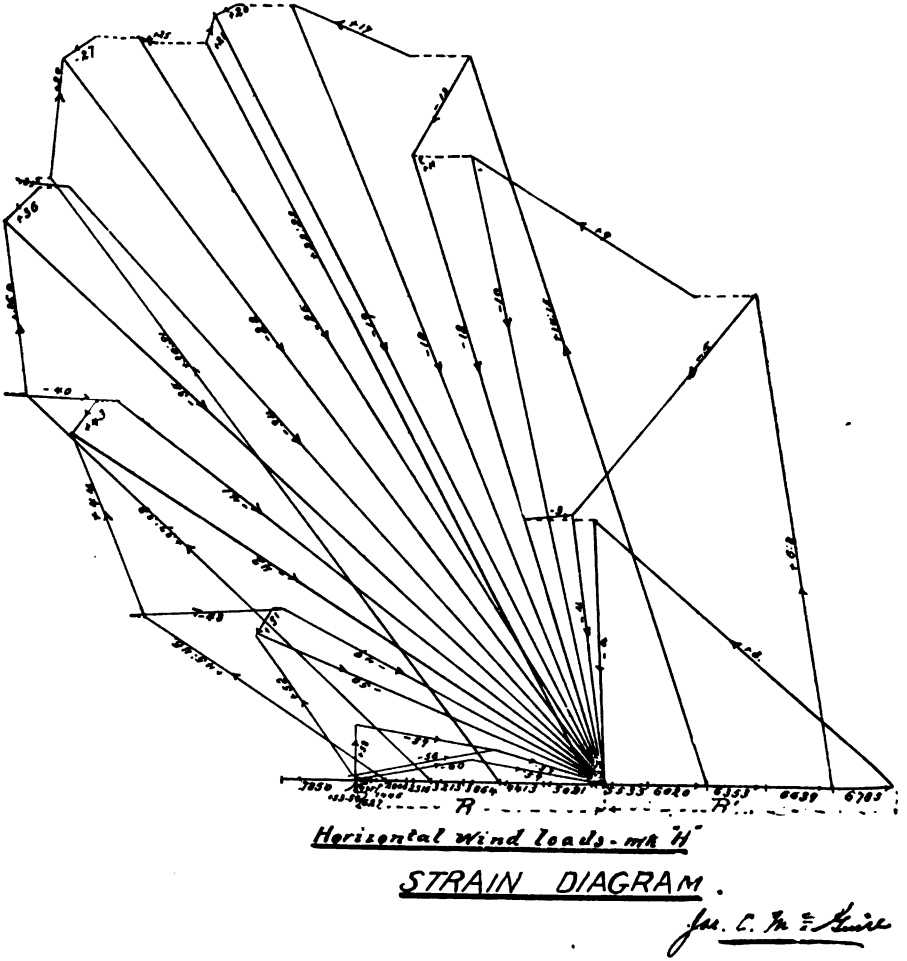

FIG. 2.

wind is blowing perpendicularly to a braced panel (the panel length being 23 feet) is $111\frac{2}{10}$ feet; and when the wind is blowing perpendicularly to an unbraced panel it is 120 feet; therefore, when the wind is blowing perpendicularly to a braced panel it is more severe on the rods than in

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any other direction from which it can blow. Consequently, each panel of rods carries twenty-three divided by one hundred and eleven and two-tenths, which is equal to two hundred and six thousandths of the entire thrust on the structure.

Now when taking the pressure on a circular structure considered as a whole, the value of that pressure is proportinal to six-tenths of the total pressure on its vertical projection. The rods in the dome are only between the deep struts which are in every third panel, three panels of the rib being equal to one panel of rods. Now for the first set of rods in the dome we have acting at the top the shear from the lantern, which is equal to its vertical projection multiplied by the wind pressure per square foot (or, the same thing, its diameter multiplied by its height by the pressure per square foot). To this we have to add half of the first panel load on the rods, which we consider acting at the top, found by taking the diameter of the dome at this point multiplied by the pressure per square foot, adding this to the shear from the lantern, and taking six-tenths of the total, gives us the total stress which has to be taken up by the rods at this elevation, and, as previously stated, all the rods around the dome carry the same, each rod has to be designed to carry two hundred and six thousandths of this total pressure. Multiplying this result by the secant of the inclination of the rods to the struts, measured in the plane of the rods, we have the stress in the rods; and so on for each deep strut. Considering one-half the panel load above and one-half the panel load below to act at the point in question, and adding, of course, what the rod above brings down; and so on down to the springing line. When this point is reached we have two sets of rods, and two sets of struts, one on the outer flange of the column, and one near the inner flange of the column.

The rods in the first two panels above the rotunda floor are of the same size as they are in the third panel above, as the dome is protected to about the level of the second strut above the floor by the main portion of the building, which is built entirely independent of the dome.

Stresses in Columns of Dome.—Starting at the springing line the stresses are induced by three different causes:

First.—The vertical, live and dead loads.

Second.—Overturning force of the wind on that portion of the dome above the springing line.

Third.—Direct wind, transmitted to the columns through the rods.

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Taking the first of these causes, we have acting at the top of the columns or the springing line a certain load caused by the vertical pressure on the dome, which we have already found; and the author has assumed that for every lineal foot on each column below the springing line there is a dead load of 200 pounds.

2d. The overturning effect of the wind on that portion above the springing line. This was found by taking the same panel loads as were used in determining the stress in the rods; first taking the pressure on the lantern, then taking six-tenths of it, as we did before, multiplying this pressure by the distance from the springing line to the center of pressure, we get the moment of this force around the springing line, and treating each of the panel loads the same, that is, multiplying them by the vertical distance from the springing line, and then taking the sum of these moments, we get the moment of the wind about the spring line; this has to be resisted by the columns, and assuming each column to carry an amount proportionate to the different diameters of the dome, measured from the head of one column, to the head of the column directly opposite, and taking the sum of these distances, we have the leverage:

$$\begin{aligned} 2 \times 116 \text{ feet} &= 232 \text{ feet.} \\ 2 \times 98 \text{ ``} &= 196 \text{ ``} \\ 2 \times 66 \text{ ``} &= 132 \text{ ``} \\ 2 \times 23 \text{ ``} &= 46 \text{ ``} \\ &\overline{606} \text{ ``} \end{aligned}$$

we have the total leverage by which this amount is resisted as 606 feet, and dividing the total moment from the wind as found above, by this, we have the down push on all the columns.

3d. Direct wind stresses: These stresses were found by using the same panel loads as were used in determining the stresses in the rods; only we multiply by the tangent of the inclination to the vertical, adding, of course, at each strut what has previously been brought down to that point (Fig. 3).

Stair Openings.—Where the stairway passes through the columns, of course, the latticing of the columns has to be omitted, and the maximum openings that could be allowed were determined as follows: Let X equal the maximum opening; then we have the moment of inertia of the section of one flange of the column, around an axis parallel to the long side of the angles and through the center of gravity of this section (as it is

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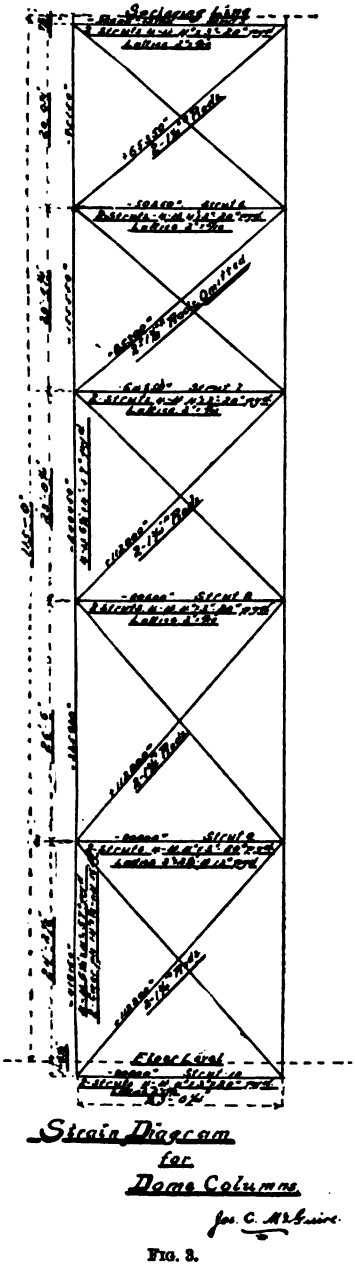

FIG. 3.

about this axis that the column will fail if the opening is made too great) is to X, as the moment of inertia of this same section, around an axis parallel to the short legs of the angles, and through the center of gravity of the section, is to the distance between struts; having solved this equation for X, this opening was found.

The uplift at the foot of each column was found by simply taking the moment of all the horizontal forces around the base of the columns, and as this moment has to be resisted by the uplift on the anchor bolts (plus the dead weight of the dome), multiplied by the effective diameter of the dome; we have this moment of all the horizontal forces, divided by 606 (which we previously found to be the effective diameter of the dome). From this resultant the dead weight was subtracted, leaving the actual uplift on the anchor bolts.

Specification.—The material specified for the dome is mild steel, with an ultimate tensile strength from sixty to sixty-eight thousand pounds per square inch, with a percentage of elongation of not less than one million two hundred thousand divided by the ultimate tensile strength in pounds per square inch; and with a percentage of reduction of area of not less than two million four hundred thousand divided by the ultimate tensile strength in pounds per square inch.

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The estimated weight of the iron and steel included in the lantern and dome is about 1 265 000 pounds, the contract for furnishing which is held by Hugh, Ketcham & Co., of Indianapolis, Ind. All of this material at the date of writing has been rolled, and the contract requires that the ironwork shall be finished and in place by the fifth day of June, 1892, under a penalty of $100 for every day's delay beyond that date.

All material of the dome is steel, except the rods, which are iron, and also some lattice bars for the struts.

The author desires to express his thanks to Mr. H. W. Hodge, Chief Engineer of the Union Iron Works of New York City, for valuable assistance as to the method of calculations received from him, while designing this work.

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Plate III

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Plate IV.

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Plate V.

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PLATE VI

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PLATE VII

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PLATE VIII.

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PLATE IX

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PLATE X

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Plate XI.

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TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

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(Vol. XXVI.—January, 1892.)

ON THE HYDRAULICS OF THE HEMLOCK LAKE CONDUIT OF THE ROCHESTER, N. Y., WATER-WORKS.

By GEORGE W. RAFTER, M. Am. Soc. C. E.

WITH DISCUSSION.

The Rochester Water-works were made a dual system, involving a domestic supply by gravity from Hemlock Lake and a supply by direct pressure from the Genesee River, for the suppression of fires. Upon the completion of the line from Hemlock Lake to Rush Reservoir its designing engineers published its capacity under normal conditions, as 9 292 800 gallons in twenty-four hours. In the annual report of the Executive Board of the City of Rochester for the year 1876 may be found a discussion of the hydraulic questions involved, together with a theoretical demonstration, that the daily discharge of 9 292 800 gallons was in accordance with the more recent determinations as to the discharge of large conduits.

The source of the domestic water supply of the City of Rochester, Hemlock Lake, is 385.6 feet above the city datum, and distant, along the pipe line, about 29.3 miles. Mount Hope, the distributing reservoir, is

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about 1.4 miles from the center of the city, and the normal water surface is 124.4 feet above the city datum. When filled to its normal depth of 15 feet, Mount Hope Reservoir stores 22 500 000 gallons of water. Rush Reservoir, intended chiefly for additional storage, is distant from Mount Hope 46 064 feet (8.72 miles) along the pipe line.* The normal water surface at Rush Reservoir is 117.4 feet above the normal water surface at Mount Hope, the depth in the Rush Reservoir being, at normal elevation, 18 feet. At this depth it stores 70 000 000 gallons. From Rush Reservoir to Hemlock Lake the distance is 101 261 feet (19.2 miles) along the pipe line,† and the mean low water surface of the lake is 143.8 feet above normal water surface at Rush Reservoir, or 385.6 feet above city datum as stated in the foregoing. Leading from Hemlock Lake toward Rush Reservoir (see profile) the first 50 776 feet of the conduit is 36-inch wrought-iron pipe, this portion of the line having been laid with a total fall of 21 feet or on a grade of 0.411 per 1 000 feet. The diameter for the balance of the distance from the end of the 36-inch wrought-iron pipe to Rush Reservoir is 24 inches, the material being partly wrought-iron pipe and partly cast-iron as defined in the following: (1) From the end of the 36-inch wrought-iron pipe 1 914 linear feet of 24-inch wrought-iron pipe; (2) 30 550 lineal feet of cast-iron; (3) 13 809 linear feet of wrought-iron pipe; (4) finally, 4 212 feet of cast-iron pipe to the face of the gate-house at Rush Reservoir. Adding the length for the continuation of the pipe line to the point of discharge in the bottom of the reservoir, gives a total of 5 222 feet. From the end of the 36-inch wrought-iron pipe to Rush Reservoir the grade is continuous, and taken to the end of the pipe in the discharge well it is to normal water surface at the rate of 2.26 feet per 1 000. Between Rush and Mount Hope Reservoirs the conduit is of 24-inch cast-iron pipe, with a grade, when reckoned from normal water surface of both reservoirs, of 2.51 per 1 000 feet.

In designing this pipe line the engineers considered it desirable to make two grades, forming thereby a compound conduit. The location actually adopted, however, involved a number of cuts, in order to keep the upper section (the 36-inch) down to the assumed grade of the line located, and at the several points where these cuts occur the actual position of the conduit is shown on the profile by a heavy line. At

* This is the distance as measured from face to face of gate-houses.
Also measured from face to face of gate-houses.

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points where the natural surface of the ground is below the assumed limiting pipe grade, the upper side of the pipe is merely below the surface of the ground far enough to insure safety from freezing; but inasmuch as the profile is on an exaggerated scale, it is obviously difficult to show the actual location of the pipe in relation to the ground surface at every point. In studying the profile it is sufficient to remember that at all points other than in the cuts where the actual position of the pipe is shown, as already described, the pipe line is, generally speaking, nearly parallel to the surface of the ground and from 3 to 5 feet below it, slight variations having been made in order to ease off curves at places where the natural surface changes abruptly. The dotted line connecting the heavy portions indicating the pipe line in the cuts, may be taken as showing where the top of the pipe would be for the whole distance, provided it were laid *in train* to a continuous grade, said dotted line being of course parallel to the theoretical grade line along the center of the pipe and 1.5 feet above it for the 36-inch section. From air valve 53 to Rush Reservoir the conduit is, as indicated on the profile, 24 inches in diameter, and the approximate position of the hydraulic grade, when the conditions of delivery are such as to give the lowest possible position of the hydraulic grade, is shown by a dotted line.

Obviously the conditions which determine this position of the hydraulic grade are, that the elevation of the lake surface be such as to just run the 36-inch section full at the upper end, in which case the hydraulic conditions for the portions of the 36-inch conduit which are laid to a pipe grade in the cuts would be expressed by saying a full pipe in train; the hydraulic grade of the 36-inch, under these conditions, corresponding to the pipe grade, provided the resistance is the same throughout the whole extent of the 36-inch pipe. The approximate position of the hydraulic grade at the time of making the following described tests is shown on the profile by broken line, thus: ——— - - ——— - - ——— - - (Plate XII).

The foregoing gives rather briefly the main points in the hydraulics of this pipe line, and we may now proceed to a brief discussion of some of the recent determinations of the actual delivery of this pipe as made by the present writer during the last season. Before doing this, however, it is proper to state that when the Rochester water-works were considered finished in 1876, the population of the city was about 82 000. In 1880 it was 89 000, and in 1890 the census returns show 133 896. This considerable increase in population has naturally led to a large use of water, and

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for the past three seasons the storage has been so drawn upon as to render the situation at times somewhat alarming. On the 1st of May, 1890, the storage at Rush Reservoir was about 44 000 000 gallons. As warm weather came on in June this was drawn upon at the rate of from 1 000 000 to 2 000 000 gallons per day, and at this rate of decrease it could be only a very small number of days before the city would be without any storage at all.

Mr. Brush, in the discussion of a paper on the "Fresh Water Algæ," etc., which the author had the honor to read before the Society about two years ago, has vividly described the excitement prevailing in Hoboken at a time when the water supply of that city became seriously affected with bad tastes and odors. His description is quite tame in comparison with the hubbub prevailing in Rochester in 1890 at the prospect of a serious water famine in the middle of the summer. The city papers were filled with editorials and communications from citizens, and at the height of the excitement the Executive Board of the city, as being charged by law with the care and maintenance of the water-works, directed the present writer to take such measures as would, if possible, avert a water famine at that time. The language of the resolution of the Executive Board directing such measures was exceedingly strong, and under its provisions full authority was given to adopt any precautions and make any expenditures necessary to produce the desired result.

During all this time of popular discussion, everything said and written had been on the supposition that the Hemlock Lake conduit was delivering at the rate indicated by the measurement made soon after its completion in 1876, or, at the rate, under normal conditions, of about 9 292 800 gallons in twenty-four hours. It appeared to the author, however, that the proper basis of any restrictive measures would be a clear idea of the present delivery, and accordingly, his first step was in the direction of determining definitely what quantity was being delivered at that time. For this purpose four measurements were made, and as the mean of the four it was ascertained that the actual delivery, including allowance for evaporation at Rush Reservoir and other allowances, was at the rate of 6 742 000 gallons per day, taken in the discussion for even figures at 6 700 000 gallons, a quantity representing approximately the net delivery. Relative to these measurements it may be stated that the determination of the quantity of flow was obtained by observing the height to which the water rose in Rush Reservoir in a

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given period of time. The dimensions of the reservoir being known, the daily inflow is obtained by a simple computation. Three of the four observations, of which the mean is given as 6 742 000 gallons in twenty-four hours, were for a period of twelve hours and one for a period of twenty-four hours.

At the time of making these measurements the surface of Hemlock Lake was considerably above normal surface, while the water surface at Rush Reservoir, into which the measured delivery took place, was several feet below normal elevation, the difference due to such departure from the normal being sufficient, if the conduit were in proper hydraulic condition, to produce a delivery of about 9 700 000 gallons in twenty-four hours instead of the 6 700 000 gallons which was actually found to exist; that is to say, the delivery was about 3 000 000 short of its capacity as stated in 1876.

In October a further measurement of the discharge was made by E. Kuichling, M. Am. Soc. C. E., for a period of seven hours and the foregoing results verified; the flow at that time being, with liberal allowance for evaporation, percolation, etc., at the rate of a trifle over 7 000 000 gallons per day. The author's own measurements are believed to be accurate within limits of 2 or 3 per cent. The difference thus found to exist between the actual delivery at the present time and the delivery as stated in 1876 is so exceedingly marked that the author has concluded a brief discussion of the hydraulic questions involved, together with a short statement of some further investigation, undertaken with a view of determining why the falling off should be so great, would be of interest to the Society, and this paper is an attempt to briefly present the same for consideration and discussion. The restrictive measures designed and successfully applied will be described in another paper.

The question of actual delivery being settled, the Executive Board further directed the author to make an investigation with a view of determining the reason for the considerable loss in delivering capacity of the conduit, and for such work the numerous air valves along the line offered convenient points of attachment for the application of piezometers. Moreover, for the 36-inch section the vertical distance from the surface of the ground to the hydraulic grade line was not so great but that actual piezometer heights could be measured by setting up tubes of wrought-iron pipe and taking the elevation of the top of the piezometric column by direct instrumental observation. For the 36-inch section and that

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portion of the 24-inch above air-valve 48, the determinations were all made by direct observation of the height of the piezometric column as indicated, while for the balance of the line the determinations were made by the use of pressure gauges. As preliminary to the work of determining the height of the piezometric column at the various air valves along the conduit line the levels were rerun from Hemlock Lake to Rush Reservoir and new bench marks established at or near each air valve. For this work two levelers were employed, each working independent of the other. The conditions expressed in the instructions to them were that their work must agree at any given bench mark within 0.02 of a foot, or failing to agree within that limit, the work was to be rerun until this limit of accuracy was reached. The greatest variation on any bench mark is 0.015 feet, and this was secured, generally speaking, without rerunning the work. What this represents in the way of care on the part of the levelers may be inferred from the character of the ground as indicated by the profile.

In taking the heights of the columns at the air valves, the stand pipe was extended vertically upward with wrought-iron pipe of the same size, this being 2 inches in the case of one pattern of air valve and three and a half in another. After so extending the stand pipe temporarily, the valve was opened and the column of water allowed to rise to its proper piezometric height; and after waiting a few minutes the height of water surface was determined by measurement to a point previously determined instrumentally on the air valve box below. In a few cases where the piezometric height left the top of the water column below the surface of the ground, the measurement was taken from instrumentally determined points down to the water surface rather than up. The location of points where this condition occurred is shown on the profile (Plate XII). The observations as to height of piezometric column were all made by two independent observers, recorded by each in his own note-book, and compared only after the necessary numerical computations for the elevation of water surface in the tube had been made. On that portion of the line where the distance from the surface of the ground to the hydraulic grade line is too great to admit of the application of tubes, and consequently the direct measurement of the actual height of column, pressure gauges were substituted as already noted. For this purpose two gauges were attached to each air valve at the same time, and allowed to remain for half an hour. The elevations of the centers of the gauges were instru-

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mentally determined in a manner similar to that previously described for the height of piezometric column, and likewise every measurement and reading of gauge was checked by two observers working independently.

From data so derived the following table has been constructed:

TABLE No. 1.

SHOWING LOCATION OF AIR VALVES ON THE PRESENT CONDUIT OF THE ROCHESTER WATER-WORKS, AND THE HYDRAULIC GRADES BETWEEN SAID VALVES AS ACTUALLY EXISTING IN JULY AND AUGUST, 1890.

Number of air valve.	Station.	Distance in feet.	Elevation of hydraulic grade.	Hydraulic gradient, rate per 1 000 feet.	Number of air valve.	Station.	Distance in feet.	Elevation of hydraulic grade.	Hydraulic gradient, rate per 1 000 feet.
Well-house.	0	0	387.55		56.....	486x15	1 545	365.53	0.48
91.....	91x96	9 198	383.49	0.44	55.....	489x09	294	365.33	0.67
90.....	96x21	623			54.....	497x03	794	365.08	0.31
89.....	106x12	791			53.....	508x19	1 116	364.65	0.38
88.....	114x30	818	382.75	0.33	52.....	527x20	1 901	357.37	3.83
87.....	120x06	576	382.42	0.57	51.....	536x58	938	355.18	2.33
86.....	128x20	814			50.....	545x02	844	353.35	2.17
85.....	132x98	478			49.....	562x09	1 707	349.57	2.21
84.....	139x83	685			48.....	569x46	737	347.94	2.21
83.....	150x08	1 025			47.....	591x41	2 195	343.55	1.99
82.....	161x45	1 137			46.....	610x36	1 895	338.40	2.71
81.....	165x33	388			45.....	615x14	478	337.78	1.29
80.....	171x20	587			44.....	660x38	4 524	327.35	2.30
79.....	184x15	1 295	379.48	0.46	43.....	662x35	197		
78.....	191x32	717	379.20	0.39	42.....	677x10	1 475	323.72	2.17
77.....	200x92	960	378.81	0.40	41.....	693x23	1 613	320.03	2.28
76.....	220x37	1 945	377.97	0.43	40.....	699x78	655	319.05	1.50
75.....	230x87	1 050	377.51	0.44	39.....	716x35	1 657	316.05	1.81
74.....	258x75	2 788	375.94	0.56	38.....	726x16	981	311.81	3.03
73.....	266x06	731	375.74	0.27	37.....	741x80	1 564	309.19	1.67
72.....	278x17	1 211	375.24	0.41	36.....	787x63	4 583	300.51 305 20	1.89
71.....	281x19	302	375.13	0.38					
70.....	292x51	1 132	374.64	0.43	35.....	828x57	4 094	298.47	1.64
69.....	313x17	2 066	373.71	0.46	34.....	849x13	2 056	292.38	2.96
68.....	323x45	1 028	373.18	0.51	33.....	856x23	710	288.18	5.91
67.....	332x52	907	372.65	0.58	32.....	867x33	1 110	284.45	3.36
66.....	343x68	1 116	372.29	0.32	31.....	870x97	364	283.10	3.71
65.....	359x78	1 610			30.....	882x40	1 143	279.58	3.08
64.....	380x38	2 060	370.88	0.38	29.....	897x91	1 551	272.91	4.30
63.....	391x08	1 070	370.39	0.47	28.....	922x37	2 446	265.69	2.95
62.....	406x26	1 518	369.65	0.48	27.....	928x42	605	263.65	3.37
61.....	422x10	1 584	369.00	0.41	26.....	948x55	2 013	256.07	3.76
60.....	428x62	652	368.36	0.98	25.....	954x54	599	254.62	2.42
59.....	441x34	1 272	367.62	0.57	24.....	984x04	2 950	245.31	3.15
58.....	463x70	2 236	366.61	0.45	23.....	1 002x67	1 863	241.79	1.89
57.....	470x70	700	366.28	0.47	Reservoir.	.....	.....	236.77	

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The foregoing Table No. 1, gives the elevation of the hydraulic gradient at nearly all the air valves, and the variation in the rate between the different valves. By way of classifying these variations as to the kind of material of which the pipe line is composed, Table No. 2 was constructed, showing the rate of hydraulic gradient in the wrought-iron and cast-iron sections separately. The air valves, however, are not located at the ends of the different sections, and per consequence sections of wrought-iron lap short distances into cast and *vice versa*, the amount of such lapping being determined by reference to the profile in connection with the column of remarks in Table No. 2.

In reference to these grades for short distances, it may be remarked, that by repeating the tests on different days it was observed that slight variations from the previous results were in some instances found. If, however, we consider long stretches of pipe, as has been done in Table No. 2, the error incident to the shorter distances is thereby eliminated, and we reach results which may be accepted as representing the real state of the case within a small limit of possible error.

TABLE No. 2.

Air Valve.	Station.	Distance in Feet.	Elevation of Hydraulic Grade.	Rate of Hydraulic Gradient per 1 000 Feet.	Remarks.
Well-house at Lake..	0	0	387.55	....	36-inch wrought-iron pipe.
53	508 + 19	50 819	364.65	0.45	24-inch wrought-iron pipe.
52	527 + 20	1 901	357.37	3.83	14 lineal feet 24-inch wrought-iron pipe. 30 123 lineal feet 24-inch cast-iron pipe.
35	828 + 57	30 137	298.47	1.95	30 137 = total between valves.
24	964 + 04	15 547	245.31	3.43	427 lineal feet 24-inch cast-iron pipe. 13 809 lineal feet 24-inch wrought-iron pipe. 1 311 lineal feet 24-inch cast-iron pipe. 15 547 = total between valves.
34	849 + 13	....	292.38	....	24-inch wrought-iron pipe.
25	954 + 54	10 541	254.62	3.58	

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In order to compare the results of these tests, we will assume that, for the observed discharge, the rate of hydraulic gradient in the 36-inch pipe ought not to exceed 0.3 feet per 1 000 feet, while in the 24-inch it should be 1.75 feet per 1 000, these quantities representing fairly the results of observation on clean pipes as determined by different experimenters. Making the comparison, a number of important facts are shown to exist, as for instance:

First.—That the average rate of hydraulic grade in the 36-inch pipe is 50 per cent. greater than it should be for clean pipe with the observed discharge.

Second.—That in the section of 24-inch wrought-iron pipe, between air valves 53 and 52, the rate of hydraulic gradient is 120 per cent. in excess of the normal with the observed discharge.

Third.—That in the section of 24-inch pipe, between air valves 52 and 35 (30 137 feet in length, of which 30 123 feet is cast-iron), the rate of grade is only slightly in excess of the normal.

Fourth.—That in the section from air valve 34 to 25, all 24-inch wrought-iron, the rate of grade is over 100 per cent. in excess of the normal for the observed discharge.

Fifth.—From all of the foregoing it is further concluded that the excessive resistance prevailing in this pipe-line at the present time is mostly confined to the wrought-iron section; the excess in the cast-iron being, generally speaking, only such as may be reasonably expected in pipe a long time in use.

In the absence of a thorough examination of the interior of the pipe, positive statements as to the exact cause of the trouble cannot be made, though it is not difficult with the information now at hand to form a clear idea of the location of the difficulty. An inspection of Table No. 1 shows the amount of variation in the rate of grade at different points. Occasionally the grade is substantially what it should be, as between air valves 67 and 66, where for a distance of 1 116 feet it is 0.32. At a number of places, however, the rate is much higher than the normal, and it has been positively stated that at these places work of an unsatisfactory character was done in the original construction. An inspection of the reservoir record indicates that the present flow has probably not been exceeded for several years, but the lack of completeness of the record renders it impossible to say what the changes have been. The present flow is, however, so much less than the reputed capacity of the con-

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duit, that, in the opinion of the author, the Rochester con not be taken as proving the correctness of the modern views value of C to be used in the formula $V = C\sqrt{RS}$ when pipe diameter and long lengths are under consideration.

NOTE.—The discussion on this paper and the next follows

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PLATE XII.

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521.

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ON THE MEASURES FOR RESTRICTING THE USE AND WASTE OF WATER, IN FORCE IN THE CITY OF ROCHESTER, N. Y.

By GEORGE W. RAFTER, M. Am. Soc. C. E.

WITH DISCUSSION.

In a paper "On the Hydraulics of the Hemlock Lake Conduit of the Rochester, N. Y., Water-Works," the author has indicated that the present population of Rochester, as per census of 1890, is 133 896, and that the daily delivery of water from Hemlock Lake is now only a little over 6 700 000 gallons, instead of 9 292 800, as originally published. Ever since 1888 the question of constructing additional works has been actively discussed by the municipal authorities; but a feeling of distrust on the part of many citizens as to the immediate necessity for additional works has thus far engendered an opposition sufficiently strong to prevent, until recently, the project assuming tangible form; and it has therefore become necessary to make the most economical use of the present limited supply. The measures adopted last year for curtailing waste and preventing unnecessary use will be briefly described.

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Referring to the accompanying skeleton map of the pipe distribution system (Plate XIV), it will be seen from the elevations there given that the city is chiefly situated in a nearly level plain, what little slope there is being from the south towards the north. The total area is something like 16 square miles, and the proportion of this covered by the distribution system is also shown on the said map. All of this area, except about 1.5 square miles in the business portion, which is protected by the Holly system, depends upon the Hemlock Lake supply not only for water for domestic and manufacturing use, but for fire protection by direct pressure from the hydrants as well; and any measures for restricting use and decreasing waste must, perforce of necessity, be designed with reference to fire protection as a controlling factor.

In July and August, 1888, both of the reservoirs were so nearly depleted as to leave only one or two days' storage on hand, and the situation was nearly as alarming in the summer of 1889. Fortunately no accident occurred either year to interrupt the flow through the single conduit from Hemlock Lake, and the city has thus far been delivered from the discomfort of a water famine. In the spring of 1890, the probability of a recurrence of the deficiency of the two previous hot seasons led to a large amount of discussion as to methods of restriction, and some of the municipal authorities took ground to the effect that the conditions were such as to render it impossible to avoid a water famine during the hot weather of that year. As is usual in such cases many suggestions were made, but none were sufficiently comprehensive to meet the condition of compelling a decrease in the use and waste of water, sufficient to bring the daily consumption within the limit of the daily supply and at the same time furnish fire protection. Some of these suggestions were interesting by reason of the lack of appreciation of the conditions which they presented, and one of them may be noticed as an example of this kind. The proposition in question was to control the delivery to the city by partially closing the outlet gates at Mount Hope Reservoir, thereby allowing the large supply mains leading down the hill to run partly full. Inspection of the map will show that the general level of the city is something over 100 feet below Mount Hope, and is reached in a horizontal distance of, say, half a mile, and the effect of adopting this suggestion will be understood after a brief description of the fire system in use. The reservoir is connected with the fire-alarm telegraph, and on receiving an alarm the gate-keepers, who are constantly on duty, by change of

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PLATE XIV

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gates at Mount Hope, immediately put the city distribution under the additional pressure due to Rush Reservoir. For this purpose Mount Hope is cut out for the time being and the distribution system fed direct from Rush instead. The plan for restriction that the author is now discussing also proposed to decrease the use and waste by so reducing the supply at night as to allow the supply mains to run nearly empty, thereby allowing the distribution system to become partly empty.

In order to test the rationality of this view a computation of the total contents of the distribution system was made, and it was found to be in the vicinity of 2 200 000 gallons, and by way of further testing the matter it was then assumed that a fire alarm might come in when the distribution system lacked 500 000 gallons of being full. Two 16-inch mains lead from Mount Hope to the city, and 5 000 gallons per minute through each of these was considered to be the safe limit, or 10 000 gallons per minute through both. Under the assumed conditions it would require fifty minutes to fill the distribution system and restore fire pressure, during which time the average fire would, of course, burn itself out. Even if we assume the distribution system as only lacking 200 000 gallons of being full, and the rate at which water might be fed to the city as somewhat greater than indicated in the foregoing, this method of reduction is still shown to be impracticable at Rochester, where the integrity of the fire protection imperatively demands an immediate response to every alarm. Probably the original hint for this plan was obtained from practice in the lower part of the City of New York, where substantially the thing proposed for Rochester was in successful use for three or four years previous to the introduction of an additional supply by the new Croton Aqueduct. The conditions in New York are not the same as in Rochester. No dependence at all is placed upon direct gravity pressure from the street mains for the suppression of fires. Steamers are used in every case, and presumably in sufficient number to control the worst fires, the street mains furnishing merely a handy reservoir system from which the steamers take suction. In Rochester, on the contrary, steamers are scarcely used at all. Only four are in commission for the whole city, and but two of these go to an ordinary fire. For several years after the completion of the Hemlock Lake supply their use was entirely discontinued, and the author is under the impression that a number owned by the city were sold to other towns. The increased use of water, however, has naturally decreased the effective pressure, and their use has again been

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resorted to as an auxiliary to direct gravity pressure from the Hemlock Lake system, outside the Holly district.

As warm weather came on in the summer of 1890 the use of water in the city gradually increased until the daily use was from 1 000 000 to 2 000 000 gallons in excess of the supply, and the storage was accordingly depleted from day to day by that amount. The alarm occasioned by this state of affairs at the beginning of summer has been referred to in the accompanying paper. Some of the municipal authorities still persisted in the view that there was absolutely no way of averting a water famine, but finally the Executive Board directed the author to devise methods of restricting the use and waste, as also referred to in the preceding paper. A study of the conditions indicated that a reduction of pressure might be made by partially closing gates on the main feeding lines in the general level of the city, without involving the objectionable features of the plan just discussed. Pressure-reducing stations were accordingly located at the points shown on the map—those at the junctions of (1) South avenue and Caroline street, and (2) Flint and Mansion streets being both on the main 16-inch feeders to the east side and to the west side respectively. By closing a few gates on small cross mains the whole supply was passed through the gates at these two points. The stations were furnished with fire gongs and telephones, each connected with its own proper system. A pressure gauge attached to the main at that side of the gate on which the pressure was to be reduced, completed the outfit of the stations. A few pressure gauges were also located in private houses at critical points in the city. The station at the junction of Mount Hope avenue and Linden street was installed as a precautionary measure purely, the only duty of the attendant there being to open gates on the cross lines in case of an alarm of fire from certain boxes in that vicinity. The station at the junction of Strong and Seward streets on the 16-inch feeding main was for a different purpose. By trial it was found that a very moderate reduction of pressure at Flint and Mansion streets left relatively high ground in the vicinity of Magnolia and Mansion streets without water, which was remedied by the location of the secondary station on the 16-inch main at Strong and Seward streets. The closing of the gate at this secondary station deflected the flow of water from the main feeder, through the small cross mains leading to the high ground, and at the same time assisted in keeping the total flow within the desired limit.

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By such a system it became very easy for a person located at a central point and provided with a telephone in connection with the pressure-reducing stations, to so manage the pressure in the different parts of the city as to just give a flow in the second stories of houses at the higher points. This meant a general lowering of pressure of from 10 to 20 pounds, and the immediate effect of this and the other restrictive measures inaugurated was to reduce the use and waste of water something like 1 800 000 gallons a day. The draft on the storage ceased at once, and in sixty days both reservoirs were full, and there is no good reason why, with economical management, they cannot be kept full, and aside from street and lawn sprinkling the necessary legitimate uses of water be supplied for two or three years, during which time the additional supply construction may be completed.

In the way of additional restrictive measures other than reduction of pressure in the manner indicated, a systematic house-to-house inspection was inaugurated, and all horse troughs throughout the city were fitted with automatic attachments; by this latter expedient a saving of something like 160 000 gallons a day was produced. The summer season of 1890 was one of frequent rain-fall, and the absence of water for lawn sprinkling was not felt. A large portion of the street sprinkling in Rochester is done with water from the Holly system, and for the last year the legitimate use of water from the Hemlock system for sprinkling, had it been allowed, would not have exceeded 800 000 gallons a day. The effect of the pressure reduction system was, therefore, a net saving of at least 800 000 gallons a day. How this saving was effected may be understood by considering that there are about 25 000 services in the city, of which 2 850 are metered. The leakage is an unknown quantity, but in a distribution system comprising over 200 miles of pipe, it may be safely taken as an appreciable one; and by way of illustrating the matter the author ventures to cite the fact that a sixty-fourth of an inch opening under 100 feet head will discharge something like 50 gallons in twenty-four hours, while the same opening under 50 feet head discharges only about 35 gallons in twenty-four hours. With services under a heavy pressure the tendency is to consume for purely domestic and toilet uses more water than is really necessary, and especially is this true when self-closing attachments are not used. This tendency is, of course, corrected by decreased pressure.

During the period of about sixty days from the middle of July to

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the middle of September, 1890, the use of water from the Hemlock Lake system was an average of say 6 000 000 gallons a day, amounting for a population of 133 896—the greater proportion of which are fully served—to 44.8 gallons per day each. During the months of July, August and September of the same year, the use from the Holly system averaged 2 000 000 gallons a day, or 15.0 gallons per head of the population, making the total use of water in the city during these hot months an average of 59.8 gallons per head per day. In this connection, it is but fair to again state that the use of water for street and lawn sprinkling by hose was prohibited during the period in question.

It is unnecessary, in a paper before this Society, to amplify as to the importance of a definite knowledge of the flow of a supply conduit, such as may be obtained from frequent and approximately exact gaugings. A failure to obtain this information has led (1) to cumbering the annual reports with questionable statistics; (2) to a long-continued controversy as to the necessity of an immediate additional supply, in which the misleading of the public as to the real facts of the case may be considered an injury to the municipality—a controversy which, however, terminated as soon as the significance of the gaugings became fully understood; (3) to a general movement in favor of metering, which, if carried out under existing conditions, would have aggravated a situation already sufficiently unsatisfactory.

The author would state that the work detailed in this and the former paper has resulted in a marked improvement in a number of directions of considerable financial value.

DISCUSSION.

E. KUICHLING, M. Am. Soc. C. E.—In reviewing Mr. Rafter's papers, one cannot fail to notice the brevity of the description of the data upon which highly important conclusions are founded, and inasmuch as these conclusions are at variance with the results of experience elsewhere, it becomes allowable to question the premises upon which the said conclusions were reached, in the hope that more light will be cast upon details that have been omitted in this paper.

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During the past year the original field notes of the piezometer and pressure gauge observations on the conduit mentioned, as recorded by Mr. Rafter's assistants, have been in my custody, and a careful study of them does not warrant me in accepting Mr. Rafter's demonstration of his propositions as unqualifiedly correct or satisfactory for the following reasons:

First.—The observations given by Mr. Rafter in Table No. 1 do not appear to have been simultaneous, or even taken on the same day. From the well-house to air valve 70, they refer to July 30th and 31st; from air valve 69 to 67 inclusive, to August 2d; at air valve 66, to August 4th; from air valve 64 to 63, to August 1st; at air valve 61 and 60, to August 4th; from air valve 59 to 57, inclusive, to August 1st; at air valve 56, to the average between readings taken on July 31st and August 5th; from air valve 55 to 47, inclusive, to August 4th; from air valve 46 to 37, inclusive, to August 5th; the first of the two bracketed observations at air valve 36 refers to August 5th, while the second refers to August 16th; and all the remaining observations refer to August 16th. At air valves 40 and 41, moreover, only one or two widely different readings of the pressure gauges at each place on the same day are given. It is also omitted to mention that during the period from July 30th to August 16th, the water surface in the reservoir at the end of the conduit rose 3.48 feet, while in the well at the beginning of the conduit there was a fall of 0.15 feet during the same time. All of the available data relating to these observations have been compiled in the appended Table A.

Second.—It is not stated that appreciable fluctuations occurred in the level of the water in the piezometers, or open stand-pipes, attached to air valves 91 to 47, inclusive, nor that during the days on which the observations were made, a number of leaky joints on the 36-inch pipe were being repaired, thereby altering the hydraulic conditions somewhat, even though the leakage may have been relatively small. To indicate the magnitude of these fluctuations, I would mention that repeated observations at air valve 53 have been made by me during the past twelve months at times when the entire conduit was reported to be free from appreciable leakage and no repairs whatever were being made thereon, with the result of finding irregular periodic differences of from 0.05 to 0.07 feet, when the conduit was practically free from air at the numerous summits; whereas, when the observations were made without having previously opened the air valves along the whole line, the fluctuations were found to be somewhat greater, not only at air valve 53, but also at a number of other places where the normal hydraulic grade does not rise above the top of the stand-pipe attached to the valve. On the other hand, among the data left by Mr. Rafter's assistants in the office, there are records of relatively great and sudden fluctuations on the same days at air valves 55, 56 and 57, of which no explanation is given. From

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my own experience, I am inclined to attribute these sudden changes of level in the stand-pipes to the movement or escape of masses of air which had been allowed to accumulate at the summits, or to gain admission into the conduit through those air valves on the 36-inch pipe which also serve as vacuum valves, when the discharge of said pipe is temporarily increased by opening one of the numerous blow-off valves for any purpose. This view is confirmed by the statements of the keeper of the reservoir, that during the summer of 1890, and for some time previously, he frequently saw large bubbles of air escape from the end of the submerged inlet pipe or conduit, and that such discharge was often accompanied by a loud rumbling sound. It therefore follows that without proof of the permanence of the hydraulic conditions of the conduit during the period from July 30th to August 16th, 1890, the conclusions stated by Mr. Rafter cannot be accepted as scientifically demonstrated facts.

Third.—In referring to the observations with pressure gauges, made for ascertaining the elevation of the hydraulic gradient at points on the 24-inch conduit where the application of piezometers is impracticable, it is not stated whether the same gauges were used throughout, nor how such gauges were rated or standardized. Every one who has had occasion to make use of such instruments has doubtless experienced some difficulty in ascertaining their accuracy, especially when small differences are to be measured at various pressures, as in this case. From the field notes it may be inferred that up to August 6th, a pair of the Utica Steam Gauge Company's diaphragm gauges, Nos. 77 349 and 77 805, were used, and that for the observations taken on August 15th and 16th a pair of Schäffer & Budenberg gauges, Nos. 1 076 544 and 1 076 545, were employed. In a remark in said field notes, under date of August 5th, 1890, it is mentioned that one of these gauges, probably No. 77 805, was "bad," and that "No. 77 805 gives 1 pound too much," although no standard of comparison is indicated, except the other gauge of the pair. The only record of any test of the gauges refers to Nos. 1 076 544 and 1 076 545, and occurs at the end of the observations in the field book of one of Mr. Rafter's assistants, but as this record bears no date, it is uncertain whether the test was made before or after the field work. On inquiry from an employee who remembered the circumstance, it was learned that said two gauges were tested by means of a force pump in comparison with a "test gauge" made by the Utica Steam Gauge Company, and owned by the Woodbury Engine Company of this city; also that such tests were made before the gauges were applied to the air valves, and that no test was made after they had been so used. Furthermore, it is stated by Mr. W. J. Creelman, the mechanical engineer of said engine company, that while the "test gauge" mentioned had originally been carefully standardized by its makers, yet it had not been compared with an accurate standard for a long time prior to July, 1890, and hence that its accuracy

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at the time of the conduit observations was unknown. Following is the record of the test of said gauges as taken from the field-book:

Woodbury (Engine Company's Test Gauge).	(Schäffer & Budenberg) No. 1 076 545.	(Schäffer & Budenberg) No. 1 076 544.
31.	30	30.5
47.	45	45.5
62.	60	60.5
76.	75	75.25
90.	90	90.
104.	105	105.
119.	120	120.
37.5	35	35.5
52.	50	50.
66.	65	65.25
84.	80	80.
94.75	95	95.
109.	110	110.
118.	120	120.
52.25	50	50.
32.5	30	30.5

. It will be observed that while the two Schäffer & Budenberg gauges show only slight differences, yet they vary materially from the "test gauge" with which they were compared; and in view of these relatively large differences, it is obvious that some measures should have been taken to ascertain the true ratings of the two gauges used in the field before deciding that the friction in the 24-inch wrought iron pipe is so much greater than in the cast iron pipe, since possible errors of from 2 to 4 pounds per square inch will make a very marked difference in the deduced position of the hydraulic gradient.

In connection with this subject of the accuracy of spring or diaphragm gauges, it may be of interest to mention that the Schäffer & Budenberg gauge No. 1 076 544 was tested a few months ago with a measured head of 10 feet of water, and the pointer barely moved from the zero pin, the indication being about one-half pound per square inch pressure for this head. The writer has also caused several of the Utica Steam Gauge Company's instruments to be tested recently under accurately measured heads of water, ranging from 5 feet to 205 feet, and has found errors in the dial graduations of nearly 2½ pounds, sometimes too much and sometimes too little. Temperature is likewise a factor in determining the pressure with such a gauge. In operating with an aneroid barometer, great differences are noticed between the readings of the instrument in the shade and in the sun at precisely the same spot, and when a check instrument fails to show any difference whatever in the atmospheric pressure; and since a diaphragm pressure gauge is constructed on the same principle as the aneroid, it follows that it may also be subject to variations of indication of the same pressure under

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different temperatures. To obtain the proof of this assertion, the following experiment was made by the writer a few days ago.

Gauge No. 77 348 of the Utica Steam Gauge Company was attached to a short iron pipe bent twice at right angles, with two vertical and one horizontal sections. The other end of said pipe was connected with a long rubber hose suspended from a high window by means of a rope and a steel tape, and water was introduced below at the junction of the hose and the iron pipe by means of a force pump, in order to drive out any entrained air. A series of readings at different measured heads were taken with the apparatus at the prevailing temperature of the air (62 degrees Fahr.) and the water (52 degrees Fahr.), whereupon the water in the short horizontal section of the iron pipe was heated by means of an alcohol lamp to a temperature of 104 degrees Fahr. without imparting an appreciable quantity of heat to the mass of cold water in the rubber hose. After this temperature had been maintained for about one-half hour, in order that the air which had been compressed in the gauge might also become warmed, as well as the diaphragm box, another set of readings at exactly the same heads as before was taken. The results thus obtained were as follows:

Measure Head of Cold Water above Center of Gauge, in Feet.	Corresponding Correct Pressure in pounds per square inch.	Pressure indicated by Gauge when cold.	Pressure indicated by Gauge when heated to 104 degrees Fahr.	Temperature of Water in the Hose.	Temperature of Air in Shade.
46.6	20.14	19.75	19.25	52 deg. Fahr.	62 deg. Fahr.
31.6	13.69	13.00	14.00	"	"
16.6	7.19	7.50	7.00	"	"

The foregoing table clearly shows the effect of changing the temperature of the gauge, or the compressed air within the diaphragm-box thereof, and hence also proves the proposition that metallic gauges are not reliable as a means of determining small differences of pressure, unless carefully rated and adjusted for variations of temperature.

Fourth.—In Mr. Rafter's paper, it has been assumed that the nominal diameters of the conduit are also the actual diameters. This assumption is hardly justifiable when dealing with the losses of head in different portions of the line. To ascertain the facts in the case, the writer has recently caused a number of the pipes which were left over after the completion of the works to be carefully callipered, with the following results: The heaviest class of the 24-inch cast iron pipe was found to be 23.97 inches in diameter, while the lightest class was 24.84 inches in diameter; of the two intermediate classes, no pieces were found which could be identified. In the 24-inch wrought iron pipe, the mean diameter of the accessible sections forming "inside" courses or sheets

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PLATE XIII

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was found to be 24.09 inches. It is, however, hardly probable that any close uniformity of diameter was maintained in the manufacture of either the cast or the wrought iron pipes, since inequalities in the formation and shrinkage of the cores of the former, and appreciable variations in length of sheet or plate in the latter, are both practically unavoidable.

Another omission in the paper is in reference to the means used in changing the direction of the pipe, both in grade and alignment, during the construction. As a matter of fact, the records left in the office indicate that in the 36-inch pipe there are at least sixty abrupt angles or elbows with deflections ranging from 5 degrees to 40 degrees, and that in the case of the 24-inch wrought iron pipe a large number of abrupt angles ranging from 8 degrees to 20 degrees were made. For the cast iron pipe, on the other hand, a variety of cast iron curves of long radius were provided at all places where the deflection was too great to be made in the hubs or bells. In the wrought iron pipe, some of these angles were made with suitable cast iron bevel or angle hubs, while the larger ones were made by cutting one or more sheets to the required angle. It is also a fact that the cast iron pipe was used where the route was smoothest, as will be seen by reference to the profile exhibited, and hence in estimating the friction loss in the wrought iron sections, due allowance for loss of head from abrupt changes of direction should be made. The question accordingly arises how great such losses of head actually are in the case under consideration, and to this the writer is at present unable to give any answer, since he is unacquainted with any experiments of this kind with pipes of the magnitude here presented. It may also be remarked that a small allowance for head due to velocity should be made in considering the hydraulic grade between air valves 52 and 53.

To further illustrate the uncertainty of the data derived from the pressure gauge records, attention is invited both to the accompanying Table A, and to the profile or diagram, Plate XIII, giving a graphical representation of this table. A striking example is afforded by the records of the observations at air valves 40 and 41 on August 5th, where, in a length of 655 feet of cast iron pipe, an ascending hydraulic grade of 2.18 per thousand is found in the forenoon, while in the afternoon of the same day the grade is found to be descending at the rate of 0.98 per thousand; but on August 16th, the same grade is found to be descending at the rate of 2.15 per thousand. Another example is given by the records at air valves 30 and 31, which are on the 24-inch wrought iron pipe and 1 143 feet apart. Here on August 5th, a grade of 0.11 per thousand is deduced, while on the 15th the computation makes the grade 2.94, and on the 16th it becomes 3.52 per thousand. Still another case is presented at air valves 25 and 26, which are 599 feet apart on the 24-inch wrought iron pipe, and where on August 15th, we find a hydraulic grade of 0.29

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TABLE A.—SHOWING ELEVATIONS OF HYDRAULIC GRADE AT VARIOUS POINTS ON CONDUIT AS DEDUCED FROM OPEN STAND-PIPES AND PRESSURE GAUGES.

Number of Air Valve.	Diameter of Pipe.	Continuous distance from Gatehouse Well.	Dates 1890.											
			July 25.	July 26.	July 27.	July 30.	July 31.	Aug. 1.	Aug. 2.	Aug. 4.	Aug. 5.	Aug. 6.	Aug. 15.	Aug. 16.
			Elevation of Water in Well of Gatehouse at Hemlock Lake.											
	36 inches.	0.0	387.65	387.65	387.65	387.48	387.49	387.49	387.48	387.45	387.44	387.60	387.35	387.33
			Elevation of Water in Open Stand-pipe at Air Valves.											
91	36 "	9 198	383.01	.....	.....	383.49	.....	.....	.....	.....	.....	.....	.....	.....
90	36 "	9 821	382.76	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....
89	36 "	10 612	382.43	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....
88	36 "	11 430	382.14	.....	.....	.....	382.75	.....	.....	.....	.....	.....	.....	.....
87	36 "	12 006	381.82	.....	.....	.....	382.43	.....	.....	.....	.....	.....	.....	.....
86	36 "	12 820	381.65	381.49	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....
85	36 "	13 298	.....	381.38 (a)	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....
84	36 "	13 963	381.71	381.11 (b)	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....
83	36 "	15 008	380.74	380.58 (c)	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....
82	36 "	16 145	.....	379.99	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....
81	36 "	16 533	.....	379.78	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....
80	36 "	17 120	.....	379.50	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....
79	36 "	18 415	.....	378.88	.....	379.49	.....	.....	.....	.....	.....	.....	.....	.....
78	36 "	19 132	.....	.....	.....	379.21	.....	.....	.....	.....	.....	.....	.....	.....
77	36 "	20 092	.....	.....	.....	378.82	.....	.....	.....	.....	.....	.....	.....	.....
76	36 "	22 037	.....	.....	.....	377.98	.....	.....	.....	.....	.....	.....	.....	.....
75	36 "	23 087	.....	.....	377.12	377.51	377.52	.....	.....	.....	.....	.....	.....	.....
74	36 "	25 875	.....	.....	.....	.....	375.95	.....	.....	.....	.....	.....	.....	.....
73	36 "	26 606	.....	.....	.....	.....	375.75	.....	.....	.....	.....	.....	.....	.....
72	36 "	27 817	.....	.....	.....	.....	375.25	.....	.....	.....	.....	.....	.....	.....
71	36 "	28 119	.....	.....	.....	.....	375.13	.....	.....	.....	.....	.....	.....	.....
70	36 "	29 251	.....	.....	.....	.....	374.64	374.67	.....	.....	.....	.....	.....	.....
69	36 "	31 317	.....	.....	.....	.....	.....	.....	373.71	.....	.....	.....	.....	.....

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68	36	"	32 345							373.18					
67	36	"	33 252							372.65					
66	36	"	34 368								372.29				
65	36	"	35 978												
64	36	"	38 088						370.88						
63	36	"	39 106						370.39						
62	36	"	40 626						369.65						
61	36	"	42 210			368.59			369.33		369.00d				
60	36	"	42 862						368.33		368.36				
59	36	"	44 134						367.62						
58	36	"	46 370			366.12			366.61						
57	36	"	47 070			365.74			366.28				366.45	366.69c	366.71c
56	36	"	48 615			365.08		365.44			365.49e	365 62	365.70	366.03c	366.99c
55	36	"	48 909	364.59f	364.57 g	364.77	365.09h	365.25	365.21i	365.31	365.33	365.37	365.58j	365.89k	365.89l
54	36	"	49 703			364.59					365.08				
53	36	"	50 820	363.97		364.11					364.65				
	24	"	50 828	Beginning of 24-inch wrought iron pipe.											
52	24	"	52 720			356.79					357.37				
	24	"	52 722	End of wrought iron and beginning of cast iron 24-inch pipe.											
51	24	"	53 658			354.63					355.18				
50	24	"	54 502			352.73					353.35				
49	24	"	56 209								349.57				

TABLE A—(*Continued*).

Number of Air Valve.	Diameter of Pipe.	Continuous distance from Gate-house Well.	Dates, 1890. July 25.	July 26.	July 27.	July 30.	July 31.	Aug. 1.	Aug. 2.	Aug. 4.	Aug. 5.	Aug. 6.	Aug. 15.	Aug. 16.
			Elevation of Water in Open Stand-pipe at Air Valves.								Elevation of Hydraulic Grade computed from Pressure Gauge Readings.			
48	24 inches.	56 946	.....	.....	.....	.....	.....	.....	.....	347.94	.....	.....	.....	.....
47	24 "	59 141	.....	.....	.....	.....	.....	.....	.....	348.55	.....	.....	.....	.....
46	24 "	61 036	.....	.....	.....	.....	.....	.....	.....	.....	338.40	.....	.....	.....
45	24 "	61 514	.....	.....	.....	.....	.....	.....	.....	.....	337.78	.....	.....	.....
44	24 "	66 038	.....	.....	.....	.....	.....	.....	.....	.....	327.35	.....	.....	.....
43	24 "	66 235	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....
42	24 "	66 710	.....	.....	.....	.....	.....	.....	.....	.....	323.72	.....	.....	.....
41	24 "	69 323	.....	.....	.....	.....	.....	.....	.....	.....	{318.87 / 320.03}	.....	.....	325.05
40	24 "	69 978	.....	.....	.....	.....	.....	.....	.....	.....	{321.05 / 319.05}	.....	.....	322.90
39	24 "	71 635	.....	.....	.....	.....	.....	.....	.....	.....	316.05	314.84	319.94	319.90
38	24 "	72 616	.....	.....	.....	.....	.....	.....	.....	.....	311.81	311.81	318.53	318.56
37	24 "	74 180	.....	.....	.....	.....	.....	.....	.....	.....	309.10	.....	317.32	314.51
36	24 "	78 763	.....	.....	.....	.....	.....	.....	.....	.....	300.51	.....	305.18	305.20
35	24 "	82 857	.....	.....	.....	.....	.....	.....	.....	.....	294.93	291.47	298.15*e*	298.47
	24 "	83 238	End of cast iron and beginning of wrought iron on 24-inch pipe.											
34	24 "	84 913	.....	.....	.....	.....	.....	.....	.....	.....	286.36	283.97	291.78	292.38
33	24 "	85 623	.....	.....	.....	.....	.....	.....	.....	.....	.....	281.35	287.58	284.18
32	24 "	86 133	.....	.....	.....	.....	.....	.....	.....	.....	280.16	275.94	283.85	281.45
31	24 "	87 097	.....	.....	.....	.....	.....	.....	.....	.....	274.05	274.80	282.50	283.10
30	24 "	88 240	.....	.....	.....	.....	.....	.....	.....	.....	273.94	.....	279.56	279.58
29	24 "	89 791	.....	.....	.....	.....	.....	.....	.....	.....	268.27	.....	272.31	272.91
28	24 "	92 237	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	264.52	265.69
27	24 "	92 842	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	263.05	263.65
26	24 "	94 855	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	254.89	256.07
25	24 "	95 454	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	254.60	254.62
	24 "	96 796	End of wrought iron and beginning of cast iron 24-inch pipe.											
24	24 "	98 404	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	244.71	245.31
23	24 "	100 267	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	239.46	241.79
			Elevation of Water Surface in Rush Reservoir.											
	24 inches.	102 277	233.25	233.47	233.78	234.64	234.85	235.03	235.34	235.54	235.62	235.76	237.99	238.12

a = average of 5 observations on date.
b = " 6 " "
c = " 4 " "
d = " 7 " "
e = " 2 " "
f = " 11 " "

g = average of 33 observations on date.
h = " 23 " "
i = " 19 " "
j = " 18 " "
k = " 13 " "

Note.—In computing the height of the equivalent water column from the pressure gauge readings, it was assumed that one pound pressure corre-

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per thousand, with the particular remark that the "gauges were tried several times" on that day, while on the following day the computed grade is 1.45 per thousand. It will be noticed that in some of these instances the computed grade in the wrought iron pipe is much less than the average grade in the cast iron sections, thus proving either the reverse of Mr. Rafter's proposition, or else that local obstructions occur in the other sections of the wrought iron pipe. Unfortunately, however, a rigid cross-examination of some of the foremen and mechanics who were employed in the construction of the works, failed to elicit much definite information as to the location of any obstruction, except that at a certain place in the cast iron pipe, where it was alleged that at least 700 pounds of molten lead had been poured into the pipe through a defectively yarned sleeve joint. Pressure gauges appear to have been applied at distances of 162 feet on each side of this particular spot, but the results were apparently unsatisfactory, since the resulting hydraulic grade was found to be only 1.08 per thousand, or less than one-half of the average. It is believed that the foregoing illustrations will suffice to point out some of the uncertainties which are attendant upon the use of ordinary pressure gauges as instruments of precision.

It is also proper to remark in this connection that the writer has no intention of disputing the correctness of the various observations and data presented by Mr. Rafter, nor of underrating his work, but he simply desires the presentation of all the facts in the case before judgment is passed. As the matter now stands, it may be conceded on broad lines that the friction in smooth and new cast iron pipe is somewhat less than in similar riveted wrought iron pipe, owing to the obstruction caused by rivet heads and the frequent alterations of diameter in passing from the inside to the outside courses or sections; but it scarcely seems probable that the difference is as great as claimed by Mr. Rafter, if due allowance for resistances caused by abrupt changes in direction is made. On this latter point, therefore, more light is needed, and meanwhile it may be considered that the enormous differences in friction urged by Mr. Rafter have not been conclusively demonstrated.

Concerning the existence of any appreciable obstruction in the pipes at any point, it may be mentioned that no satisfactory evidence thereof has yet been manifested by any test thus far applied. At the point where the diameter of the conduit changes from 3 feet to 2 feet, which is about midway between the lake and Rush Reservoir, a long steel probe was inserted through a specially designed tap, and the interior of the pipe for a length of about 6 feet found to be absolutely free. A similar exploration with like result was also made at the tapering casting at Rush Reservoir where the conduit divides into two branches, one leading into the reservoir and the other communicating with the pipe running to Mt. Hope Reservoir. It was thought that at these two points any loose plank or timbers left in the pipe during construction might

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possibly have become lodged, but the examinations have proved otherwise. All of the blow-offs at the principal depressions in the conduit lines were likewise opened with the expectation of removing any accumulations of sand or mud; the discharge was, however, practically clear water in every instance, and hence no evidence of obstruction from such a cause was gained.

Owing to the inexpediency of emptying the conduit, no knowledge of the condition of its inner surface is now available, except what was obtained from the two probings above mentioned, in which no evidence of tuberculation by rust, mineral incrustation, sediment or vegetable growth was found. The only clue thus far obtained consists in the discovery of an extensive growth of spongilla on the interior surface of the 24-inch cast iron effluent pipe from Rush Reservoir, on each side of the stop gate in the gatehouse. This gate has long been out of order, and it was repaired on September 19th, 1890, the mouth of the pipe in the screening well of the reservoir being temporarily closed for this purpose by a plank shutter, whereupon the dome of the gate could be unbolted and removed. An opportunity was thus afforded to examine the interior of the pipe for a short distance on each side of the opening, and it was found that the top and sides of the pipe as far as arm could reach, as well as the inside of the gate castings, were entirely covered with the growth mentioned. A significant circumstance was that the pitch coating of the iron pipe appeared to peel off along with the foreign matter and remained adhering to the roots of the latter. Considerable corrosion was also noticed in places on the inner surface of the gate castings, while the brass facings or seats were covered with a thin film of calcareous or silicious matter which was easily scraped off with chisels.

The occurrence of such growths in cast iron water mains is not unusual. In a paper on the subject which was read before the American Society of Civil Engineers on October 15th, 1884, by Mr. Desmond FitzGerald, Resident Engineer on the Boston Water Works, the author states that "he has seen large mains, under a pressure of 100 feet, where the entire surface, as far as examined, was filled with masses of sponge closely packed between and around the tubercles (or accretions of rust). He has also seen them in all stages of growth. Within a few days a break in a 48-inch main gave an opportunity to examine the sponge in company with Professor Hyatt, who recognized the young sponge as *Spongilla lacustris*, variety *Flexis pina*. Flushing will not remove this growth. Some form of scraper or wire brush is necessary." Similar experiences have also been recorded by engineers in other cities, and it hardly seems necessary to enlarge further upon the subject.

E. Sherman Gould, M. Am. Soc. C. E.—I regret very much not being able to be present at the reading of these interesting papers, and to take part in the oral discussion of the same. It would be very desirable to know what kind of wrought iron pipe was used, for the experiments

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seem to show that the flow is much less free in the wrought iron than in the cast iron sections. The actual flow, as compared with that originally calculated, seems to be accounted for by the author in the summary at the end of the paper, at least in part, by the statement that unsatisfactory work was stated to have been done in some places where the worst results were obtained. What was the nature of this unsatisfactory work, and was the pipe line as a whole below the average standard of ordinarily good work? These questions are very important, because the tendency of hydraulic engineers seems to be to use formulas giving higher and higher rates of discharge, which, as it seems to me, in view of the many probable imperfections which are liable to exist in even well inspected work, is of very questionable wisdom.

Table No. 2 is exceedingly instructive. In that we see that some 51 000 feet of wrought iron 36-inch pipe is connected with 1 900 feet of wrought iron 24-inch pipe, and further down in the same line there are about 10 500 feet more of wrought iron 24-inch pipe. The hydraulic grade line, as determined by actual experiment with piezometric tubes and pressure gauges, gives 0.45 feet per thousand for the 36-inch pipe, 3.83 feet for the first section of 24-inch pipe and 3.58 for the second. These figures give us the means of checking their own correctness. For whatever quantity of water passed through the 36-inch pipe per second must have also passed through the 24-inch pipes in the same time. Now, the discharging capacities of pipes of different sizes are as the square roots of the fifth powers of the diameters. Therefore the discharging capacity of the 36-inch pipe is 2.76 times that of the 24-inch. Moreover, using Darcy's co-efficients for rough pipes, the ratio in favor of 36-inch pipe is $\frac{64}{62} = 1.032$. Therefore the total capacity of the 36-inch pipe is 2.85 times that of the smaller. As the same volume of water per second must pass through the two, the ratio of their respective losses of head must counterbalance the 2.85 factor of the large pipe. Now the discharging capacities of pipes are as the square root of their losses of head, consequently the loss of head of the 36-inch pipe being 0.45 per thousand, that of the 24-inch should be 3.755, in order that the ratio of their square roots may be $\frac{1}{2.85}$. But by actual measurement this loss of head was in one case 3.83, and in the other 3.58, the average of which is 3.70, which is a very close agreement with the result, 3.755, of calculation. We may therefore accept the measurements as being rather unusually accurate.

Another fact arrests our attention. The probable discharge of the system was predicted, from previous calculations, as 9 292 800 gallons per twenty-four hours, or 14.39 cubic feet per second. It is to be regretted that the formula used was not given. By actual recent measurement the present discharge was found to be 6 742 000 gallons per twenty-

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four hours, or 10.43 cubic feet per second. That is, the former elaborate calculation is for some reason nearly 38 per cent. in excess of the present reality.

Now let us see how a very rough and ready formula would have fitted the facts. Granting a considerable, but highly probable, degree of roughness, leakage or other modifying imperfections, a long pipe, 1 foot in diameter, with a grade of 1 foot in 1 000, will discharge 1 cubic foot of water in one second of time. This fact enables us to write the two simple formulas:

$$D = \sqrt[5]{\frac{Q^2}{H}} \quad \ldots\ldots (1)$$

$$Q = \sqrt{D^5 \times H} \quad \ldots\ldots (2)$$

in which:

D = diameter in feet.

Q = cubic feet per second.

H = loss of head per thousand.

Now taking the case of the 36-inch pipe, in which $H = 0.45$, we have, using equation (2): $Q = 10.46$, that is, a discharge of 10.46 cubic feet per second. But the actual discharge, by direct measurement, was 10.43 cubic feet per second; therefore the formula gives a practically identical result. As applied to the 24-inch pipes, the results are not so close, for the 3.83 grade gives 11.07 cubic feet per second, and 3.58 gives 10.70 cubic feet per second. These discrepancies, however, are in the right direction, for if the above formula gives a right result for a pipe of a certain diameter, it will give too large a result for a similar pipe of smaller diameter, and too small a one for a pipe of larger diameter.

To me, one of the most important features of these experiments is that they emphasize the fact that, in view of the many retarding influences at work inside of an ordinary pipe line, particularly after the lapse of years, during which the consumption is increasing, it is unwise to put faith in hair-splitting formulas which raise false hopes as to the capacity of the pipes.

It appears to me that the measures taken by the author for restricting waste were wise and intelligent. The distributing pipes of a large city, particularly when the circuits are connected, constitute a complicated arterial system in which the pressures at different points cannot be even approximately foretold. If it is desired to obtain an even distribution of water in such cases, the use of gates and gauges as described in the second paper seems to be inevitable.

RUDOLPH HERING, M. Am. Soc. C. E.—Mr. Rafter's first paper closes as follows: "In the opinion of the author the Rochester conduit cannot be taken as proving correctness of the modern views as to the value of c to be used in the formula $v = c\sqrt{RS}$ when pipes of large diameter and long lengths are under consideration."

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Shortly before concluding, Mr. Rafter assumes certain hydraulic elements as to gradients, etc., and compares the results obtained therefrom with the results of the gaugings, and draws certain conclusions, the fifth of which, as a summary, cannot be gainsaid. But he says further, "considerable doubt is thrown by the results of these tests upon the original determination of the daily flow of 9 292 800 gallons in twenty-four hours, and until a thorough history of the interior of the pipe is available, it must be considered an open question as to whether such flow has ever taken place."

It is my purpose to present a few calculations respecting this pipe line, based on other gaugings, to show that when the pipes were new, the discharge of 9 292 800 gallons must have been very nearly the true one, and that the information given by Mr. Rafter does not disprove the correctness of the modern views as to the value of *c*.

I shall use for the calculations the data given in Table No. 2, comprising the results of recent gaugings, and I shall assume that in the third and fourth sections the pipes are cast and wrought iron respectively, for their whole length. This assumption will cause an error in the final result, but a comparatively small one, that will not vitiate the general results, and simplify the calculations.

HYDRAULIC ELEMENTS, ROCHESTER CONDUIT.

Air valves.	Length in feet.	Diameter of pipe.	Slope per 1 000.	Mean velocity in feet per second.	Co-efficients.	
					c	*n*
Well house at lake to 53.....	50 819	36 inches, wrought.	0.45	1.47	80.4	.0168
53 to 52	1 901	24 " "	3.83	3.32	76.0	.0166
52 to 35	30 187	14 feet 24 " " / 30 123 " 24 " cast.	1.95	3.32	106.4	.0125
35 to 24	15 547	427 " 24 " " / 13 809 " 24 " wrought / 1 311 " 24 " cast.	3.43	3.32	80.2	.0158
24 to 25	10 541	24 " wrought.	3.58	3.32	78.5	.06

In the above table these data are partly repeated and I have added three columns in which there are given respectively, the mean velocity of the pipe, the co-efficient *c* in the Chezy formula and the co-efficient *n* in Kutter's formula. The latter represents the resistance mainly due to the roughness of the wetted perimeter, such as they exist in the pipe at the present time. It will be noticed that the values for wrought iron pipe in the different sections are 0.0168, 0.0166, 0.0158 and 0.0161 respectively, and that the value for the cast iron pipe is 0.0125.

The difference in the values of *n* referring to the different sections of wrought iron pipe agree well among each other. From other gaugings

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of cast and wrought iron pipes that have been in use for a number of years, this co-efficient of roughness (.0158 to .0168) indicates a considerable degree of tuberculation.

For instance, a main in Jersey City gauged by Mr. Bailey in 1867 (Fanning, "Treatise on Hydraulics"), measuring 29 715 feet in length and 20 inches in diameter, known to be heavily tuberculated, give a co-efficient of roughness of $n = .0166$. The Croton main gauged by Kirkwood in 1867 is 11.217 feet long and 3 feet in diameter, also known to be heavily tuberculated, gives a co-efficient of $n = .0168$.

The co-efficient of roughness in the cast iron section (.0125) indicates but a slight change from the co-efficient for new pipe. Gaugings of new cast iron pipe give values as follows: In Hamburg* a 20-inch pipe 3 514 feet long gives a co-efficient varying from .0112 to .0119, according to the velocity, which correspondingly varies from 0.70 to 2.50 feet per second.

At Hackensack, New Jersey,* a 20-inch pipe, 75 000 feet long, having a number of bends and angles, gives a co-efficient varying from 0.0110 to 0.0127, for velocities ranging from 2 to 3 feet per second.

At Philadelphia* a 30-inch pipe 20 200 feet long, with easy curves, gives a co-efficient varing from 0.0113 to 0.0128, as the velocities ranged inversely from 3.23 to 1.47 feet per second.

In the Sudbury* conduit a 4-foot pipe 1 747 feet long, with easy curves, gives a co-efficient varying from 0.0105 to 0.0109, for velocities varying from 2.6 to 6.2 feet per second.

The co-efficient of roughness for new pipe under the above conditions varies therefore from .0105 to .0128, while that found in the Rochester conduit is .0125. Mr. Rafter's fifth conclusion, that the excessive resistance is mostly confined to the wrought iron section, is therefore substantiated by the gaugings of other and similar conduits, and in a very satisfactory way, for pipes of large diameter and long lengths; also by means of the modern formula and views as to the value of c in the Chezy formula.

Let us now ascertain the results of these same views with reference to the probable discharge of the conduit when it was new. Darcy* gauged a new wrought iron pipe about 12 inches in diameter and 365 feet long with velocities of 1.3 and 3.9 feet per second, the co-efficient of roughness being respectively .0110 and .0098.

H. Smith, Jr.,* gauged similar pipes nearly 13 and 15 inches diameter and each about 700 feet long with velocities 4.4 and 4.6 feet per second, in which the co-efficient of roughness varied from .0109 to .0110. He likewise gives the results of a pipe 17 inches diameter and about 4 440 feet long, giving for a velocity of 20.1 feet per second a co-efficient of roughness equal to .0099. Finally he gives a pipe 25 inches diameter

*"Flow of Water," by Ganguillet and Kutter. Translated by Hering and Trautwine. John Wiley & Sons. 1891.

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and about 1 200 feet long and for a velocity of 12.6 feet per second a co-efficient of roughness equal to .0106.

Under the conditions as reported to have existed in the Rochester conduit when built, it will therefore appear safe to assume for them a co-efficient $n = .011$ as a probable maximum for wrought iron sections. For the cast iron section we are likewise justified from the gaugings previously quoted in assuming the co-efficient $n = .012$, as representing the probable maximum when the pipe was new. With these co-efficients let us now ascertain the discharge in a pipe line such as the Rochester conduit is described to have been.

We have, beginning at the lake:

(1) A 36-inch wrought iron pipe 50 819 feet long with a co-efficient of roughness of $n = .011$.

(2) A 24-inch wrought iron pipe 1 901 feet long with a co-efficient of $n = .011$.

(3) A 24-inch pipe, mostly cast iron, 30 137 feet long, with a co-efficient of $n = .012$.

(4) A 24-inch pipe, mostly wrought iron, 15 547 feet long, with a co-efficient of $n = .011$.

Let us designate the heads at the end of each of these four sections, respectively, to be h_1, h_2, h_3 and h_4. Let us further designate the mean velocity in the 36-inch pipe as v_1, and that in the 24-inch as v_2. It will be evident that the discharge in the 36-inch pipe and that in any of the sections of the 24-inch pipe must be equal. As the area of the 36-inch pipe is 7.07 square feet, and that of the 24-inch pipe 3.14 square feet, the discharge will therefore be equal to $7.07\ v_1$, and likewise equal to $3.14\ v_2$.

Let c_1, c_2, c_3, c_4, respectivley, represent the co-efficient for each of the four sections in the formula $v = c\sqrt{RS}$, in which v = mean velocity in pipe; R = mean radius of pipe; S = hydraulic slope.

If we substitute for S the head h divided by the length l and for $\sqrt{R}$ and l the respective numerical values, we have the following five equations:

$$(1) \quad 7.07 v_1 = 3.14 v_2.$$

$$(2) \quad v_1 = c_1 \times .866 \sqrt{\frac{h_1}{50\,819}}.$$

$$(3) \quad v_2 = c_2 \times .707 \sqrt{\frac{h_2}{1\,901}}.$$

$$(4) \quad v_2 = c_3 \times .707 \sqrt{\frac{h_3}{30\,137}}.$$

$$(5) \quad v_2 = c_4 \times .707 \sqrt{\frac{h_4}{15\,547}}.$$

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As the entire head from the lake to the reservoir is 143.8 feet, we can add a sixth equation:

$$(6) \quad h_1 + h_2 + h_3 + h_4 = 143.8 \text{ feet},$$

from which six equations the two mean velocities and the four heads can be computed, provided we substitute the numerical values of c. According to the Ganguillet and Kutter formula, we can neglect the effect of the variation of slope upon the co-efficient for the case in hand. Assuming this slope to be 0.411 per thousand for the 36-inch pipe, and 2.26 per thousand for the 24-inch pipe, we obtain from the formula:

$$c_1 = 131.7 \qquad c_3 = 112.3$$
$$c_2 = 125.0 \qquad c_4 = c_2 = 125.0.$$

Substituting these values in the above equations, substituting further, from equation 1, the value of v_1 expressed in terms of v_2, and reducing respectively to the values of h, we get:

$$(7) \quad h_1 = 0.770\, v_1^2 \qquad (9) \quad h_3 = 4.781\, v_1^2$$
$$(8) \quad h_2 = 0.243\, v_1^2 \qquad (10) \quad h_4 = 1.991\, v_1^2$$

Substituting these values in equation 6, we find the mean velocity in the 24-inch pipe, $v_2 = 4.30$ feet. From equation 1 we then get the mean velocity in the 36-inch pipe, $v_1 = 1.91$ feet, and from equations 7 to 10 the various heads:

$$h_1 = 14.2 \qquad h_3 = 88.3$$
$$h_2 = 4.5 \qquad h_4 = 36.8.$$

The discharge of the mains is, therefore,

$$7.07\, v_1 = 13.50 \text{ cubic feet per second, or,}$$
$$8\,725\,200 \text{ gallons in twenty-four hours.}$$

This quantity is not quite as great as that given originally, but as the assumption above made for the co-efficient n was rather large, in order to be on the safe side, I consider from the above independent data and above reasoning, the original quantity most likely to have been correct. As the recent tests were made in old pipe, evidently much tuberculated in the wrought iron sections, I therefore see no justification in the assertion of Mr. Rafter that by them "considerable doubt is thrown" upon the original determination of flow. It is also evident, notwithstanding the sentiment expressed in the last sentence of his paper, that the modern views as to the value of c have in the Rochester case rather been substantiated than otherwise.

JOHN THOMSON, M. Am. Soc. C. E.—Regarding Mr. Rafter's prefatory paper on the "Hydraulics" of the Rochester conduit, its evident object is to compare the past and the present maximum discharging capacity of the Rochester conduit and also to submit certain inferences as to the physical condition of its internal surface. The late Mr. L. L.

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Nichols, C. E., made the first gauging of the discharging capacity of the Rochester conduit, acting as an assistant engineer under J. Nelson Tubbs, M. Am. Soc. C. E., then the Chief Engineer of the line. I had the honor of Mr. Nichols' acquaintance, and firmly believe that he was thoroughly competent to accurately determine the discharging capacity of the line in the construction of which he himself had performed an important part.

But the details of Mr. Nichols' gaugings, as observed and recorded by himself, are elsewhere fully treated with proper credit, and I allude thereto with the view of adding concurrent testimony that the original measurements were properly made and the results obtained were within the requirements of practical accuracy in such cases.

Regarding the means employed by Mr. Rafter to ascertain the dynamic conditions of flow, there are a number of features of which it would be interesting to learn more of the details. Such, for instance, as to how the summits of the piezometric columns were indicated for instrumental reading. Then as to whether the several stand-pipes were read simultaneously; or, if not, what time elapsed between the readings. It will be obvious that much of the value of these observations will depend upon the manner in which these features were carried into effect.

Although I believe and admit that the practice here described by Mr. Rafter is usual, I yet have grave doubts as to the reliability of piezometers when attached directly to the air valves of a pipe line long in use. As a means of determining the present internal condition of any particular air valve such an attachment would appear to be most efficient; but unless it were known to an absolute certainty that their internal conditions were identical each with the other, then it has always appeared to me that to predicate the internal physical condition of a long pipe line from the flow past orifices of unknown contour is an unreliable deduction. I should say in a case of this kind, where it may be fairly well assumed in advance that the conduit is obstructed by incrustations upon the interior surface, that the inner ends of the piezometers might better be inserted into the main body of the conduit, the internal ends of all the tubes to be at the same distance from the exterior of the conduit, as in this wise practically identical dynamic conditions would obtain.

As to the determination of the hydraulic gradient of the 24-inch section by means of ordinary spring pressure gauges, I have examined Mr. Rafter's paper diligently for a reference to the means employed for standardizing the gauges; but although the manner of checking their reading by independent observers is expressly set forth (which would appear to be necessary, as they must have been read to fractions of a pound, judged by the tabular deductions), there is but this statement as to the gauges themselves: "The gauges were attached to each air valve at the same time, and allowed to remain for half an hour."

Now, as a glance at the profile will denote the considerable fluctua-

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tions in the total pressures required to be indicated by the gauges, this fact alone would demand that they should have had frequent and most accurate comparison with a reliable standard. If this were not done, then, in my opinion, but very little value may be given to this portion of the experiment. The fact that the gauges did or did not indicate the true conditions does not affect my present proposition; if they were not calibrated to every change of condition, then the result can only be accepted as hypothetical.

In this connection the admission of Mr. John R. Freeman, M. Am. Soc. C. E., in his discussion of my paper on "Proportional Water Meters," wherein he is an advocate of spring gauges for an analogous purpose, may not be inaptly quoted: "In any important work in either steam or water, all Bourdon gauges should be often tested. Mere comparison with a standard gauge is not fully satisfactory." And in this regard I presume to say that Mr. Freeman knows whereof he speaks; for although he has accomplished results in this direction approximating practical accuracy, and that, too, by scientific methods and approved apparatus, yet even these were open to adverse criticism.

That the Rochester conduit has undergone serious deterioration is not questioned, but as to the cause and location thereof, it is my contention that the present demonstration is not conclusive; that the case stands on the Scotch verdict, "Not proven."

The end sought by Mr. Rafter, as detailed in his second paper, was to increase the storage reserve of the reservoirs, and the exigencies of the case were such that almost any means would be tolerated to effect this end. The method evidently most prominently in mind for immediate application, both by citizens and experts, was to increase the time necessary to obtain a given quantity.

The manner here described of carrying this into effect seems to have been based upon the assumption that any diminution at or near the source of supply would cause the mains to "run partly full," with obvious results in the event of applying the direct pressure from Rush reservoir. The question here is, was the somewhat elaborate process applied by Mr. Rafter necessary? I am willing to go on the record as saying that, in my opinion, it was not; that if competent attendants had been placed at the discharging gates of Mount Hope reservoir, or better still at some lower portion of the hydraulic gradient beyond the principal distributing area, in telephonic communication with the city fire alarm, the valves carefully pinched down to a point where the draft there through became slightly less than the supply, and continually so maintained, then the same results would have been reached. By this method the minimum pressure throughout the city would have been constantly increased until merged into the maximum, while yet at any instant the full pressure might have been with safety applied for fire purposes. But what was the consequence of this "restriction" in actual

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fact? Simply as already intimated, that each and every water-taker in a city of nearly 140 000 inhabitants, the just and the unjust alike, was peremptorily taxed that the storage reserve might be increased. The form of the tax here levied was in time; the monetary value of this will depend upon the wage earning capacity of Rochester's citizens. What this means may only be conjectured, even when we have Mr. Rafter's statement that the result of these restrictive measures was to decrease the consumption by nearly 2 000 000 gallons a day, that is to say, increase the former time necessary to obtain a given volume by from 30 to 40 per cent., or made it impossible to get such supply at all.

The summation of Mr. Rafter's opinions, however, has my particular attention, wherein he sets forth that by this method of restrictive supply the "general movement in favor of metering" was, fortunately for the public, presumably, headed off, "which, if carried out under existing conditions, would have aggravated a situation which is already sufficiently unsatisfactory."

The premise upon which I shall mainly rely in my further contention is this: that if the actual cost, due to the loss of time and personal inconvenience, to which the citizens of Rochester have been subjected by these restrictive measures, had been invested in water meters of any fairly reliable pattern, were they of the cheapest inferential Continental type or the most expensive positive system here in vogue, that then not only would there have been no temporary inconvenience, but there would immediately have been water enough and to spare; the pressure would have been restored and the future supply insured beyond all doubt. By this simple expedient every water-taker would have become an inspector. Instead of a handful of irresponsible employees acting as water-waste detectors, there would have been thousands of willing volunteers. And why? Because it would then have been a simple matter of selfish pecuniary interest on the part of every metered water taker. Human nature is the same the world over. It is not implied, nor does it follow, that because the segregated members of a community act with thoughtless disregard of others that therefore all are venal. Nevertheless, this hard, indisputable fact remains: that he who purchases by the sole measure of what he can take in the time at his disposal, will carry off more than if he had bargained to pay for bulk or weight. The proof of this assertion is on every hand; from the earliest evolutionary evidence of our ancestry, grabbing more than can be carried and snarling at the loss, to our modern teachers of morals who complacently latch down the spring closing valve that cool water may be ready to the cup, the principle is in all cases the same. And so it would be if you and I were provided with coal or gas, bread or champagne at so much per quarter, take what we like; the damper would not then be so carefully regulated; the extra jets would not be put out; bread would only be used for end pieces and corks would fly upon the slightest indication of las-

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situde. Wherefore, I do not believe that we would be morally worse than at present; for the par value of the individual is the resultant of the society of which he is a factor. But in setting this, my opinion, with these generalities, as against the judgment of Mr. Rafter, I do not thereby wish to be understood as implying that even if he had held my undoubtedly extreme views on this subject, that he, per consequence, would or could have applied them in practice: because it is not neglected to be observed that too often the municipal engineer may be limited in his work by the judgment of higher municipal authority; be it always excepted, however, that he is seldom justified in the performance of a professional duty not warranted in its method and execution by the best practice of the hour.

But the lavish waste which so nearly resulted in a water famine in Rochester did not take place nor come upon it without ample notice from competent authority, as may be found in the annual reports of its long time Chief Engineer, Mr. Tubbs, who for years had been a conservative advocate of restrictive measures. Neither was the more or less brilliant idea of closing the spigot a growth of the year 1890; as may be judged by the following paragraph from a letter written by me and published in the Rochester *Daily Herald* of May 27th, 1889:

"In my judgment Mr. Kuichling is right in either of the two remedies which he proposed to your Special Water Works Committee. 'Good gracious,' you say to the first, 'and shall the pressure be reduced?' Why not, if you are already demanding more than the supply? Mr. Kuichling evidently recognizes a point you overlook, namely, that if you were supplied with twice your present volume you would doubtless double your wastage. This feature is already being considered here in New York, for when we shall have had the increased pressure of the new conduit, the 'consumption' of leaky pipes, faulty faucets, etc., is certain to reach high figures.

"So far as possible I endeavor to keep pecuniary motives in the background when I say, as a matter of concurrent judgment, that Mr. Kuichling's second remedy, namely, to meter the services, is the only true and satisfactory method, and that this is not theoretical or visionary has been proved times over.

"And the cost of the meter method of increasing your water supply by equalizing its disposal would not be the 'enormously expensive job to the city or its citizens' which you may think, and for this reason if no other, namely, that even if a portion of your services were metered the waste would be enormously reduced, because you will find that the great majority of your water takers are willing to pay for what they use; that such people in any case would not abuse their privileges; that the opposition, the growl, will be, as doubtless it now chiefly is, from those who are taking more than they are honestly entitled to."

I have purposely refrained from quoting the generally known data in

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support of my position as to which is the better method, legitimate use by all or waste by all, for the particular reason that I have here to present a most admirable discussion, covering this phase of the subject, prepared at my request by L. N. Case, Esq., Secretary of the Board of Water Commissioners, Detroit, Mich. I applied to Mr. Case for these principal reasons, namely: that the relative conditions as between Rochester and Detroit are more nearly alike than any others of which I am aware; that meters and restrictive measures were applied there for the purpose of avoiding the duplication of force mains and pumps, and that Mr. Case is not only thoroughly conversant with all the available facts, but has the peculiar skill of compressing such matter into clear and compact form.

To recapitulate in brief, my criticisms in respect of Mr. Rafter's papers are these: That while the methods which he employed may have been generally correct, the apparatus itself was not adequate to the requirements of the case nor in touch with well known modern practice; and that his opinions as to the probable consequence of metering are fully controverted by good and sufficient evidence.

Without pointing the moral which you will the better apply, I take the liberty of concluding in the following words from Judge Stallo's preface to his "Concepts of Modern Physics":

"It is an old truth, which, however, is too often lost sight of, that many of the questions of science and philosophy remain unanswered, not by reason of the insufficiency of our knowledge, but because the questions themselves are founded upon erroneous assumptions and require answers in irrational or impossible terms."

L. N. Case, Board of Water Commissioners, Detroit, Mich.—For the purpose of properly estimating the waste prevalent in the City of Detroit, I will state that the population of the city in 1888 was 192 729, and the daily average consumption 39 397 716 gallons or 204 gallons per capita; a waste, allowing for a generous use, of 150 gallons per capita, or nearly 30 000 000 gallons per day. In 1890, two years thereafter, as a result of the introduction of restrictive measures, the following data are offered: Population of city, 214 122; daily consumption, 33 208 067 gallons, or a per capita supply of 155 gallons, and a saving of about 12 000 000 gallons of water daily, and of about $20 a day or $7 000 annually in fuel alone. This, however, does not give the real saving effected. Several times in the winter of 1888–89 over 50 000 000 gallons of water were pumped into the city daily, and the demand even then was by no means satisfied, as many complaints reached us of a short supply in the upper and western portions of the city. It was then demonstrated that our capacity to supply was exhausted, or rather its limit reached.

I will explain by saying that our pumping station is 4 miles up the river

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with two 42-inch force mains leading therefrom into the city, one for the low and one for the high service. Our average daily consumption had increased 3 000 000 gallons each year for several years, and of course was gradually increasing. The necessity for another force main, and another engine, an enlargement of our present engine house or the building of a new one, and other attendant expenses accompanying such construction, was conclusive. An estimate was made of such extensions, which amounted to about $600 000.

Had the Board concluded to enter into such enlargement of the works, bonds would have had to be issued bearing 4 per cent. interest, entailing an annual expense thereafter of $24 000, to say nothing of the constant depreciation in the value of the plant, as well as the increased expense for fuel, salaries of engineers, lubricators and other supplies, which would amount at least to $11 000 per annum. I will give as my basis for this calculation, the fact that the total expense at the pumping station in 1890 was $54 433.49 for three engines and never but two of them running, so that an estimate of the above increase is a modest one to say the least. Here then is an annual expense of $35 000, which an enlargement of the plant would have forced upon the Board, or rather upon the citizens of Detroit.

At its meeting in February, 1889, these facts were presented and several other facts bearing upon the question of waste, and comparisons offered betwéen Detroit and other cities that had adopted measures to prevent waste. The Board, after carefully considering these facts, adopted a resolution to at once enter upon an active campaign against a waste that had become actually appalling, and directed their Secretary to adopt such measures as were reasonable and lawful that would accomplish their purpose.

Now, as to the measures adopted, they were by no means novel or unusual, and were simply those of house to house inspection and the introduction of meters. Could meters have been universally introduced, the inspection of premises would have been unnecessary, and at best accomplishes but little. Innumerable leaks, of course, are discovered, and require to be repaired, but the willful, extravagant, and utterly useless running of water from all classes of fixtures, through carelessness, and often a mistaken idea that such streams cleanse and purify the sewers, can only be controlled and regulated by the placing of meters. This of itself is a sufficient reason for their adoption, and yet another appeals to me with almost an equal force.

Estimating the quantity of water consumed, even with the best of intentions and ability on the part of the assessor, is after all only guesswork. This is not simply an assertion, but is the result of actual experience. The following table is prepared from actual facts upon the books of this office, and is based upon the consumption of water which

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was being used as developed by the meter, and upon the assessments which were paid before the water was metered:

Consumers.	Monthly Consumption as per Meter.	Monthly Assessment.	Paid for 1 000 Gallons.
A	583 500 gals.	$7 50	1½ cents.
B	132 000 "	2 00	1½ "
C	3 429 803 "	36 66	1 "
D	531 375 "	33 33	6¼ "
E	185 250 "	13 67	7⅜ "
F	2 697 831 "	119 16	4⅜ "
G	472 201 "	39 66	8⅜ "
H	259 312 "	17 50	6¾ "
I	138 850 "	28 33	20 "
K	727 125 "	50 00	6⅞ "
L	2 127 050 "	53 33	2½ "
M	270 750 "	37 66	14 "
N	1 352 850 "	142 00	10½ "
O	462 818 "	43 00	9⅓ "

The parties "C" and "D" are engaged in the same business, competing for the same trade, and yet "D" was handicapped by being obliged to pay, in the same city and under similar circumstances, over six times as much for water for manufacturing purposes as "C" did. The party "I" is a miller, and his uses were of a mechanical nature almost entirely, and easily estimated, and it was proved that he was paying exactly right, as 20 cents per 1 000 gallons, when estimated, was then the rate charged by the Board.

As a result of the saving thus made, the Board have reduced their meter rates from ten cents per 1 000 gallons to three and one-third cents to all consumers, and in addition have abrogated the charge for hose use for lawn and street sprinkling purposes, the assessments upon which, the year previous to such action of the Board, amounted to over $20 000.

The placing of meters has been with us, and will be everywhere, a great saving individually and collectively. For instance, the present receipts for the entire metered consumption is not over two-thirds of what it would be under the assessment plan, and the amount of water consumed not over one-fourth. The result of the introduction of meters in the City of Detroit can best be shown by the following comparison:

Had the Board concluded in 1888 to make the contemplated enlargement of the works instead of introducing measures of restriction, the annual expense caused by such enlargement, as already shown, would have been at least $35 000, or for three years $105 000, with an unestimated loss for wear and tear, and no reduction in rates. The total expense for the introduction of meters has been up to October 1st, the present month, $50 812.50, with an estimated sum for the three months thereafter will make $56,000.

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But mark the difference between the two expenses! The former, of $35 000 per annum or $105 000 for the three years, would have been an absolute expenditure; while of the latter at least 85 per cent. is still in existence, and is a valuable part of the plant.

J. Nelson Tubbs, M. Am. Soc. C. E.—The methods described in Mr. Rafter's paper for reducing the consumption of water in the City of Rochester, while confessedly not new or original, may be regarded as a wise provision to tide over a temporary crisis, and without doubt accomplished the purpose of temporarily preventing the further depletion of the reservoirs. The lack of vigorous action on the part of the municipal authorities having the control of the water department, in failing to provide an abundant temporary supply of water for the city from one of several possible near sources, left no alternative to the engineer but to restrict the use of water to such an extent as would prevent its utter exhaustion from the reservoirs. In this view of the case the action of the engineer is to be commended, while the want of prompt and right action on the part of the municipal authorities, whereby such drastic measures in the way of restrictive use became necessary, is to be seriously reprobated. The adoption and continuance of these restrictive measures have proved so annoying, vexatious and threatening to manufacturers and other citizens that private sources of supply have been secured by many of them, thus reducing the consumption from the city works to the extent of at least one and one-half million gallons per day, involving the consequent loss of revenue to the city. It has also involved the necessity of sprinkling the streets with the sewage laden water of the Genesee River. It has had a tendency to restrict the introduction of new manufacturing establishments, the opening of new streets, and the erection of new dwellings; and it has prevented the proper flushing of sewers and drains, and has thus proved detrimental to the sanitary condition of the city. I cannot, therefore, concur in the wisdom of the further suggestion of the paper under discussion, that by the continuance of these restrictive measures the reservoirs may be kept full, and "aside from street and lawn sprinkling, the necessary legitimate uses of water can be supplied for two or three years, during which time the additional supply construction may be completed." The many and complex interests of a city are so injuriously affected by a scarcity in the water supply for a series of years, that no make-shift should be tolerated which looks to the continuance of a system alike perilous to the business, prosperity, comfort and health of the municipality.

Some of the conclusions of the writer of this paper I am impelled to call in question. These are based upon a doubt as to the original capacity of the conduit to discharge at the rate of 9 000 000 gallons per day, from the fact that the discharge is at the present time but 7 000 000 gallons per day. The method of determination by which it was found

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to deliver the larger quantity in 1876, was exactly the method employed in 1890, when it discharged the smaller quantity.

The evidence that the said conduit did originally discharge 9 000 000 gallons per day is of the highest character. In 1876, I was chief engineer of the water-works in question, and the late Lorain L. Nichols, a very competent man of large experience, was one of my assistants. During the filling of the reservoirs and the months following, Mr. Nichols was directed by me to make such careful measurements and gaugings of the reservoirs as would enable him to determine very closely the approximate flow of the conduit. He did this on at least four different occasions, the records of which, in his own handwriting, are now on file in the Water Department at Rochester, and his conclusion from these experiments was, that the flow was substantially 9 000 000 gallons per day. I, therefore, regard it as much a definitely settled fact that the flow in 1876 was 9 000 000 gallons per day, as it is a fact that at the expiration of sixteen years, in the year 1890, the flow was but 7 000 000 gallons per day, and both facts were determined in the same way, and are based upon the same class of evidence, to wit: the rise of water in the reservoir under the influence of the flow of a full conduit.

From what has been said, it may be seen how difficult it would prove to determine when, or at what moment, any statistics published in the annual reports of the Rochester Water Department, based upon the earlier determination of the capacity of the conduit, ceased to be entirely true. Only two facts are known, to wit: In 1876, and for some years thereafter, I am convinced by personal observation of the conduit, the discharge was substantially 9 000 000 gallons per day. In 1890 the flow was 7 000 000 gallons per day. When did it cease to be the greater and become the less quantity? The imperative necessity that has existed for the past three years, that there should be no interruption in the flow of water for a single hour which could be avoided, has prevented such an examination as would determine the cause of this diminution of flow. Presumably, however, the decrease was a gradual one, and so gradual as to be unsuspected, and not such a sudden and abrupt one as would have at once attracted attention. History repeats itself in these matters. So far as I know, the history of all iron pipe water mains has been a gradual and more or less rapid decrease in their carrying capacity by the formation of tubercles, often accompanied with the growth of extensive vegetable life, the latter of which in the form of spongilla in large quantity has been found recently in the cast iron pipe forming a part of the Rochester conduit, by Mr. Kuichling, a member of this Society, on removing the dome of a 24-inch gate for repairs, the details of which are given in his annual report recently published. I can see in all this no reason to question the large co-efficients used in modern formulas for large, new and clean pipes, but it emphasizes the fact that the pipes will not always remain new and clean, in which case the large

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co-efficients are no longer applicable, or, in other words, that to approximately determine the flow of a pipe at the end of twenty years' use formulas with relatively small co-efficients should be used.

W. N. RADENHURST, M. Am. Soc. C. E.—Mr. Rafter, in his paper, states in his comparisons of the results of tests made: First. That the "average rate of hydraulic grade in the 36-inch wrought iron pipe is 50 per cent. greater than it should be for clean pipe with the observed discharge." And again, "That the excessive resistance prevailing in the 24-inch pipe at the present time, is mostly confined to the wrought iron section, the excess in the cast iron being, generally speaking, only such as may be reasonably expected in pipe a long time in use."

As no 36-inch cast iron pipe exists on the line, all criticism must be directed to the 24-inch conduit alone. Attention may, however, be called to the fact that the hydraulic grade line found the same day is practically a straight line, and that there are no very marked irregularities in loss of head in this section.* As to the irregularities on the 24-inch pipe pointed out by Mr. Rafter in his basis of comparison, the question arises whether such a marked difference in loss of head would be discovered, if the pressure observations had been made with a suitable mercury gauge.

The engineer who made the original gaugings and calculated the inflow into Rush Reservoir, the late L. L. Nichols, has left in his handwriting carefully prepared notes of the gaugings taken by him at Rush Reservoir on four different occasions, the least of which gives about 8 000 000 gallons, and the greatest about 10 000 000 gallons in twenty-four hours. That it now delivers only a trifle over 7 000 000 gallons daily must also be conceded. A reduction in delivery must accordingly be acknowledged, and I desire to discuss the question on that basis. The reason for this diminished delivery may, perhaps, be due to obstructions at a number of places, since the diagram of the several hydraulic grade lines does not show a sufficient fall between any two consecutive air valves to account for the entire difference in discharge. The probable causes are tubercular formations and the growth of spongilla or similar organisms. Mr. Rafter is inclined to the belief that the wrought and not the cast iron pipe is to be blamed for the failure in delivery. This may be so; but that apparently well coated cast iron pipe deteriorates, and often deteriorates very rapidly, is, nevertheless, a fact of which ample demonstrations have been given in our professional literature.

The probings made by Chief Engineer Kuichling in the 36-inch wrought iron pipe at air valve No. 53, and in the large tapering casting inserted in the 24-inch cast iron pipe at Rush Reservoir, and fully described by him in his last annual report (1890–91), gave no evidence of "tuberculation by rust, mineral incrustation," etc. He did, however, find a large growth of spongilla on the interior surface of the 24-inch

* See profile accompanying the discussion on Mr. Rafter's paper already submitted to the society by Mr. Kuichling, M. Am. Soc. C. E.

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cast iron effluent pipe at Rush Reservoir. In the city work at Rochester it has been the practice for over a year to carefully examine the interior surfaces of all mains, wherever the same may be cut for insertion of special castings, or for making extensions, etc., and to make notes of the conditions found and preserve specimens of the broken pipe, and the following table showing the condition of pipes of different ages, selected from a number—all Hemlock mains—may be of some interest on the question of deterioration.

No.	Diameter of Pipe. Inches.	Street.	Location.	Date of Laying.	Date of Examination.	Years.	Conditions.
1	4	Stone St.	Corner of Ely St.	1874	Nov. 8, '90	16	Badly corroded, large tubercles half an inch thick, covering surface coating, destroyed. This pipe was made at one of the foundries that furnished the 24-inch cast iron conduit pipe.
2	12	Genesee St.	50 feet South of College St	Sept. 15, '80	Aug. 25, '91	11	Large tubercles, over three-eighths of an inch, and thick mat of spongilla.
3	6	West St.	75 feet North of Lorimer St...	Aug. 31, '87	Oct. 28, '91	4	A few tubercles and clots of rust —coating good between.
4	10	Lake Ave.	At Clay Avenue	Oct. 27, '88	Oct. 27, '91	3	Tubercles sparse, but of fair size, pipe coating between tubercles glossy and smooth.

Specimen No. 2 was particularly luxuriant, both as to the thickness of the tubercles and the strong growth of spongilla. Specimen No. 1 was cut out of pipe laid sixteen years ago, and was cast at one of the foundries that supplied the 24-inch pipe laid in the conduit line. It doubtless was treated to the same coal-tar pitch bath as the large pipe. By parity of reasoning, if rust tubercles have formed on the inside of the small pipe, which has actually been seen, why not form on the large pipe, which has not yet been examined?

It is generally assumed that the coal-tar pitch coating as applied to cast iron water pipes by most of the foundries in this and other countries, is a permanent protection to the iron against rust. It certainly does not appear to act in this manner in all cases. The writer is informed that Messrs. A. D. Wood & Co. did some years ago in their foundry at Florence, N. J., use a mixture of 20 per cent. of pure linseed oil with the coal-tar pitch, but does not know whether this practice was continued.

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Most foundries use the coal-tar pitch alone. A permanent coating seems to be almost as vital to the life and usefulness of the pipe as the quality and strength of the iron; and it is respectfully suggested that the members of the Society who are interested in this branch of the profession, should, from their experience and observation, give their views as to what change could be effected in the composition of the coating material, so as to render it more durable than it is found to be in practice. The coating as applied to both exterior and interior surfaces of the wrought iron 36 and 24-inch pipe used in the conduit was composed of about equal parts of refined Trinidad asphaltum and coal-tar pitch, the proportions being somewat varied in cold weather by an increase in asphaltum, as the coal-tar pitch had a tendency to make the mixture brittle.* The testimony of those who have seen the exterior of the wrought iron conduit pipe is to the effect that the coating is still in a good condition (the same, however, is mainly true in the case of the cast iron), but unfortunately in no case has the interior of the pipe been seen.

In connection with this question of coating wrought iron, the writer personally examined a case where a 24-inch wrought iron pipe used as a flume for conveying water, and which was coated with "Black Parafine Varnish," ordinary boiler-maker's composition, was exposed after seventeen years' life, it having been laid in 1874 and exposed April 3d, 1891. The pipe coating showed a bluish black color, and exhibited no signs of corrosion. Judging from the exterior, as seen, the question may be asked: Is the interior asphaltum coating a preservation against corrosion? The members of the Society are respectfully requested to send in contributions also on this point.

It may be of interest to state that Mr. Nichols made what is believed in this country to be the first examination in the nature of accurately determining "Discharge from Drainage Basins." He acted as Mr. Jervis' assistant, and was for two years gauging the flows of Eaton and Madison Brooks, for feeding the summit level of the Chenango Canal.†

GEORGE W. RAFTER, M. Am. Soc. C. E.—In closing the debate I may first call attention to a fact that has been apparently lost sight of by some of the gentlemen participating, namely, that the tests briefly recorded in the paper on the Hydraulics of the Hemlock Lake Conduit, etc., were not undertaken originally for the purpose of either discovering new principles in hydraulics or confirming old ones, but merely for the purpose of finding out by the application of known principles why the Hemlock Lake Conduit of the Rochester Water Works only delivered in the summer of 1890 about 73 per cent. of what it had been claimed to be doing a little over a year before. Instructions to begin the tests were given by the Executive Board to the writer on July 7th, 1890. At the

* For a further statement see Report of J. Nelson Tubbs, M. Am. Soc. C. E., for year ending January, 1874, page 25.

† See Report of J. B. Jervis, dated January 14th, 1836. Reported in Senate Doc., No. 5 N. Y. S., for the year 1838, p. 209; also in State Engineer's and Surveyor's Report, State of N. Y., for 1862, pp. 203 and 204.

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same time it was requested that the work be completed and the results presented in not exceeding thirty days from that date.

On looking the matter up it was found that the original construction bench marks were nearly all obliterated and the records were deficient in many respects. It appeared necessary, therefore, to make a new profile with careful location of all the details. The measurements were made with a 100-foot steel tape, and the locations of the air valves are believed to be fairly accurate to the nearest foot. The new series of benches have been referred to in the paper.

Reference has been made in the discussion to my results as not scientifically correct. The writer of this statement probably means minutely correct, in which case it becomes at once evident there is as yet no science of hydraulics. An engineer is not necessarily a man of science in the sense of always working to the second, third or fourth decimal. As one who "adapts the forces of nature to the comfort and convenience of man," he will often be obliged to use the things at hand; and generally he is the best engineer who uses the material at hand in the shortest time for the attainment of the desired result.

In writing my papers, details have been purposely omitted as likely to be of little interest to the Society. Inasmuch, however, as the detail has been asked for, I will endeavor to give such as seem necessary in reply to the discussion.

It is conceded on my part that whatever the actual performance of the Rochester conduit may be, its theoretical capacity is in the vicinity of 9 000 000 gallons daily; but the realization of this capacity in actual performance would imply that theoretical conditions were complied with in its construction. The positive statements to the contrary by intelligent persons should be taken account of in discussing the question, and it is because of the existence of such information that I have doubted the correctness of the original determination.

In reviewing the remarks of Mr. Kuichling I will, for convenience, use the same heads which he has himself used, namely, first, second, third, fourth.

(1) It is admitted that the observations given in Table No. 1 were not simultaneous; they were not even all taken on the same day. Indeed I know of no practicable way by which they could be all taken simultaneously, although they might possibly all be taken on the same day. As shown by Table No. 1 and the profile Plate XII, there are sixty-nine air valves between Rush Reservoir and Hemlock Lake, and sixty-nine simultaneous observations would imply at any rate sixty-nine observers, each with at least two assistants. However, at about ten valves the conditions are such that two observers would be required with four or five assistants. Simultaneous observations would therefore imply say seventy-five observers with one hundred and sixty assistants, or a total of two hundred and thirty-five men in all. Before the work could be properly performed, all of these persons would need special instructions

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by actual practice in the field. Mr. Kuichling's objection that the observations were not taken simultaneously may be therefore dismissed without further consideration.

Mr. Kuichling has compiled in his Table A the record of work from day to day, and about what can be done by one party is there indicated at various parts of the line. From six to eight fully equipped parties would be required for doing the work in one day, and this, while not impossible of attainment, would also be somewhat difficult.

In reference to these observations it is stated that they were begun, as indicated in the appendix, on July 25th, 1890. At that time repairs of a serious leak at about Station 70 were in progress, and it was found that the effect of the centrifugal pump in use for draining the excavations was clearly felt in causing a pulsation in the flow of the conduit. This was especially marked on July 26th and all the observations taken during the working hours of these two days were accordingly rejected. During the night of July 26th and a portion of the morning of July 27th, while the said pump was not in operation, a series of observations were taken at Air Valves 83, 84 and 85, in order to determine whether pulsations took place when there were no such disturbing influences as had existed during the working hours of July 25th and 26th. The result of these observations was as per following:

TABLE No. 8.

July 26th and 27th. Hours.	Elevation of Piezometer at Air Valve 83.	July 26th and 27th. Hours.	Elevation of Piezometer at Air Valve 84.	Elevation of Piezometer at Air Valve 85.
July 26, 7 P. M.		July 26, 7 P. M.		
7.30 "	380.72	7.45 "	381.23	381.52
8 "	380.72	8.15 "	381.23	381.47
8.30 "	380.69	8.45 "	381.17	381.51
9 "	380.70	9.15 "	381.16	381.50
9.30 "	380.71	9.45 "	381.17	381.48
10 "	380.74	10.15 "	381.17	381.48
10.30 "	380.73	10.45 "	381.20	381.47
11 "	380.73	11.15 "	381.21	381.47
11.30 "	380.73	11.45 "	381.16	381.50
12 "	380.70	July 27, 0.15 A. M.	381.17	381.49
July 27, 12.30 A. M.	380.71	0.45 "	381.17	381.44
1 "	380.71	1.15 "	381.17	381.47
1.30 "	380.71	1.45 "	381.18	381.47
2 "	380.71	2.15 "	381.18	381.47
2.30 "	380.73	2.45 "	381.18	381.47
3 "	380.73	3.15 "	381.18	381.47
3.30 "	380.73	3.45 "	381.18	381.47
4 "	380.725	4.15 "	381.175	381.47
8.30 "	380.73	8.25 "	381.21	381.57
9 "	380.73	9.05 "	381.22	381.51
		11.15 "	381.18	381.49

Taking the means we obtain:

		Grade.
Air Valve 85 —	0 — 381.474	
	(0.295) —	0.4307
Air Valve 84 —	685 — 381.179	
	(0.456) —	0.4449
Air Valve 83 —	1 025 — 380.723.	

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Mr. Kuichling has given in his Table A the series of observations (*a*), (*b*) and (*c*) for July 26th, at these valves. All observations as here tabulated for July 25th and 26th, have been rejected by reason of known disturbance from the pump already referred to.

In the same way other observations have been rejected for good and sufficient reason. Relative to this matter of rejecting observations, I may refer to the dictum of Mr. Wright. He says :*

"In making a series of observations the observer is given full power. He can vary the arrangements, choose his own time for working, reject any result or set of results ; he can do anything, in fact, that in his best judgment will tend to give the best value of the observed quantity."

In reference to the rise of water surface at Rush Reservoir, a study of the matter with a profile platted at large scale shows that this also is not of such significance as to in any degree vitiate the conclusions.

(2) The fluctuations referred to by Mr. Kuichling were frequently observed, and the recorded elevations in the notebooks show the mean of the fluctuations at the time of making the observations. Usually the range was from 0.02 to 0.05, and, except at air valves 55, 56 and 57, very rarely as much as 0.07, although 0.10 has been observed on a few occasions. The fluctuations were mostly followed by periods of rest, during which the level of water in the piezometer was nearly constant, and to which it may be said to return after one of the periodic fluctuations. With very short distances between air valves an error in grade may occur by reason of these fluctuations, but, so far as the final result on the long lines is concerned, the fluctuations have practically no significance. It will be observed that my conclusions in the paper are based entirely upon the results for the long line, and it is these results that Mr. Kuichling must show untrue before the conclusions of my paper are in any way impeached.

At air valves 55, 56 and 57 a record was kept for a number of days, but the observations were not reduced last year for lack of time, and inasmuch as the notes are not now in my custody I have as yet had no opportunity of reducing them. The relatively great and sudden fluctuations at air valves 55, 56 and 57 may to some slight extent be due, as suggested by Mr. Kuichling, to disturbances by reason of the progress of repairs. They occurred, however, not only at times when no repairs were going on, but also when the conduit was found by trial to be free of air. They are, I believe, largely due to disturbing influences in the way of obstructions in the conduit itself in the vicinity of air valve 56. The statement of the keeper of Rush Reservoir in regard to discharge of air, as made to me, applied only to periods immediately following the emptying and refilling of the line.

There is, however, no question as to the freedom of the conduit

* A Treatise on the Adjustment of Observations. By T. W. Wright. See Note II on the Rejection of Observations, page 131.

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from air during the period covered by these tests. Special care was exercised on this point, not only by observation when opening air valves for the tests, but by frequently opening valves at critical points where the tests were not in progress in order to insure such freedom.

(3) Taking up the question of pressure gauges as used from air valves 46 to 23, it may be said that the four gauges used on August 5th and 6th were from the Utica Steam Gauge Company, while the two in use August 15th and 16th were made to special order by Schaeffer and Budenburg. The history of the matter is as follows: Several new Utica Steam Gauge Company's gauges, purchased for another purpose, were on hand at the time of beginning these tests, and it was assumed that an ordinary pressure gauge would answer the purpose of determining whether there were special obstructions at particular points. It was further assumed that even though the gauges were not absolutely standard, so long as the error remained the same or nearly the same at every point, the indication of rate of grade between consecutive points might be accepted as reasonably accurate. The truth of this latter assumption, which is the important one, is shown by comparing the results between air valves 35 and 29 as obtained August 5th and August 16th, respectively. The gauges used August 5th are known to have been at least a pound in error, yet the grade obtained on that day bears the proper relation to the grade of August 16th. The Utica Steam Gauge Company's gauges, however, were not satisfactory, and the work was therefore stopped on August 6th and a special order given to Schaeffer and Budenburg to make a pair of gauges which would stand the pressures required. The gauges received from them were the Nos. 1 076 544 and 1 076 545, referred to by Mr. Kuichling. They were tested together on August 14th, 1890, giving the first record, tabulated by Mr. Kuichling. They were again tested August 23d, giving the second record of Mr. Kuichling's table. It was fully understood at the time of making these tests that the Woodbury Engine Company's test gauge was far from correct, and the record of that gauge has no significance other than as showing its incorrectness. These tests of gauges were made at the Woodbury Engine Company's shop by reason of a pump and convenient appliances for attachment of gauges being at hand there. Schaeffer and Budenburg certify in writing that the two gauges were made to a special order, were accurately adjusted to standard and were warranted to stand water pressure up to 150 pounds. The tests made both before and after their use in the Rochester conduit justifies this statement on their part.

Mr. Kuichling refers to his Table A, and diagrams, Plate XIII, as illustrating the uncertainties attending the use of pressure gauges. I have already stated that a considerable portion of the data which he has compiled in these two exhibits was rejected because of known inaccuracy. The probable limit of error in the matter is, I believe, properly indicated in the foregoing explanation.

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(4) As to the relation between nominal diameters and actual diameters I would refer to the annual report of 1876, which states that the diameters were determined at 24 inches and 36 inches respectively, from careful measurements made during the progress of the work.

This question was, however, given attention at the time of making the tests. The 24-inch cast iron pipe was cast, I believe, at three different foundries, and pieces of the same class from different foundries, as measured last year, gave different results. The sections of wrought iron pipe still in stock were found to vary somewhat, and there was no way of treating the matter except to assume the nominal diameter, and variations from other causes were probably greater than those due to this assumption. The figures given by Mr. Kuichling indicate that, generally speaking, the variations in diameter are in the right direction to favor the pipe.

Mr. Kuichling is in error in stating that the method used in changing the direction of the pipe was not taken into account. It was not only considered carefully, but in my percentage comparisons, allowances have been made in excess of the real necessities of the case, as is shown by what follows:

Let us estimate the amount of head which is probably consumed in overcoming the various resistances due to bends and angles in this pipe, making use of the method of Weisbach, found in Neville's Hydraulics,* Section XI, where is given a convenient epitome of the several experimental researches on the subject. Weisbach's formula apparently takes into account all the several elements of the problem. It is, moreover, based on experiments with small pipes, and it may be safely assumed that the actual resistance from bend in pipes 2 and 3 feet in diameter will not be greater than is indicated by the formula.

The final expression for the loss of head due to bends in circular pipes is:

$$h_b = \frac{\phi}{180}\left\{0.131 + 1.847\left(\frac{d}{2p}\right)^{\frac{7}{2}}\right\}\frac{v^2}{2g} \quad \ldots\ldots\ldots\ldots \quad (1)$$

in which

h_b = head in feet consumed in overcoming the special resistance due to any given bend or curve;

ϕ = the deflection angle;

d = the diameter of the pipe in which the bend is inserted;

and p = the radius of the axis of the bend.

For the 36-inch pipe two separate cast iron bends were provided, namely, 8 degrees and 15 degrees. These are what may be termed short bends, the radius of the axis being the semi-diameter of the pipe. Let us apply formula (1) to the 15-degree bend.

With a discharge of 6 700 000 gallons in twenty-four hours $v = 1.467$

* "Hydraulic Tables, Co-efficients and Formulas, etc." By John Neville. Third Ed., 1875.

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linear feet per second. For convenience we will take it at 1.5. We have then for the 15-degree bend:

$$hb_1 = 0.0059.$$

If we compute the value of h_b for the 36-inch (8-degree) bend, we find:

$$hb_1 = 0.00316.$$

Table No. 4 gives the number of bends on the 36-inch pipe, and there was one of 40 degrees at station 493 + 65. This was a special of 20 feet radius, made, probably, of about the form shown in Fig. 1.

Formula (1) for this "special" gives

$$hb_3 = 0.00002 —.$$

In the same way, the formula gives for bends of intermediate degrees of deflection a nearly inappreciable loss of head. Table No. 4 gives in detail all the bends on the 36-inch pipe. In the column of "Resistance allowance" is given, without actual computation for every case, what may be considered the reasonable allowance for each bend.

TABLE No. 4.

Serial Number.	Station.	Degree.	Horizontal or Vertical.	Remarks.	Resistance Allowance.	Serial Number.	Station.	Degree.	Horizontal or Vertical.	Remarks.	Resistance Allowance.
										Br't for'd.	0.1745
1	79 + 03	14°	*H*	*.....	0.006	31	355 + 00	10°	*V*	*.....	0.0045
2	82 + 70	14°	*V*	*.....	0.006	32	357 + 00	20°	*V*	*.....	0.0250
3	83 + 00	14°	*V*	*.....	0.006	33	370 + 72	15°	*V*	Special†	0.006
4	100 + 78	13° 53′	*H*	*.....	0.006	34	371 + 72	10°	*V*	Special†	0.0045
5	161 + 00	15°	*H*	*.....	0.006	35	386 + 43	19°	*H*	Hub‡	0.0250
6	166 + 85	15°	*H*	*.....	0.006	36	390 + 36	9°	*V*	Hub‡	0.0045
7	167 + 15	15°	*H*	*.....	0.006	37	393 + 40	13° 30′	*V*	Hub‡	0.0060
8	191 + 50	28°	*H*	*.....	0.010	38	395 + 65	9° 30′	*V*	Hub‡	0.0045
9	221 + 40	12° 20′	*H*	Special†	0.005	39	411 + 00	2° 30′	*H*	*.....	0.0001
10	221 + 75	8°	*V*	*.....	0.004	40	417 + 00	10° 20′	*V*	*.....	0.0045
11	249 + 00	10°	*V*	Special†	0.004	41	419 + 30	10° 20′	*V*	*.....	0.0045
12	263 + 23	8°	*V*	Hub‡	0.004	42	423 + 60	15°	*V*	*.....	0.006
13	273 + 13	15°	*V*	*.....	0.006	43	424 + 05	15°	*V*	*.....	0.006
14	273 + 70	15°	*V*	*.....	0.006	44	432 + 00	13¼°	*V*	Hub‡	0.006
15	274 + 00	8°	*V*	*.....	0.004	45	432 + 51	8°	*V*	Hub‡	0.004
16	275 + 20	8°	*V*	*.....	0.004	46	436 + 00	8°	*V*	Hub‡	0.004
17	283 + 20	8°	*V*	*.....	0.004	47	437 + 00	8°	*V*	Hub‡	0.004
18	312 + 58	15°	*V*	*.....	0.006	48	445 + 00	6°	*V*	Hub‡	0.003
19	318 + 00	7°	*H*	*.....	0.003	49	447 + 50	7°	*H*	Hub‡	0.004
20	318 + 15	15°	*V*	*.....	0.006	50	461 + 50	30°	*H*	Special†	0.0005
21	319 + 65	15°	*V*	*.....	0.006	51	466 + 25	23° 20′	*H*	Special†	0.0004
22	327 + 00	7°	*H*	*.....	0.003	52	486 + 16	15°	*V*	Hub‡	0.006
23	333 + 10	12°	*V*	*.....	0.009	53	486 + 75	30°	*V*	Special†	0.0005
24	333 + 70	8°	*V*	*.....	0.004	54	487 + 15	11¼°	*V*	Hub‡	0.005
25	341 + 60	15°	*H*	*.....	0.006	55	488 + 50	20°	*V*	Special†	0.0004
26	342 + 30	15°	*V*	*.....	0.006	56	490 + 00	16°	*H*	Hub‡	0.007
27	344 + 00	10°	*H*	*.....	0.0045	57	493 + 23	20°	*V*	Special†	0.0004
28	347 + 00	18°	*V*	*.....	0.020	58	493 + 65	40°	*V*	Special†	0.0015
29	349 + 00	8°	*H*	*.....	0.004	59	494 + 12	8°	*V*	Hub‡	0.004
30	354 + 50	8°	*H*	*.....	0.004	60	496 + 00	11°	*H*	Bend*	0.005
				For'd	0.1745					Amount.	0.3313

* Record does not indicate what was used.

† As per Fig. 2, with 20 feet radius.

‡ Special cast iron bend with radius of axis equal to semi-diameter of pipe.

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In assigning values in the column of resistance allowance it is assumed that at places where the deflection is either 8 or 15 degrees the special cast iron bends were used. At odd angles where no indication is given, I have used a large enough value to cover an abrupt angle.

We thus determine that 0.3313 feet is the approximate theoretical value of the resistance due to bends in the 36-inch sections with a daily delivery of 6 700 000 gallons.

I have never seen any record as to the bends in the 24-inch wrought iron pipe, and am unable therefore to state the losses in that portion of the conduit. With a higher velocity and smaller diameter the effect of bends in it will, of course, be relatively much greater, but a very casual examination of the question will convince one that friction in bends does not explain the excessive resistance found in that section.

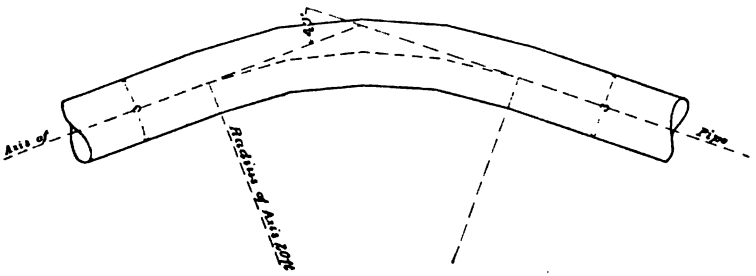

FIG. 1.

In order to compare the resistance in the whole conduit as actually determined by the tests with that in theoretically perfect pipe, the value of h'' was computed in the formula—

$$h'' = v^2 \left(4m\right) \frac{l}{2gd} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (2)$$

in which

h'' = the loss of head due to the various frictional resistances in a thousand feet.

v = velocity in feet per second = 1.467 for the 36-inch pipe when discharging 6 700 000 gallons in twenty-four hours.

m = a co-efficient, derived from Mr. Fanning's diagram.*

For 36-inch pipe with velocity of 1.467 per second we may take $m = 0.00448$. l = length in feet = 1 000 feet. d = diameter in feet = 3 feet.

Whence for the 36-inch pipe $h'' = 0.199596$ = say, 0.2.

This represents the result of reliable experiment as to loss of head per 1 000 feet for a daily discharge of 6 700 000 gallons in pipes 36 inches in diameter; and it is clear, since the effect of bends has been found so slight, that this value of h'' could be fairly used for the percentage comparisons as given in the summary to the paper. In order, however,

* Plate XI, Vol. XIX, of the Transactions of the Society.

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that I might do the conduit no injustice, I have used 0.3 in making the percentage comparisons for this sized pipe. In the same way a theoretical value of $h'' = 1.60$ was obtained for the 24-inch pipe and 1.75 used in making the comparisons. There is no reason, however, why the comparisons cannot be legitimately made with the values 0.2 and 1.60, and these values will accordingly be used for such purpose for the balance of this discussion.

Mr. Kuichling's remarks on the question of bends can only be taken as meaning, that he is disposed to explain a large portion of the excess loss in the 36-inch pipe at any rate as due to the bends. In the original computation for a 7 000 000-gallon conduit an addition of 0.267 feet was made to the diameter on account of the supposed considerable influence of bends in the upper section; while in the lower section only 0.009 feet was added for the same reason. The clear indication at the present time is that there is a lack of proportion between the 36-inch and the 24-inch sections of this conduit. The discharging capacity of the 24-inch is considerably less than that of the 36, as shown by the height of piezometer column at air valve 53, but whether this is due to changed conditions or to original lack of proportion, is unknown.

Moreover, if it is true that the bends are in any large degree responsible for the excessive friction in the 36-inch pipe, it follows, that where there are either no bends or only a few, the friction should be much less. Now what is the fact? From the gatehouse at Hemlock Lake to air valve 91, the alignment is not only nearly straight, but for a large portion of the distance the pipe is laid on a true grade. Just before reaching air valve 91, however, there are three bends of 14 degrees each. The hydraulic grade from the gatehouse to air valve 91 is 0.44, or 0.01 less than the mean grade for the entire distance to air valve 53. Again, from air valve 75 to air valve 74 the grade is 0.56, while in the intervening space there is only one bend of 10 degrees.

Table No. 5 gives the equivalent diameters of pipes which will allow the water now flowing to pass with only the loss of head per unit of length that has been found to exist by experiment with such pipes.

Taking the fundamental formula—

$$v = C\sqrt{RS} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (3)$$

in which the mean value of C for these computations $= 117$ and substituting in it the equivalents of R and S, which are $R = \frac{d}{4}$ and $S = \frac{h''}{l}$, we have

$$v = C\sqrt{\frac{d}{4} \times \frac{h''}{l}} \quad \ldots\ldots\ldots\ldots\ldots\ldots (4)$$

where d = diameter of conduit in feet,

h'' = head used in overcoming resistance in a distance l in feet as already found, namely,

for 36-inch pipe $h'' = 0.2$ in 1 000 feet, for 24-inch pipe $h'' = 1.60$ in 1 000 feet, and $l = 1\,000$ feet.

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The general equation for velocity is:

$$v = \frac{Q}{F} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (5)$$

Combining equations (4) and (5) there results:

$$Q = FC\sqrt{\frac{h'' d}{4l}} \quad \ldots\ldots\ldots\ldots\ldots\ldots (6)$$

whence we determine $d = \frac{4lQ^2}{F^2 C^2 h''}$ $\ldots\ldots\ldots\ldots\ldots\ldots\ldots (7)$

and $d F^2 = \frac{4lQ^2}{C^2 h''}$ $\ldots\ldots\ldots\ldots\ldots\ldots\ldots (8)$

In this form the problem resolves itself into computing with known values of Q, l, C and h'', the value of dF^2, and then finding by inspection and trial such value of d as will, with its corresponding value of F, satisfy the equation. In this way the values of d, as given in the last column of Table No. 5, have been found.

TABLE NO. 5—SHOWING THE EQUIVALENT SIZE OF THEORETICALLY PERFECT PIPE FOR THE APPROXIMATE ACTUAL DELIVERY OF THE SEVERAL SECTIONS OF THE HEMLOCK LAKE CONDUIT OF THE ROCHESTER WATER-WORKS IN 1890.

Section of Conduit.	Equivalent Size of Pipe for Actual Delivery in inches.	Section of Conduit.	Equivalent Size of Pipe for Actual Delivery in inches.	Section of Conduit.	Equivalent Size of Pipe for Actual Delivery in inches.
Well House to A. V. 91	31	A. V. 68 to A. V. 67	29	A. V. 49 to A. V. 48	22
A. V. 91 to A. V. 38	33	" 67 " 66	33	" 48 " 47	23
" 88 " 87	29	" 66 " 64	32	" 47 " 46	21
" 87 " 79	31	" 64 " 63	31	" 46 " 45	24
" 79 " 78	32	" 63 " 62	31	" 45 " 44	22
" 78 " 77	32	" 62 " 61	32	" 44 " 42	23
" 77 " 76	31	" 61 " 60	27	" 42 " 41	22
" 76 " 75	31	" 60 " 59	29	" 41 " 40	24
" 75 " 74	30	" 59 " 58	31	" 40 " 39	24
" 74 " 73	35	" 58 " 57	31	" 39 " 38	21
" 73 " 72	32	" 57 " 56	31	" 38 " 37	24
" 72 " 71	32	" 56 " 55	29	" 37 " 36	24
" 71 " 70	32	" 55 " 54	33	" 36 " 35	24
" 70 " 69	31	" 54 " 53	32	" 35 " 34	21
" 69 " 68	30	" 53 " 52	20	" 34 " 33	19
		" 52 " 51	22	" 33 " 32	21
		" 51 " 50	23	" 32 " 31	20
		" 50 " 49	22	" 31 " 30	21
				" 30 " 29	20
				" 29 " 28	21
				" 28 " 27	21
				" 27 " 26	20
				" 26 " 25	22
				" 25 " 24	21
				" 24 " 23	23

The foregoing tabulation may serve to saliently illustrate the decreased capacity due to increase of hydraulic gradient.

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Mr. Kuichling gives fresh water sponge as one of the principal causes of the present decreased delivery of the conduit. I do not agree with this. In February, 1888, in a paper on the Micro-organisms in Hemlock water, which I read before the Rochester Academy of Science, I called attention to the fact that at that time the spicules of fresh water sponge were present in every filtering. In the following months of May and June, Mt. Hope Reservoir was drawn off for repairs and an examination there revealed some considerable amount of sponge growing attached to the masonry in the outlet well. This was removed and the walls scraped clean. The bottom and sides of the reservoir and end of the inlet pipe were also examined at the same time without giving any further indication of the presence of sponge. In July and August of that year, Rush Reservoir was also entirely empty, and on examination no sponge was found. A similar examination was made about the same time, by raking the sides of the masonry of the well at Hemlock Lake. Scrapings from the screens at Hemlock Lake and the reservoirs were also examined microscopically several times during the summer and fall of 1888 and the following winter. In no case was any further indication other than occasional sponge spicules found, and inasmuch as the amount of sponge discovered in the outlet well at Mt. Hope reservoir in the spring of 1888 was probably enough to account for the spicules seen the previous winter, it was concluded that the cleaning of the masonry at Mt. Hope had removed their chief source. Since 1889 a number of microscopical examinations have been made, especially during last winter, without discovering more than an occasional sponge spicule. I therefore venture the opinion that fresh water sponge is not the chief cause of the trouble.

Is it not possible that what Mr. Kuichling really saw in the Rush Reservoir stop gate on September 19th, 1890, was not fresh water sponge, but fresh water polyzoa? The statoblasts of a number of species of polyzoa have been plentiful in the Hemlock water in the winter season for a number of years and it has always been something of a mystery as to just where they came from. Much searching at Hemlock Lake has thus far utterly failed to reveal them.

The substance stated to have been growing in the cast iron pipes, I have been unable to find, and the difficulty in the Hemlock Lake conduit is as shown entirely in the wrought iron pipes. We can hardly accept the sponge theory, therefore, unless it be proven that the sponge, or whatever other organism it may be, has a power of selection and prefers the wrought iron pipe to the cast iron.

It was found during the fall of 1890 that when an accurate pressure gauge at Mount Hope Reservoir registered 18 pounds and Rush Reservoir had a depth of from 14 to 16 feet, the flow from Rush to Mount Hope was just about equal to the inflow from Rush to Hemlock Lake; that is

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to say, with 14 to 16 feet of water at Rush and the pressure gauge at Mount Hope showing 18 pounds, the delivery from Rush to Mount Hope was about 6 700 000 gallons daily, Mount Hope being also at from 14 to 15 feet. We have then the 24-inch cast iron line between Rush and Mount Hope Reservoirs delivering 6 700 000 gallons daily, the same as due to an elevation of 35 feet above water surface at Mount Hope. This is shown in Fig. 2, where the data applying are compiled. The pressure gauge in use at that time was set about 6.5 feet below the mean water surface at Mount Hope, and 18 pounds indicated pressure gives a total of 41.5 feet or 35 feet above Mount Hope water surface. The length of 24-inch cast iron main from the outlet at Rush to the point of inflow at Mount Hope is 46 840 linear feet, which gives, with a total loss of head of 82.4 feet for the delivery of 6 700 000 gallons, a hydraulic gradient per 1 000 feet equal to 1.76.

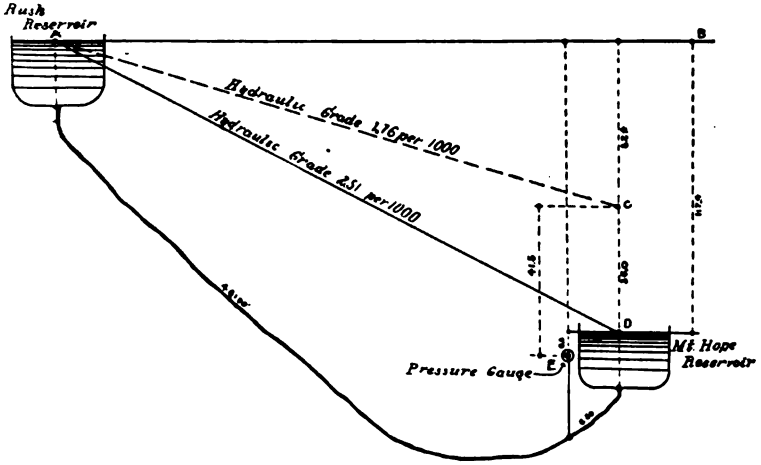

FIG. 2.

Substantially the same rate of grade also existed during the summer of 1891, as may be seen by the following table. It is fair to say, however, that a new self-recording gauge has also been set up at Mount Hope, and I believe it is about half a foot higher than the old gauge, or say 6 feet below normal water surface. Inasmuch as I have not got the detail of its location, I am obliged to base the argument on the known location of the old gauge.

Table No. 6 shows that with 17 pounds pressure at Mount Hope, Rush Reservoir slowly fell; while, with 20 pounds pressure, Rush slowly rose, and the movement is such, even after making the necessary allowances for evaporation, etc., as to indicate that the point of equilibrium is somewhat nearer 20 pounds than 17. Taking 18 as a safe figure, we obtain the rate of 1.76, as already indicated.

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TABLE No. 6.—GIVING DAILY DEPTHS OF WATER AT MOUNT HOPE AND RUSH RESERVOIRS, AND THE CORRESPONDING INDICATIONS OF THE PRESSURE GAUGE AT MOUNT HOPE, FROM AUGUST 1ST TO SEPTEMBER 8TH, 1891.

Date.	Mount Hope, 7 A.M.	Pressure Gauge. Pounds.	Rush. 7 A.M.	Date.	Mount Hope. 7 A.M.	Pressure gauge. Pounds.	Rush. 7 A.M.
August 1..	13.33	20	14.74	21..	14.50	17	14.48
(Sunday) 2..	13.75	20	14.72	22..	14.33	17	14.46
3..	14.50	20	14.69	(Sunday) 23..	14.50	17	14.45
4..	13.08	.....	15.26*	24..	15.23	17	14.41
5..	13.29	16	15.19	25..	15.12	20	14.42
6..	13.58	16	15.14	26..	15.00	20	14.44
7..	13.71	16	15.08	27..	14.83	20	14.48
8..	13.79	16	15.05	28..	14.58	20	14.52
(Sunday) 9..	13.91	16	15.00	29..	14.54	20	14.52
10..	14.58	16	14.97	(Sunday) 30..	14.58	20	14.55
11..	14.37	16	14.91	31..	15.25	20	14.52
12..	14.16	17	14.84	Sept. 1..	15.16	20	14.53
13..	14.21	17	14.79	2..	15.25	20	14.54
14..	12.92	.....	15.22*	3..	15.08	22	14.60
15..	13.50	17	14.93	4..	14.92	22	14.71
(Sunday) 16..	13.92	17	14.80	5..	14.75	22	14.77
17..	14.62	17	14.75	6..	14.00	22	14.88
18..	14.54	17	14.68	7..	15.00	22	14.95
19..	14.50	17	14.60	8..	15.08	22	15.00
20..	14.50	17	14.55				

* Mount Hope gates shut part of the day.

This gradient of 1.76 per 1 000 feet in the 24-inch cast iron pipe between Rush and Mount Hope being nearly the same as that found in the same kind and size of pipe between Hemlock Lake and Rush, emphasizes the view that the difficulty between Hemlock Lake and Rush is chiefly in the wrought iron pipe rather than in the cast iron.

The use of mercury gauges has been suggested, and I admit that with abundant leisure, observations could be taken more accurately with them, than with ordinary metallic spring gauges. The use of the mercury gauge, however, is not altogether a simple matter, as scrupulous attention to a number of corrections becomes absolutely imperative in order to obtain results which are of any value at all; while with metallic gauges an observer who properly understands the limitations of his tool can quickly reach results which, although not accurate to the third and fourth decimal place, are still sufficiently accurate for most practical purposes.

In further discussing the subject matter of the papers, I give from official records Tables Nos. 7 and 8:

Table No. 7 is derived by use of the record of the pressure gauge at Mount Hope Reservoir from Table No. 8, which was computed by Mr. Nichols. The location of the pressure gauge and its relations to the reservoirs are shown in Fig. 3.

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TABLE No. 7.—SHOWING THE AVERAGE DAILY USE OF WATER IN THE CITY OF ROCHESTER, FROM APRIL 1ST, 1876, TO APRIL 1ST, 1889.

TIME—April 1st to April 1st.	Total Use of Water for Each Year, in Gallons.	Average Use per Day, in Gallons.
1876-'77	813 202 600	2 227 953
1877-'78	1 052 530 300	2 883 650
1878-'79	1 159 687 250	3 177 225
1879-'80	1 409 186 850	3 860 785
1880-'81	1 584 513 942	4 341 134
1881-'82	1 546 898 500	4 238 078
1882-'83	1 653 056 800	4 531 662
1883-'84	1 763 054 100	4 819 809
1884-'85	1 950 392 200	5 343 540
1885-'86	1 881 019 200	5 153 426
1886-'87	2 107 309 000	5 773 695
1887-'88	2 207 928 800	6 032 565
1888-'89	2 299 663 300	6 300 447

TABLE No. 8.—SHOWING THE APPROXIMATE AMOUNT OF WATER PASSING INTO MOUNT HOPE RESERVOIR FROM RUSH AS INDICATED BY THE PRESSURE GAUGE AT MOUNT HOPE.

Indicated Pressure in Pounds.	Gallons per day as per Table in An. Rept. for 1876.	Gallons per day as per Table used at Mount Hope Reservoir.	Indicated Pressure in Pounds.	Gallons per day as per Table in An. Rept. for 1876.	Gallons per day as per Table used at Mount Hope Reservoir.
53	0	0	26	5 278 421	5 246 800
52	1 015 840	1 009 750	25	5 375 290	5 343 000
51	1 436 601	1 428 000	24	5 470 550	5 437 000
50	1 759 478	1 749 000	23	5 563 944	5 530 000
49	2 031 679	2 019 600	22	5 655 917	5 622 000
48	2 271 470	2 257 900	21	5 746 421	5 712 000
47	2 488 277	2 473 400	20	5 835 528	5 800 600
46	2 687 645	2 671 500	19	5 923 816	5 887 800
45	2 873 218	2 856 000	18	6 009 754	5 973 800
44	3 047 519	3 029 000	17	*	6 058 500
43	3 212 252	3 193 100	16	*	6 142 100
42	3 369 189	3 349 000	15	*	6 224 500
41	3 518 957	3 497 900	14	*	6 298 600
40	3 662 640	3 640 700	13	*	6 386 200
39	3 800 894	3 778 100	12	*	*
38	3 937 032	3 910 700	11	*	*
37	4 063 359	4 039 000	10	*	*
36	4 180 384	4 163 300	9	*	*
35	4 309 819	4 283 000	8	*	6 773 600
34	4 439 914	4 401 000	7	*	*
33	4 542 941	4 515 700	6	*	*
32	4 635 131	4 627 300	5	*	*
31	4 764 672	4 736 000	4	*	*
30	4 821 837	4 842 600	3	*	7 174 000
29	4 976 496	4 946 700			
28	5 079 197	5 048 800			
27	5 179 766	5 148 700			

* Not given in the record.

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Table No. 8 was probably either computed with the formula $V = 100 \sqrt{RS}$, or by a slight modification thereof. As it has been shown that at the present time the 24-inch cast iron conduit between Rush and Mount Hope reservoirs delivers 6 700 000 gallons a day into Mount Hope, with a mean hydraulic gradient of 1.76 per 1 000 feet, it follows that at full performance this section of the conduit delivers considerably more. A discharge of 6 700 000 gallons a day gives for a 24-inch pipe $Qs_1 = 10.367$ cubic feet, and $Vs_1 = 3.300$ linear feet.

Taking a gradient of 1.76 per 1 000, we obtain for a delivery of 6 700 000 gallons from the formula $V = C\sqrt{RS}$ a value of $C_1 = 111$, and for full performance we have $S_1 = 0.00251$. With these values of S and V we get $V_1 = 111.0\sqrt{0.5 \times 0.00251} = 3.94$, and for the daily discharge 7 999 500 gallons. In other words, if 6 700 000 gallons pass per day, when the Mount Hope pressure gauge shows 18 pounds, the full capacity between Rush and Mount Hope is practically 8 000 000 gallons per day.

In the same way, taking the daily discharge for 18 pounds pressure at 7 000 000 gallons as per Mr. Kuichling, and we obtain a value of $C_2 = 116$, with a daily discharge at full performance of 8 380 000 gallons.

Again, a daily delivery at full performance of 9 292 800 gallons gives a value for $C_3 = 130.2$.

It is clear from the foregoing discussion that the statistics of daily use of water as derived from a tabulation computed on the assumption of full performance of this conduit of 7 174 000 gallons daily are considerably in error.

If we compare the values of v and Q_d for the several values 111.... 116....130.2, for various readings of the pressure gauge, we have the following tabulation:

TABLE No. 9.

Pressure gauge. Pounds.	Effective Head. Feet.	$v = c\sqrt{RS}$.				$c_3 = 130.2$
		$c_1 = 111.0$		$c_2 = 116.0$		
		(v)	Qd_1 in gal.	(v)	Qd_2 in gal.	Qd_3 in gal.
53	.....	.....	.....	.....	.....	.....
40	32.0	2 057	4 177 400	2 149	4 361 100	4 707 000
30	53.1	2 700	5 481 900	2 821	5 727 500	6 276 000
20	78.2	3 215	6 512 500	3 361	6 823 900	7 524 000
10	101.3	3 695	7 502 000	3 828	7 772 100	8 589 000
3	.....	.....	.....	.....	.....	9 266 000

As intimated in my paper "On the Hydraulics," etc., I believe my measurements of flow into Rush Reservoir are 2 or 3 per cent. in error, and on reviewing all the circumstances I conclude they may be about

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Plate XV

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that amount too small. Mr. Kuichling's, I am equally satisfied, are somewhat too large, and the truth, therefore, lies between the two.

By way of briefly illustrating the matter graphically we may refer to Plate XV, where the discharge curves for the several different suppositions are shown, the curves *Aa* indicating the discharge for full performance of 7 000 000 gallons daily, as per Mr. Kuichling, while *Ab* is for 6 700 000 gallons per day. The mid-line, *Ac*, which probably represents the real discharge within a small limit of possible error, indicates for full performance a daily delivery of 8 200 000 gallons.

It is further clear that the discharge curves of Plate XV, furnish a convenient method of determining the true value of the quantities tabulated in Table No. 7. By its use we obtain the column "Total average daily use in gallons," in Table No. 10. It will be noted that the daily use for the municipal year, 1888 to 1889, is given as 7 250 000 gallons. The excess quantity is due to (1) draft on the storage, and (2) to increased delivery with low water at Rush Reservoir.

TABLE No. 10.

	Hemlock Lake Water.			Average daily use of Holley water per head of population, in gallons.	Total average daily use per head of population from both systems, in gallons.
Municipal year. April 1 to April 1.	Total average daily use, in gallons.	Population.	Average daily use per head of the population, in gallons.		
1876 to 1877	2 500 000	83 600*	30.5	.....	.....
1877 to 1878	3 280 000	85 150	38.9	.....	.....
1878 to 1879	3 675 000	86 700	42.4	.....	.....
1879 to 1880	4 510 000	88 430	51.0	.....	.....
1880 to 1881	5 040 000	90 930	55.4	.....	.....
1881 to 1882	4 925 000	93 100	53.0	.....	.....
1882 to 1883	5 260 000	96 650	55.0	12.6	67.6
1883 to 1884	5 590 000	99 850	56.0	12.8	68.8
1884 to 1885	6 160 000	104 200	59.1	12.4	71.5
1885 to 1886	5 990 000	108 500	55.2	11.2	66.4
1886 to 1887	6 660 000	113 900	58.5	11.9	70.4
1887 to 1888	6 930 000	119 700	57.9	12.2	70.1
1888 to 1889	7 250 000	126 500	57.3	12.5	69.8

*Official population in 1875, 81 722; in 1880, 89 366; in 1890, 133 896. No additions to territory.

During the year from April 1st, 1888, to April 1st, 1889, the storage at Rush was exhausted, and for three months, from about the middle of July to about the middle of October, Rush Reservoir was either entirely or nearly empty. During this period the discharge from Hemlock Lake to Rush Reservoir was that due to a head of, say, 143.8 + 16.0 = 159.8 feet. This head may be considered as giving, under the known conditions, a discharge of about 7 000 000 gallons per day.

At no time that year, after July, was there more than 8 feet of water at Rush, and for several months the quantity was less than this.

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The following table is of interest in the general discussion of the subject in hand:

TABLE No. 11.—SHOWING THE APPROXIMATE USE OF WATER BY HOURS ON THREE DIFFERENT DAYS IN 1890. AS DETERMINED BY OUTFLOW FROM MOUNT HOPE RESERVOIR.

Hour.	Saturday, July 5th, 1890 (12-hour measurement).		Sunday, August 10th, 1890, to 7 A.M. of Monday, August 11th (24-hour measurement).		Monday, August 25th, 1890, to 7 A.M. of Tuesday, August 26th (24-hour measurement).	
	Hourly outflow in gallons.	Outflow in six hours in gallons.	Hourly outflow in gallons.	Outflow in six hours in gallons.	Hourly outflow in gallons.	Outflow in six hours in gallons.
7 A.M. to 8 A.M.	330 563	.....	217 907	.....	392 041	
8 " 9 "	374 220	.....	260 911	.....	355 035	
9 " 10 "	394 238	.....	277 490	.....	389 125	
10 " 11 "	366 163	.....	285 393	.....	387 659	
11 " 12 M..	392 583	.....	250 098	.....	333 590	
12 M. " 1 P.M.	330 497	2 188 264	258 079	1 549 878	332 493	2 189 943
1 PM. " 2 "	373 238	.....	248 800	.....	348 811	
2 " 3 "	363 562	.....	239 670	.....	330 245	
3 " 4 "	345 789	.....	204 907	.....	277 078	
4 " 5 "	345 877	.....	161 936	.....	345 676	
5 " 6 "	341 381	.....	212 638	.....	292 693	
6 " 7 "	326 262	2 096 109	204 707	1 272 658	291 818	1 886 321
7 " 8 "	.....	.....	169 426	.....	205 479	
8 " 9 "	.....	.....	186 026	.....	143 816	
9 " 10 "	.....	.....	151 926	.....	170 626	
10 " 11 "	.....	.....	118 004	.....	170 326	
11 " 12 "	.....	.....	151 505	.....	102 063	
12 PM. " 1 A.M.	.....	.....	111 676	888 563	135 925	928 235
1 AM. " 2 "	.....	.....	67 222	.....	135 685	
2 " 3 "	.....	.....	134 146	.....	101 643	
3 " 4 "	.....	.....	150 757	.....	118 485	
4 " 5 "	.....	.....	100 326	.....	135 204	
5 " 6 "	.....	.....	217 105	.....	185 607	
6 " 7 "	.....	.....	266 463	936 019	286 080	962 704
Totals....	4 284 373	4 284 373	4 647 118	4 647 118	5 967 203	5 967 203

From Table No. 11 we derive—

TABLE No. 12.

TIME.	July 5th (Saturday). Gallons.	August 10th and 11th (Sunday and Sunday Night). Gallons.	August 25th and 26th (Monday and Monday Night). Gallons.
7 A. M. to 7 P. M.....	4 284 373	2 822 536	4 076 264
7 P. M. to 7 A. M.....	.....	1 824 582	1 890 939
Totals.....	.....	4 647 118	5 96 7 203

It appears from the foregoing that the purely domestic use of water, including leakage in services and the distribution system, as indicated

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by the Sunday measurement, was, while the restrictive measures were sharply in force last year, say, 4 650 000 gallons a day, amounting for a population of 134 000 to 34.7 gallons per head per day. The difference between the Sunday and Monday measurement further indicates that the use of water for manufacturing purposes was, with some allowannce for increased domestic use on Monday, perhaps 1 200 000 gallons per day. Mr. Tubbs, however, claims that the adoption and continuance of the restrictive measures has led to a reduction in the use for manufacturing purposes of at least 1 500 000 gallons per day.

When the Rochester Water-Works were first constructed a number of large consumers were supplied at a rate of 3½ cents per 1 000 gallons, or at a price, when the fixed charges are taken into account, of about one-third the actual cost per 1 000 gallons to the city. For conservative figures we will say that the amount of water sold in the last ten years is 1 000 000 gallons a day over what it would have been if the price in the beginning had been made cost plus a small profit. 1 000 000 gallons per day, at 3½ cents per 1 000 gallons, amounts to $12 775 per year. At 11 cents per 1 000 gallons, which is a trifle more than the actual cost, the yearly sum for 1 000 000 gallons a day becomes $40 150. The difference of the two is ($40 150 — $12 775) = $27 375; we therefore reach the result that in ten years the City of Rochester subsidized a few large consumers to the amount of $273 750. A couple of years ago the Executive Board placed the price at 11 cents per 1 000 gallons for quantities over 5 000 gallons per day, and a few establishments accordingly sought their own sources of supply as a measure of economy.

In regard to loss of revenue the following are the official figures: The total receipts of the Rochester Water Works during the year 1889 were $169 879.72; during 1890 they were $211 743.94; while in 1891 they were $251 576.21.

Mr. Thomson in his discussion states he is willing to go on the record as in favor of limiting the supply by pinching the valves carefully down "at some lower portion of the hydraulic gradient beyond the principal distributing area"; and this he says would be preferable to partially closing the gates at Mount Hope Reservoir. If he will read carefully what I have written on this point in the second paper, and further examine the map (Plate XIV), he will see that the thing which he states as preferable is precisely the thing done, and I am greatly obliged to him for thus strongly emphasizing the utility of the design. His view, that the necessary restrictions could have been obtained at all, in 1890, by so closing the supply gates at Mount Hope as to allow the feeding mains leading therefrom to run just full, is probably due to a lack of full information as to the detail. The center of the supply gates is 18.5 feet below the normal water surface of 15 feet above reservoir bottom; 8 pounds is therefore the maximum possible reduction at this point. It would, however, be desirable to leave a little

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pressure on the city side of the supply gates, in order to provide for the contingency of a sudden increase of draft. Making such an allowance and taking say 5 pounds as about the practicable limit of reduction of pressure, by closing the supply gates at Mt. Hope Reservoir, this could only be attained by keeping the depth of water in the reservoir at fully 15 feet. As stated in the paper, the general lowering of pressure actually attained was from 10 to 20 pounds.

As to the originality of the method of reduction actually applied, the claim is that the combination of the fire alarm gong, telephone and pressure gauge was a new and useful application of modern appliances to the specific problem in hand.

The actual results obtained in increasing the storage are shown by

TABLE No. 13.

Date.	Mt. Hope.		Rush.		Total Net Gain at both Reservoirs = (3) + (5), in Gallons.	Total Net Gain + the Evaporation, divided by 60, the number of days, 49 668 600 / 60 in Gallons.
	Depth in Res., in feet.	Corresponding Quantity of Water as Derived from Mr. Nichols' Table, in Gallons.	Depth in Res., in feet.	Corresponding Quantity of Water as Derived from Mr. Nichols' Table, in Gallons.		
Sept. 15 at 7 A. M.	15.50	23 383 000	16.35	62 741 700		
July 17 at 7 A. M.	10.82	15 372 800	6.95	24 733 300		
Gain.....	4.68	8 010 200	9.39	38 008 400	46 018 600	
	Evaporation at 0.2 inches per day over the mean area, =				3 650 000	
	Total net gain + the evaporation, =				49 668 600	827 810

A few words as to the probable causes of a smaller delivery in this conduit than theory indicates.

(1) It appears that the spigot end of the 36-inch pipe was made by a small fillet of half round iron. This has been found in practice altogether too light, the effect of sharply setting up the lead (as I have myself seen) being to kink the spigot end, with the result that the oakum and lead project into the interior of the pipe. The whole number of joints on the 36-inch pipe is about 1 000, and during the thirteen years ending December 31st, 1890, a total of 557 leaks have been repaired.

(2) If posts have been inserted to resist earth pressure on the wrought pipe, their presence, in connection with the high grades on the same sections, is strong evidence of a number of deformations where the pipes

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are covered deeply with earth, and such deformations would tend to cause increased resistances.

(3) When the deficiency in delivery was first definitely determined, in 1890, Mr. Tubbs was strongly of the opinion that the chief cause was floating obstructions, and extended inquiry among men employed upon the original construction indicated that probably some pieces of plank and timber had been left inside the pipe. If this view of the matter be entertained it may explain the irregular action at air valves 55, 56 and 57. It would also be a full explanation of the discordant piezometer observations at those valves. The piezometer measurements seem to indicate that floating obstructions are not the chief cause, though they may contribute to the difficulty.

(4) The relation of the hydraulic gradients in the two sections of wrought iron pipe indicates that as a whole the same cause has been operative in both pipes. From the theory of flow in conduits we derive that under normal conditions, with the same discharge, the rate of hydraulic gradient in a 24-inch pipe should be about 7.7 times what it is in a 36-inch pipe. An application of this principle has been made in the discussion of Mr. Gould, and it is unnecessary to elaborate in reference thereto in this place. The rate of about 3.50 as the approximate probable value for the 24-inch wrought pipe, divided by 0.45, the mean value for the 36-inch pipe, gives a ratio of 7.8, which by itself is, as Mr. Gould has pointed out, the very strongest confirmation of the correctness of my deductions from the observations. The first part of Mr. Hering's discussion also embraces the same view from a slightly different standpoint. I infer, therefore, that the original roughness of the interiors of the built up pipes, together with a probable tuberculation of the same, exercises a very material influence in increasing the retardation, and is one of the chief causes.

On this head it may be noted that wrought iron is *per se* a material more liable, in the absence of a proper protective coating, to corrosive action than cast iron.

(5) There may be some adhering plant and animal life, though this explanation involves serious difficulties, as already pointed out. In any case, I venture the opinion that plant and animal growths are not the chief cause of the difficulty.

(6) There may be some undiscovered leakage, though Messrs. Tubbs, Kuichling and myself all agree, I believe, that at the present time the leakage is nearly *nil*. For the last year and a half both line foremen have carried a waterphone and an iron probe on their daily inspections, using them wherever any indication of a leak could be found, and it appears difficult to assume any large amount of undiscovered leakage. Again, the soil through which the line is laid is, with the exception of the first mile and a half from Hemlock Lake, mostly clay, some of it very heavy, and it may be considered settled that leaks, when

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appearing at all, except on the mile and a half in question, which is quicksand and rock, show upon the surface. The piezometers show, however, that the first mile and a half has about the mean grade of the whole 36-inch section, thus effectually negativing the theory of excessive loss from leakage on this part.

The second part of Mr. Hering's discussion confirms this view. Thus he makes a slight allowance for departure from theoretical conditions, and falls 6 per cent. short of the original determination of 9 292 800 gallons. By repeating his computation with a value of n, appropriate to the known conditions of this conduit, he will obtain about 8 000 000 gallons as the original daily flow.

As the result of the discussion we may say:

(1) That the average rate of hydraulic gradient in the 36-inch pipe is in reality about 120 per cent. in excess of that for clean pipe, instead of 50 per cent., as stated in the first paper.

(2) That in the 24-inch wrought iron pipe the hydraulic gradient is from 120 to 135 per cent. in excess of the normal with the observed discharge.

(3) That in the cast iron mains of the Hemlock Lake conduit the hydraulic gradient is only from 10 per cent. to 34 per cent. in excess of the normal, with a mean value of the excess of about 22 per cent.

(4) From all the foregoing we may fairly conclude that the built up conduits, if used at all, must be constructed with the greatest care and with special attention to strict compliance with theoretical conditions.

(5) Experience in the Hemlock Lake conduit of the Rochester Water-Works indicates that as between cast iron and wrought iron, cast iron is on the whole the preferable material for water pipe lines, and that the legitimate sphere of use of built up pipes is only reached when, by reason either of large diameter or heavy pressure, or both, cast iron becomes inapplicable.

(6) Mr. Gould's point that it is unwise to put too much faith in hair splitting formulas is well taken, and we may go a step further and say that even a little allowance for future contingencies may be properly made.

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OF THE
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Edited by the Secretary, under the direction of the Committee on Publication.

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522.
(Vol. XXVI.—February and March, 1892.)

SOME DISPUTED POINTS IN RAILWAY BRIDGE DESIGNING.

By J. A. L. WADDELL, M. Am. Soc. C. E.

WITH DISCUSSION.

The object of this paper is to induce those members of the American Society of Civil Engineers who have made a special study of bridge superstructure to enter into an exhaustive discussion of a number of important points in bridge designing whereon authorities differ. The end to be attained by such discussion is the reconciliation of conflicting opinions, the simplification of bridge specifications and the consequent improvement of design. Such an object cannot, of course, be attained at once, but must be approached, so to speak, by an asymptotic curve.

That there is decided difference of opinion among experts as to what constitutes a good structure, and as to how a bridge should be proportioned, no one who has had much to do with bridge work will be disposed to deny. Some years ago there was hardly any uniformity whatsoever either in the detailing, or in the types and general outlines of bridges. To-day, however, by a process of elimination based

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upon the principle of the survival of the fittest, certain types of bridges for the different governing conditions have almost become standard, and the details of construction are gradually being reduced to a system.

For instance, in railroad bridges, of which structures, by the way, this paper will treat exclusively, experience has shown that plate girder spans should be used up to a limit of 90 or 100 feet, and that single intersection trusses with vertical intermediate and inclined end posts are preferable to other types of trusses. Nevertheless, certain well known engineers are still employing the Whipple truss with its double system of cancellation; and pony-truss pin-connected bridges are still being built for short spans. Again, in respect to detailing, certain antiquated and very objectionable practices, such as the use of suspended floor-beams with adjustable hangers, are, to a certain extent, still in vogue.

All this is sufficient reason for the opening of an extended discussion in which every one will be given an opportunity to put himself on record either in defense of peculiar personal practice or in objection to the practice of others; and it is the earnest wish of the writer that every prominent bridge specialist will take advantage of the opportunity offered, and thus do his share towards the attainment of a more satisfactory state of affairs in the specialty of bridge superstructure, and towards the placing of said specialty upon a still higher plane than that which it occupies to-day in the United States.

In order to systematize the contents of this paper it will be advisable to group as follows the disputed points, and to treat each group separately:

A. Live Loads.

B. Wind Pressure.

C. Styles and Proportions of Bridges.

D. Intensities of Working Stresses.

E. Combined Stresses.

F. Plate Girder Proportioning.

G. General Details of Construction.

In group *A* will be considered:

First.—The subject of Uniform Loads *vs*. Engine Concentrations, and *second*, The proper Live Loads for Modern Bridge Specifications.

Several years ago two or three American engineers evolved independently the theory of engine concentrations for the finding of stresses in

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bridge members. No doubt these gentlemen at that time considered, and perhaps even yet think, that they were conferring a great boon upon the engineering profession; but instead of this they were really hampering it with a burden grievous to be borne. That such is the case every professional bridge computer, whose business it is day after day, month after month, and year after year, to calculate the stresses in bridge trusses, will readily and sadly acknowledge. The plea that this method is more accurate than the old one involving uniform loads either with or without engine excesses at panel points, and that the calculations of stresses made thereby are just as simple and easy, is a fallacy. Of course for any particular engine and train, the former method will give slightly more accurate results than will the latter. But there are never two trains alike in respect to amount and distribution of load, for the engines on any railroad vary greatly in weight and wheel distribution; and the weights on the cars are extremely irregular. It must be acknowledged, therefore, that all assumed loads for computing stresses in bridges are merely typical; consequently when assuming a typical load, would it not be well to adopt one that will reduce the labor of the computer to a minimum, provided, of course, that it will give just as correct results as would one consisting of engine concentrations followed by a uniform car load?

That such is practicable the writer will show presently.

Now as to simplicity and ease of calculation: Even in the most favorable case, viz., that where the chords are parallel, the panels of equal length, the structure square, and the span comparatively short, it will take fully twice as much time to compute the stresses by concentrations as it will to find them by the old method. But when the span is long, the top chord broken, the panel lengths unequal or the structure on a skew, the amount of labor involved by the concentration method is simply discouraging. How many engineers are there who make or have made a practice of computing the exact stresses in Whipple trusses by engine concentrations?

Not content with burdening computers to the extent of at least doubling their labor by enforced calculations involving engine concentrations, certain writers of bridge specifications have "added insult to injury" by specifying that each structure must be designed throughout for two and even three fundamentally different live loads, each involving a peculiar type of engine.

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Let us for an instant consider the designing in competition of either a draw-span or a cantilever bridge according to one of these modern specifications and see what work there is involved. To begin with, there will be, say, six competing bridge companies, each employing for this work, on an average, two computers. Now, although these gentlemen may be firmly convinced that the requirements of the specifications in respect to live load are mere humbug and nonsense, and that they could obtain practically correct results with one-tenth of the amount of figuring by assuming equivalent uniformly distributed loads, still they dare not shorten their work in this way for fear that some captious critic in checking their papers might find a variation in the stresses of 1 or 2 per cent. ; consequently all twelve of them set to with more or less ill grace, to calculate the exact maximum and sometimes also the exact minimum stress for each member of the bridge. Now, it was discovered not long ago, that when the top chord is inclined to the horizontal, the maximum stresses do not occur with the same position of wheel loads as they would were the top chord horizontal. Hence it was sometimes necessary to make for each truss member three or four trials, each involving considerable figuring, before ascertaining the exact position of wheel load that would give absolutely the greatest stress.

Since then, however, the true theory of maximum stresses for engine concentrations in cases of inclined top chords has been worked up by one of our clever mathematicians, consequently it will no longer be necessary to use the "cut and try" method; but merely to employ the following simple little equation:

$$\text{Shear} = \left[\frac{M_r d}{l} - \frac{M_x (d + np)}{p}\right] \frac{1}{d + (n+1)p}$$

after having found, by trial, which wheel load should be over the panel point under consideration, by means of another pretty little equation, viz.:

$$\frac{P_n + wy_n}{N} \gtrless P_x \frac{d + np}{d}$$

It must be borne in mind, too, that d has to be either calculated or scaled for each panel point, and that M_r will have to be computed every time that it is used, unless—which is highly improbable—a wheel load will come directly over the end of the span.

After having worked out all the maximum stresses by this method,

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our twelve computers have accomplished only one-third of their task; for they must twice more attack the same problem but with different wheel concentrations.

After finding all of the live load stresses in the manner called for by the specifications, the remainder of the computations for the six designs will be of a comparatively simple character; so let us suppose that all the papers of the six competing companies have been handed in. One company only can secure the contract; consequently the work of at least ten men has been entirely wasted; and in fact there is often a strong probability of the work of all twelve being useless or at least unrewarded; for some other bridge company may have "an inside track" and build the bridge upon the set of papers which appears to be most satisfactory to the parties purchasing; or said parties may conclude that the proposed bridge is too expensive, and that they will build either a combination or a wooden structure. How much better off all six companies would have been, had the calculations been made for an equivalent uniformly distributed live load, either with or without engine excesses, by the quick and elegant graphic method!

If one considers that the proper intensities of working stresses are to a great extent a matter of opinion, and that engineers often differ thereon as much as 20 per cent., he ought to be very quickly convinced that an endeavor to obtain stresses with extreme accuracy is nothing more nor less than hair splitting. But the best possible argument that any one can advance against the use of engine diagrams in computing bridge stresses is a simple reference to Professor Eddy's elaborate paper entitled "A New Graphical Solution of the Problem: What Position a Train of Concentrated Loads must have in order to cause the Greatest Stresses in any given part of a Bridge Truss or Girder," published in the *Transactions* of the American Society of Civil Engineers for May, 1890. Ye gods! what a mass of complicated mathematical formulas that paper contains, and all to settle such a trivial little point! It needed not such an elaborate investigation as this to attest to Professor Eddy's great mathematical ability, consequently it seems a pity to have wasted so much valuable time. However, it may not prove to be time wasted after all; for Professor Eddy's paper may be the means of proving to the profession that in computing stresses by engine diagrams and proportioning parts by assumed intensities which do not provide adequately for the effect of impact, they have been "straining at a gnat and swallowing a camel."

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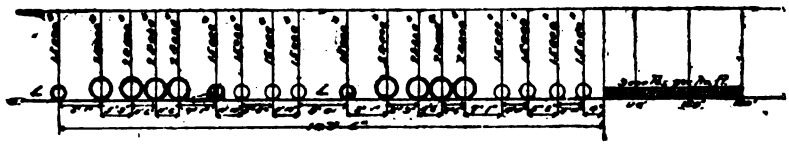
In order to demonstrate the writer's assertion that practically exact stresses can be found by the assumption of equivalent uniformly distributed loads, let us take as a standard for comparison Mr. Theodore Cooper's engine diagram denominated "Class A," viz.:



as it is undoubtedly the diagram that has been most widely used since calculations by means of such diagrams came into vogue. By its use have been figured the bending moments and shears in the following tables.

Those for stringers and plate girders have been computed for span lengths varying by 2½ feet up to a length of 25 feet, by 5 feet between the limits of 25 and 60 feet, and by 10 feet between 60 and 100 feet. In the case of truss spans, the smallest length has been taken equal to 100 feet, and the greatest equal to 300 feet, the intermediate spans varying in length by 50 feet; all of which is merely for convenience. For the same reason a single panel length (25 feet) has been adopted for all cases, and the chords have been assumed to be parallel and the trusses to be of the simple Pratt type. Although these conditions would not all be realized in modern practice, still the assumptions cannot in any way vitiate the results of the investigation.

It will be observed that there are but two cases of loading to be used in the proposed simple method of making computations, viz., equivalent uniformly distributed loads for the chords and webs of all girders and trusses, and an additional load for testing the shearing resistance of the webs of plate girders at the ends of the spans. In reality the last load will be found to be needed only in cases of very shallow girders, consequently we may say that the proposed method for all cases of ordinary practice reduces to the use of equivalent uniformly distributed live loads. That these equivalent loads can be used in finding the reactions or concentrations over floor-beams, the writer has just ascertained while preparing a summary of the results of the calculations that were made in compiling the tables in this paper.

The rule for finding a floor beam concentration is to multiply the equivalent uniformly distributed live load per lineal foot for a span of

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two panel lengths by the length of one panel. If said equivalent load were determined for the mid span bending moment, the result will be mathematically correct; but if it were determined for the absolutely greatest bending moment at any part of the span, the result will give a very small error on the side of safety.

That the principle involved was unknown to the engineering profession only two years since, is evident from a perusal of Mr. Theodore Cooper's paper on "American Railroad Bridges," published in the July, 1889, number of the *Transactions* of the American Society of Civil Engineers; for on page 27, he says: "To represent the action of a train load properly by this method, we should need four different series of equivalent loads, all varying for each span.

"The apparent simplicity, therefore, of using equivalent uniform loads for proportioning our structures is a fallacious one; when applied to partial loads it becomes far more confusing and untrustworthy.

"The old method of computing strains by use of panel loads and equivalent uniform loads (and too often using only the one derived from the moments), has gradually given way to a system much simpler, more accurate, and also as easy of application."

Again, referring to the table at the top of page 26 of the same paper, drawing lines from the loads in the "Double Shear" column to the loads for twice their span lengths in the "Moment" column, and allowing for slight inaccuracies of calculation, or for the fact that the absolute maximum moments rather than moments at mid span were adopted in computing, what do we find? Not a confirmation of the mathematical theory herein given, for mathematical work needs no confirmation; but a sure proof that not one of us at that time knew the existence of the principle that will be demonstrated presently.

Referring to the quotation from Mr. Cooper's paper, it can be concluded that that gentleman is not only strongly in favor of the continued use of engine diagrams, but also that he is just as strongly opposed to the use of equivalent uniform loads. Consequently it is fair to presume that, after reading what precedes and follows this, he will give the matter a thorough discussion.

The following is a mathematical proof of the rule previously stated, and it is so simple that the writer is amazed that the principle was not discovered long ago:

Let P be any load, L the double panel length, x the distance from the nearer end of the span to the point of application of P, R the reaction or concentration over floor beam, and w the equivalent uniformly

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distributed load per lineal foot of span that will produce at the middle of the span the same bending moment as that due to P.

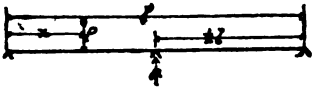

FIG. 1.

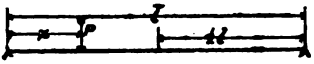

FIG. 2.

In Fig. 1 we have $R = \frac{2\,Px}{l}$ ....Eq. 1.

In Fig. 2 we have the moment at mid span $M = \frac{Px}{2}$

Equating this to the moment at the same point caused by the equivalent uniformly distributed load, we have

$$\frac{Px}{2} = \frac{1}{8}\,w\,l^2, \therefore\ w = \frac{4\,Px}{l^2}$$

Now, multiplying w by the panel length, we have

$$\frac{1}{2}\,wl = \frac{l}{2}\cdot\frac{4\,Px}{l^2} = \frac{2\,Px}{l} \text{.....Eq. 2.}$$

Comparing equations 1 and 2, we have $R = \frac{1}{2}\,wl$.

Now, as this is true for any single load, it therefore holds good for any summation of loads, consequently the statement is proved.

To the writer's assistants, Mr. A. C. Stites, Assoc. Mem. Am. Soc. C.E., and Mr. Lee Treadwell, Jun. Am. Soc. C. E., is due the credit for making the numerical calculations before referred to, and upon which the treatment of this whole subject of equivalent loads *vs.* engine concentrations is based. These gentlemen figured independently, being some 300 miles apart, then compared results; consequently said results can be accepted as reliable.

EQUIVALENT UNIFORMLY DISTRIBUTED LIVE LOADS FOR PLATE GIRDER SPANS, COMPUTED FOR GREATEST BENDING MOMENT AT ANY POINT OF SPAN.

Span.	Equivalent Uniform Load.	Span.	Equivalent Uniform Load.
15 feet.	5 760 pounds.	45 feet.	3 995 pounds.
17½ "	5 407 "	50 "	3 860 "
20 "	5 040 "	55 "	3 726 "
22½ "	4 993 "	60 "	3 652 "
25 "	4 804 "	70 "	3 490 "
30 "	4 573 "	80 "	3 379 "
35 "	4 327 "	90 "	3 310 "
40 "	4 164 "	100 "	3 226 "

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It is not necessary to prove here that such a table as the foregoing can be used legitimately in designing plate girder spans; for the reason that for five years past bridge computers have been using Thacher's tables of equivalent uniformly distributed live loads in plate girder designing, even if they have not been able to use them in computing stresses in trusses.

It may be possible that if the parabola of moments due to the equivalent uniform load, and a number of moments from concentrations at intermediate points, were platted on the same sheet, some of the latter would pass a trifle outside of the curve; but if so, this would cut no figure, because the maximum bending moment is in most cases the quantity sought for in proportioning such structures; and in determining lengths for cover plates, a good designer will always add 2 or 3 feet to the length required by theory. The moment parabola referred to affords a very expeditious method of ascertaining the proper lengths of cover plates.

If we plot on cross-section paper the loads given in the table, and draw, as nearly as is practicable, through the points indicating the loads, a shapely curve, we will find that one or two of the points will not lie therein. This is due to some peculiar relation between the wheel spacing and the span length, and would not occur for the same span with the slightly different wheel spacing of some other similar locomotive; consequently it would be proper, in preparing tables of equivalent uniformly distributed loads for actual use, to ignore such variations from uniformity and fill out the table from the ordinates to the curve.

A strong point that can be raised in favor of using equivalent uniformly distributed loads for finding bending moments in plate girder spans is that the ordinary engine diagram cannot be employed, for the reason that the greatest bending will nearly always occur when some of the wheels at the head of the train have passed off the span. In consequence one would either need to have at hand a moment diagram with each wheel leading in turn for each standard loading, or would have to discard all diagrams and make a special investigation of moments for each case as it arises, determining each time by experience or by trial which of several wheels placed at mid span will give the greatest moment. It is the latter method that the writer has used for years when forced into figuring by the concentration method because of competition.

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As we have been dealing thus far with plate girder spans exclusively, we will now treat of the end shears on these before passing to trusses. Investigation shows that the amount to be added to the half of the total uniform load on any span in order to equal approximately the end shear is given by the equation—

$$W = A + BL$$

where W is the additional load, L the span length, and A and B are constants that will differ for different engine and car loads.

For the engines used in these calculations $A = 8\,000$ and $B = 100$.

In the following table are given the results of the calculations:

Span = S.	Uniform Load = W.	Half Load = $\frac{WS}{2}$.	Additional Load by Formula.	Summation.	Calculated End Shear.	Percentage of Variation.
15-foot	5 760	43 200	9 500	52 700	50 880	3.5 Safety.
17½ "	5 407	47 311	9 750	57 061	57 325	0.5 Danger.
20 "	5 040	50 400	10 000	60 400	62 160	2.8 Danger.
22½ "	4 993	56 177	10 250	66 427	65 920	0.8 Safety.
25 "	4 804	60 050	10 500	70 550	70 248	0.4 Safety.
30 "	4 573	68 595	11 000	79 595	77 427	2.8 Safety.
35 "	4 327	75 723	11 500	87 223	85 402	2.1 Safety.
40 "	4 164	83 280	12 000	95 280	93 129	2.3 Safety.
45 "	3 995	89 887	12 500	102 387	100 114	2.3 Safety.
50 "	3 860	96 500	13 000	109 500	106 843	2.5 Safety.
55 "	3 726	102 465	13 500	115 965	113 295	2.4 Safety.
60 "	3 654	109 620	14 000	123 620	119 930	3.1 Safety.
70 "	3 490	122 150	15 000	137 150	136 310	0.6 Safety.
80 "	3 379	135 160	16 000	151 160	153 307	1.4 Danger.
90 "	3 310	148 950	17 000	165 950	169 685	2.2 Danger.
100 "	3 226	161 300	18 000	179 300	185 622	3.4 Danger.

By consulting the last column it will be seen that the formula assumed will give in most cases a small error on the side of safety. It is really only in the spans exceeding 70 feet that the error is on the danger side; for the value of W in the 20-foot span would in practice be increased by the adoption of a uniform curve of loads, as before mentioned.

Moreover the end shear on plate girder spans is by no means an important factor in designing, for two reasons, viz.:

First.—There is nearly always an excess of section in the web of a well proportioned plate girder, and

Second.—The intensity of shearing stress is always taken very low, being 4 000 pounds for iron and 5 000 pounds for steel.

The main object in inserting the last table is to show that it is practicable to establish, for any system of live loads, a simple formula for

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determining readily within a small limit of error the end shears on plate girder spans.

Passing now to truss spans, the following table gives the equivalent uniformly distributed load at each panel point of each span considered, which will produce the same bending moment as will the loads for the engine diagram. The averages are for the loads at all the panel points of a span, and not the direct averages of those given in the table.

Panel Point.	100-foot Span.	150-foot Span.	200-foot Span.	250-foot Span.	300-foot Span.
No. 1.....	3 383	3 283	3 220	3 179	3 151
" 2.....	3 157	3 145	3 120	3 098	3 083
" 3.....	....	3 207	3 137	3 116	3 094
" 4.....	....	....	3 106	3 084	3 068
" 5.....	....	....	....	3 056	3 047
" 6.....	....	....	....	....	3 043
Average.....	3 306	3 215	3 151	3 112	3 085
Greatest Safety Error by using the Average.....	4.77%	2.23%	1.45%	1.83%	1.38%
Greatest Danger Error by using the Average	2.21%	2.07%	2.14%	2.10%	2.09%

It will be observed that the greatest error on the side of danger in any span is very little over 2 per cent., and that with the exception of the 100-foot span the greatest error on the side of safety is a little less than 2 per cent. The fact that the uniform load gives in general a small error on the side of safety for the chord members, except at the ends of span, is a good feature; because by placing cars ahead of the locomotives, greater chord stresses can often be found than those obtained by the use of the ordinary engine diagram.

As for the 2 per cent. error on the side of danger at the ends of span, it will cut but little figure even if it really affect the sections, which it generally will not, because in single track bridges the two panels of bottom chord at each end of span, being stiffened for wind pressure, have always a slight excess of metal; and the inclined end posts, being proportioned for bending, have a great excess of metal, as far as the direct stress is concerned.

Adopting for the web members the same uniform load as the average determined in the last table for the chords, and computing the web

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stresses therefrom, also those for the same members, by means of the engine diagram we will obtain the results given in the following table:

Span.	Member.	Shear in Pounds.		Percentage of Error.
		By Diagram.	By Uniform Load.	
100-foot ..	End Post	126 881	124 050	2.23 Danger.
	First M. Diagonal.....	59 429	62 025	4.40 Safety.
	First Counter	17 247	20 675	19.9 Safety.
150-foot ..	End Post	205 285	200 745	2.21 Danger.
	First M. Diagonal.....	134 585	133 830	0.56 Danger.
	Second M. Diagonal...	78 142	80 298	2.76 Safety.
	First Counter	36 576	40 149	9.77 Safety.
200-foot...	End Post	281 874	275 716	2.18 Danger.
	First M. Diagonal.....	209 992	206 787	1.53 Danger.
	Second M. Diagonal...	148 784	147 705	0.72 Danger.
	Third M. Diagonal....	97 484	98 470	1.01 Safety.
	First Counter	55 768	59 082	5.94 Safety.
	Second Counter	26 220	29 541	12.66 Safety.
250-foot...	End Post	357 668	349 875	2.18 Danger.
	First M. Diagonal	285 278	279 900	1.88 Danger.
	Second M. Diagonal...	220 844	217 700	1.42 Danger.
	Third M. Diagonal....	164 804	163 275	0.93 Danger.
	Fourth M. Diagonal...	116 264	116 625	0.31 Safety.
	First Counter.....	75 887	77 750	2.45 Safety.
	Second Counter.....	43 645	46 650	6.85 Safety.
300-foot...	End Post	433 260	424 182	2.10 Danger.
	First M. Diagonal.....	360 438	353 485	1.93 Danger.
	Second M. Diagonal...	293 893	289 215	1.59 Danger.
	Third M. Diagonal....	234 684	231 372	1.41 Danger.
	Fourth M. Diagonal...	181 734	179 956	0.97 Danger.
	Fifth M. Diagonal.....	135 034	134 967	0.05 Danger.
	First Counter.....	95 206	96 405	1.26 Safety.
	Second Counter	62 431	64 270	2.95 Safety.

The results of this investigation cannot be considered anything but satisfactory; for the "Danger" percentages of error are all small (never exceeding 2¼ per cent.) and affect the heavier members, while the "Safety" percentages of error affect principally the counters. Setting aside the consideration of the inclined end posts for the reason mentioned previously, we find that the uniform load stresses for main diagonals give a very close coincidence indeed, the variation being hardly worth mentioning. As for the counter stresses, an error on the side of safety is a very good thing, because of both impact and oftentimes adjustment. Moreover, the method of proportioning counters is very variable, some computers deducting the dead load stress of the panel, and others not deducting it; while every conscientious designer always adds something

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to the section required for small counters in order to provide for uncertainties.

In concluding this subject of Concentrated *vs.* Uniform Loads, the writer wishes to urge each engineer who discusses this paper to give his opinion concerning the advisability of continuing to use the present laborious method of computing stresses in railway bridges, or of abandoning it and adopting instead tables of equivalent uniformly distributed loads for certain standard, locomotive and car loads, to be adopted by the profession, similar to those hereinafter indicated. It is only by an array of opinion of authorities, that railroad engineers can be induced to depart from established custom to such an exent as to specify uniform loads when calling for bids on their proposed bridges; and it is, undoubtedly, these gentlemen who keep up the fashion in bridge specifications after that fashion has once been set by specialists.

Taking up now the second division of "Group A," viz., "The Proper Live Loads for Modern Bridge Specifications," the writer would suggest the following (see Plate XVI):

Class Z of this series corresponds practically to Cooper's "Class A," and the Class U to his "Lehigh Heavy Grade Engines." It will be observed, however, that there are but two wheel spacings employed, and that these are in even feet. This is merely for the sake of simplifying calculations; and any one who has for years struggled with Cooper's spacing of feet and odd inches when reduced to decimals of a foot, will agree with the writer that the change is an improvement.

If the general sentiment of the profession prove to be in favor of the preceding standard loads, the writer will see that all of the necessary "equivalents" in accordance with his proposed system shall be calculated, tabulated and presented to the American Society of Civil Engineers within a reasonable time. The regularity of increase of loads for the various proposed classes can be seen by inspection, so if in the future heavier loads than the "Lehigh Heavy Grade Engine," with its car load of 4 000 pounds per foot be employed, it will be a simple matter to extend the series.

Cooper's specifications provide alternative engine loads consisting of 100 000 pounds equally distributed on two pairs of drivers, 7 feet center to center, for his heaviest load, and reduced proportionately for his lighter loads. As these must be supposed to act without any other live

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load, it is evident that they can affect only certain bridge members for very short panels. But as the tendency of modern bridge designing is towards the use of longer and longer panels, it is evident that these alternative loads are unnecessary, unless it be for the wooden ties, in the proportioning of which judgment rather than figuring, ought to govern. If we were to compare the moments found by Mr. Cooper's alternative live load for Class A with those obtained from our previously found equivalent uniformly distributed loads *after their adjustment by the curve*, we would see that there is only one case where the former would exceed the latter, and that by only 1.4 per cent., hence it is reasonable to conclude that, for spans and panels exceeding 15 feet, the alternative load is unnecessary.

Passing to "Group B," which relates to "Wind Pressure," we will divide the subject into two parts, viz., "Amounts" and "Effects." In respect to the former the practice has of late years changed somewhat, the tendency being to reduce the intensities specified and to provide more thoroughly for the effects. For instance, many of the older specifications called for a pressure of 50 pounds to the square foot of surface on both sides of the span, when the bridge is empty, while in fact nearly every structure designed by said specifications would double up like a jack-knife under such a pressure. In most of the modern specifications, instead of stating the amount of wind pressure per square foot of surface, the amount per lineal foot for each chord is given, that for the loaded chord being 450 pounds, and that for the unloaded chord 150 pounds. For short spans this method is all right, in that it prevents the use of too light lateral rods and struts, but for long spans the amounts are too small. The writer has verified this statement a number of times lately in the designing of unusually long spans, more especially where the structures have to carry both railway and highway traffic. In one case, for a span of 500 feet, the total pressure figured for an intensity of 30 pounds, ran as high as 900 pounds per lineal foot, and in another case for a 560-foot span, figured for an intensity of only 25 pounds, it ran as high as 1 100 pounds per lineal foot. It is true that these spans were intended to carry both railway and highway loads; but even had they been intended for railway traffic only, the total pressure per lineal foot would have over-run considerably the 600 pounds ordinarily specified. In the writer's opinion the best specification for wind pressure would

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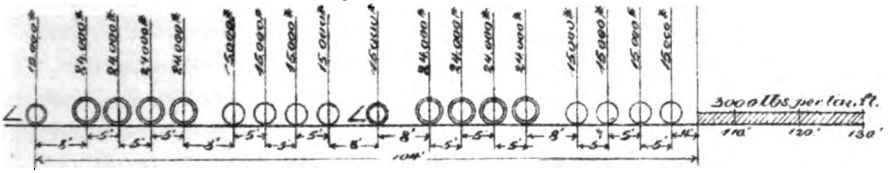
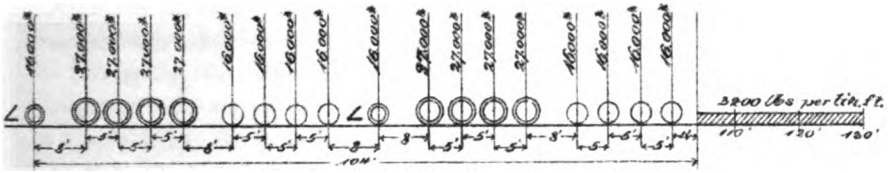
PLATE XVI.
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XXVI. Nº 522.
WADDELL ON
BRIDGE DESIGNING.

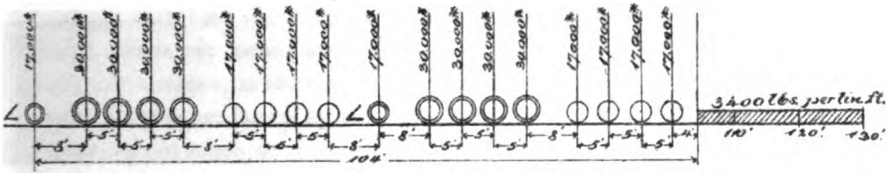
Class Z.



Class Y.



Class X.



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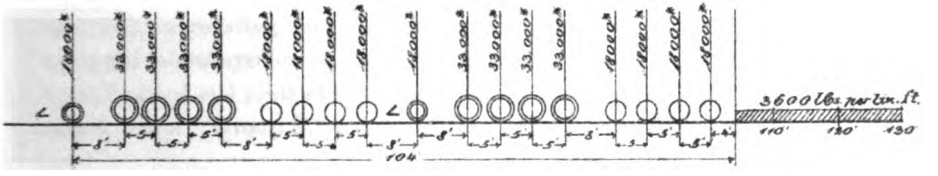

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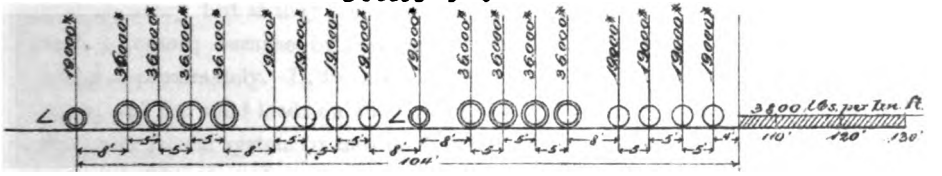
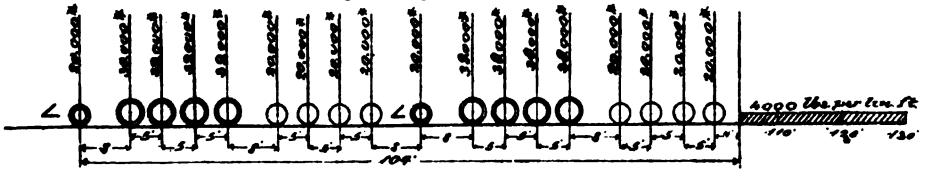


Class U.



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embody both methods, using the standard 600 pounds as a minimum, and calling for intensities on the empty structures varying between 40 pounds for very short spans and viaducts and 25 pounds for very long spans, those on the loaded structures varying from 30 to 25 pounds.

In respect to areas opposed to wind pressure, it is the writer's practice to double the area of the vertical projection of one truss, deduct therefrom the area of the leeward web protected by the train, and add to the difference the area of the floor system found by multiplying the span length by the vertical distance from the top of the guard rail to the bottom of the stringer. If the lower lateral diagonals be not protected by the windward chord or by the stringers, their area is to be included also.

Now as for the "Effects" of the specified wind loads, the most important of these are too often entirely ignored by computers, especially in competition; for it does not suffice to calculate the stresses in the lateral diagonals and stop there. In good practice the effects of the wind loads are followed from the points of application until the masonry is reached, and both the direct and indirect effects are considered. By direct effects are meant stresses which come directly from the loads, such, for instance, as the tension on the leeward bottom chord from the loads of the lower lateral system. By indirect effects are meant those which do not come so directly (and are, therefore, too often ignored), such, for instance, as the increase in the tension of the leeward bottom chord due to the load that is transferred from the windward to the leeward side in resisting the overturning moment of the wind loads of the upper lateral system, or the somewhat complicated wind stresses in the inclined end posts of through bridges. The latter subject will be treated under "Group E," but the former might as well be taken up here.

The amounts for the transferred load stresses cannot be determined with accuracy, but as they are large, it is not proper to ignore them for such a reason; because by making certain assumptions they can be found approximately. If, for instance, there were no upper lateral diagonals, and the wind loads on the top chords were therefore carried to the lower lateral system by means of the overhead transverse bracing, there would be transferred from the windward to the leeward truss at each panel point a load equal to the product of the wind load at the top chord panel point and the truss depth, divided by the perpendicular distance between trusses. The resulting tensions on the leeward bottom

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chord (which are the only stresses from this source that are of any importance) could be readily calculated, and would be found to increase from end to middle of span. But, again, if there were no overhead vertical bracing, the wind loads applied at the panel points of the top chord would be carried entirely by the upper lateral system to the upper ends of the inclined end posts, and thence down the same to the piers, producing a release of load on the windward pedestal and an increased load on the leeward pedestal. The latter, if the chords were parallel, would produce a tension on the leeward bottom chord that would be constant from end to end of span ; or, if the top chord were broken, one that would decrease from end to middle of span.

Now, as in general, the upper lateral system has, or ought to have much more rigidity than the overhead transverse bracing, the probability is that most of the upper wind loads will travel by the former ; so certain engineers, and of late among others the writer, have been in the habit of assuming for convenience, that they travel entirely in that way, and that in consequence the stresses from transferred loads in both leeward and windward bottom chords are constant from end to end of span, provided the top and bottom chords be parallel. The writer has gone a step farther by assuming that this holds good even when the top chord is broken ; and, perhaps, here he is nearer the mark than in the case of parallel chords, for on one account the stresses tend to decrease from end to middle, and on another account they tend to increase in the same direction.

The released loads on the windward pedestals cause a decrease in the dead load tensions of the windward bottom chord, and this effect should be added invariably to the compressions in said chord from the wind loads of the lower lateral system when testing for reversing bottom chord stresses with empty structure. Too often this is ignored by computers, especially when competing for contracts.

We will now pass to "Group C," viz., "Styles and Proportions of Bridges." As before stated these, by a process of elimination, are being gradually reduced to a system, although certain well known engineers persist in adhering to antiquated types. Experience has shown that for spans up to 90 or 100 feet, plate girders are the proper thing to use. They should, however, be built in one piece at the shops, and never spliced in the field. This will limit the span for such structures to

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three car lengths, the weight during shipment being carried entirely by the outer cars, the intermediate one being an idler.

On account of the unavoidable uncertainty concerning stress distribution that exists in riveted connections, plate girders whenever they can be used are preferable to lattice girders. The latter, however, as through bridges, for single track spans between 100 feet and 150 feet in length, and for double track spans between 100 feet and 125 feet, are the proper style of structure to adopt. However, they should invariably be built upon the single cancellation principle instead of, as ordinarily, with several systems of intersection. The latter method is not only unscientific, but also extravagant in the use of metal; and the claims of its advocates that it is superior to the Warren type are imaginary and unfounded. Here is a fine opportunity to discuss the much-vexed question of multiple intersection *versus* single intersection riveted girders, and it is to be hoped that it will be taken advantage of by those interested in both sides of the topic.

It is evident from the preceding that the writer would rule out entirely the pony truss bridge. His reason for this is because of the uncertainty in computing the strength of the top chord. In the first place its length as a strut is indeterminate; and in the second place the side bracing, when attached to shallow floor beams, as is often likely to be the case, is liable to do more harm than good by forcing the panel points of the top chord out of line. The only satisfactory method of determining how to build good pony-truss bridges would be to construct a number of them and load them to destruction, thus ascertaining their weak points and making, if practicable, the necessary corrections. But would not this be a case where "*le jeu n'en vaut pas la chandelle?*"

For spans exceeding 150 feet for single track bridges and 125 feet for double track bridges, the Pratt truss with its vertical intermediate and inclined end posts has proved to be the most satisfactory structure, although one of our prominent bridge engineers is advocating a truss with posts of varying inclination to the vertical. His principal claim for superiority for this truss is economy of material; but in the writer's opinion this is much more than offset by several inferior details, prominent among which is the suspended floor beam. It is hoped by the writer that the gentleman referred to, a good friend of his, by the way, will consider the foregoing as a sufficiently severe attack upon his pet truss to warrant his bringing up the subject for a thorough discussion by the profession.

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The principal objections to suspended floor beams are their lack of rigidity to resist vibration, and their inefficiency to act as lower lateral struts. It has been whispered that riveted connections of floor beams to posts are not as perfect as they are held to be, but the writer has yet to hear of the first case of failure of the rivets in this connection when the beams were well proportioned and of the proper depth, although he has himself seen cases where extremely shallow and weak floor beams could not be retained to the post by any practicable means. He would like to hear the experience of other engineers on this point.

Beyond the limit of about 250 feet (more or less according to the lightness or heaviness of the load carried) it is well to economize metal by adopting what is termed by some engineers the "Pettit" truss and by others the "subdivided Pratt truss," inclining the top chord as may seem advisable—in fact the top chord may be inclined or broken to advantage in Pratt truss spans exceeding say 175 or 200 feet. In the Pettit truss the writer has become convinced that, in dividing the panels, it is much better to carry the intermediate panel load by a strut towards the pier than to take it by a tie towards the center of the span, even if the former method require, as it often will, more metal; because the rigidity of the truss as a whole is thereby increased, and the top chord sections are much more perfectly supported at mid length.

The inclination of the top chord to the horizontal is a feature that can be and is sometimes carried to excess. Its effect is undoubtedly to economize in metal; but at the same time its excessive use will throw too much work upon the top chord and leave very little to be done by the web, which therefore becomes light and vibratory. Again, with excessive curvature of top chord, the stresses in web diagonals are liable to reverse. If such reversion or tendency towards reversion be provided for adequately by stiffening the diagonals, it is true that the objection vanishes, but at the same time so does the economy. In the extreme case of a parabolic top chord, towards which some of our latest long spans approach closely, every panel would need counter-bracing in order to transfer the advancing live load; and surely no well-posted engineer will claim that the parabolic truss bridge is a good structure to adopt.

It is the writer's practice when designing a bridge with inclined top chords, to figure the advancing live load stresses for both tension and compression upon every web member in the truss from pedestal to pedestal, then, after allowing whatever seems advisable for impact,

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determine what diagonals will need stiffening and what will not. The percentage to be allowed for impact is a matter of judgment, and will depend largely upon the character and velocity of the advancing load.

The proper minimum distance between central planes of trusses for long through spans is still an undetermined point, some engineers making it one-twentieth and others one-eighteenth of the span. The writer is now inclined to adopt a middle course by calling it one-nineteenth of the span, although he once designed some 500 feet spans with a perpendicular distance between trusses of 25 feet. It is hoped that those engineers who think that the smaller limit will induce too much vibration, will, in the discussion of this paper, give their reasons for so thinking. In case of deck structures, either the limit for the pependicular distance will have to be increased or the truss depth will have to be decreased in comparison with the same dimensions for through bridges, on account of the greater overturning moment of the wind pressure; and for through spans of medium length, say 250 feet, the perpendicular distance between central planes of trusses should be much more than one-nineteenth of the span. The writer would suggest the following table for through spans of railway bridges:

Span.	Least Perpendicular Distance between Centers of Trusses.
250 feet and under.	18 feet.
300 " "	19 "
350 " "	21 "
400 " "	23 "
450 " "	25 "
500 " "	27 "
550 " "	29 "
600 " "	31 "

The writer is an earnest advocate of the increasing of clear roadways from 14 to 16 feet for single track through bridges and from 27 to 29 feet for double track through bridges. The extra 2 feet of width thus gained, together with a properly designed floor system and efficient protection and re-railing apparatus at each end of the bridge, would do away with fully 90 per cent. of the bridge accidents caused by derailments, and thus deprive the opponents of pin-connected bridges of their sole argument that is worthy of consideration.

The maximum allowable truss depth, except for draw bridges, in

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the writer's opinion, should never exceed three times the perpendicular distance between central planes of trusses; and when the extreme depth is used at mid-span it should be reduced toward the ends considerably.

For several years past the writer has held the opinion that in important bridges the lower lateral systems should be rigid, and he has now about come to the conclusion that an adjustable member is better out of any railway bridge than in it. However, he is not yet prepared to abandon entirely the use of adjustable members until such time as the profession will begin to agree with him substantially in this opinion.

In regard to the proper panel length for any truss bridge, the tendency of late years has been to increase this length for the purpose of saving metal not only in the eye-bars but in nearly all the other members of the structure; but, while saving metal, some designers have gone a little too far by using eye-bars too shallow for their lengths. Such proportions as 30 feet length and 4 inches depth are not unknown; but what the sag of these bars is the writer cannot say. Although not entirely guiltless himself in this particular, he has for several years been endeavoring to reform by adopting the following table, occasionally, however, departing from it slightly, but in all such cases compensating or endeavoring to compensate by the use of extra section in the member:

Unsupported Horizontal Length of Bar.	Minimum Depth of Bar.
15 feet.	2 inches.
16 "	2½ "
17 "	3 "
18 "	3½ "
20 "	4 "
22 "	4½ "
24 "	5 "
27 "	6 "
30 "	7 "
33 "	8 "
40 "	10 "

The theory has been advocated by a prominent engineer that the use of shallow bars for long panels can do no harm, for by the formation of a catenary in the bar the metal is not overstrained; but such a theory does not appear to the writer to be tenable. Long panels for wide bridges are all right, but for single track spans of short and medium lengths, extraordinarily long panels cause the lateral diagonals to act

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with too small an inclination to the planes of the trusses, and thus lessen their power to check vibration.

In respect to spacing of stringers there seems to be considerable difference of opinion among engineers. Some say that they should be placed exactly under the rails in order that the load may be applied directly, while others space them as much as 9 feet and even 10 feet apart so as to economize on metal in floor beams. If the former method be adopted, outer stringers should be used so as to support the ends of the ties in case of derailment. This, of course, increases the weight of metal, and is, therefore, objectionable, if a more economical and equally effective system can be designed. As a matter of fact, can any one point to a case where metal stringers have been injured by the bending down of the horizontal legs of inner upper flange angles, and if there be such a case, was there not some glaring weakness in the floor system proper? Statistics on this subject would be valuable.

For several years the writer has adopted the following floor system: metal stringers spaced 8 feet centers; pine ties, 8 x 12 inches on edge, and dapped 1 inch onto stringers, each tie being bolted at each end through the stringer flanges and outer guard timbers with $\frac{3}{4}$-inch bolts staggered in respect to the flange angles; outer guard timbers, 6 x 10 inches, or 8 x 10 inches, dapped 2 inches onto ties, which are spaced, as nearly as may be, 13 inches centers, leaving openings of 5 inches, and inner guards either of 5 x 4 x $\frac{1}{2}$-inch angle iron, or of 6 x 6-inch timber dapped 2 inches onto the ties and faced on the sides next to the rails with 3 x 3 x $\frac{3}{8}$-inch angle iron, screw-bolted to the timber, the guard timbers being bolted to alternate ties with $\frac{3}{4}$-inch bolts. Certain railroad engineers have objected to this system, because of the difficulty of replacing ties. They say that only every third or fourth tie should be bolted, and that the bolts should not go through the stringer flanges, but should grip the same by one-sided heads. The writer's reply to this criticism is that the system was designed not to make it easy of replacement, but to make it difficult to break up or remove in case of derailment; for a floor system should be so built as to be able to carry a train entirely derailed across the structure without injury to the latter. The writer thinks that this floor system on bridges having 16 or 29 feet clear roadway, and provided with proper re-railing devices and collision piles in the embankment near the ends, will afford the best possible protection against failure of structure by derailment.

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If any one has a better floor system than this, let him describe it in his discussion and show why it is superior, after which the writer will be only too happy to abandon his system and adopt the better one.

Opinions differ concerning the use of pine and oak for floor timber. The writer's preference for the former is due to the fact that oak is unsatisfactory because of its warping and splitting. Moreover, when it gets old, dry-rot sets in and forms a punky material that is easily ignited. Good pine is decidedly superior to oak; but one should not economize on the depth of the ties when the stringers are much spread, for the strength of timber is usually overrated.

We will now pass to "Group D," viz., "Intensities of Working Stresses," and will take the bold step of assuming that in the near future the material for the metal portions of railway bridges will be exclusively steel. This step is warranted by the fact that to-day mild steel is practically as cheap as iron, and that it is fully as reliable.

There is no portion of bridge designing in which there is greater diversity of opinion among authorities than this matter of intensities of working stresses. For this there are two reasons: First, the problem of what is the relation between statically applied loads and the same loads applied with different velocities, is practically an unsolved one; and, second, the effect on metal of oft-repeated loads has been experimented upon very extensively, but not in a manner that makes the deductions from the experiments applicable to bridge designing.

The subject of the relation between statically and dynamically applied loads has for years been occupying the writer's mind; and at one time, when in Japan, he went so far as to design and build an apparatus for measuring the extensions of bridge members under loads. Unfortunately, either the design or the manufacture was defective (possibly both), for the instrument failed to give any record of value; and since then no opportunity has been found to continue the investigation. The French technical papers have lately stated that there has been designed in Europe an apparatus for making such measurements by means of water pressure on cylinders of widely varying diameters, and that the apparatus is a success. Here is a fine opening for a few energetic engineers and professors of civil engineering to make a series of experiments of the greatest value; and it is not unlikely that, were said experiments made under the auspices of the American Society of Civil Engineers,

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an appropriation to defray the expenses of same could be obtained from the United States Government. The results of a proper series of tests of this kind would be of incalculable value to the engineering profession, and consequently also to the general public. It is probable that the railroad companies would lend their assistance to the extent, at least, of furnishing trains and bridges for the experiments. The series of tests should cover spans of all kinds and lengths, and should be applied also to all the different members of said spans.

Now, as to the wearing effect of oft-repeated loads: a number of German scientists have devoted several years to an elaborate series of experiments upon the effect of loading and unloading quite rapidly test pieces of iron and steel beyond the elastic limit. The effect of these tests upon American bridge specifications has been to introduce a somewhat burdensome formula, which is generally of the form:

$$\text{Intensity} = \text{constant}\left(1 + \frac{\text{minimum stress}}{\text{maximum stress}}\right)$$

If the live loads on bridges were applied every two or three minutes, such a formula would be applicable; but in most cases the metal in bridges has time to recover itself between the applications of the live loads; consequently, the adoption of a "fatigue" formula would appear to be unnecessary, involving as it does the use of extra metal.

The adoption of a modification of this "fatigue" formula to cover the effects of both impact and fatigue, is probably, in the present state of our knowledge, the best compromise that can be made; and the writer would consequently suggest that of

$$\text{Intensity} = \text{constant}\left(1 + \frac{1}{2} \cdot \frac{\text{minimum stress}}{\text{maximum stress}}\right)$$

This form of the equation (by no means original with the writer) accords very well with his ideas of what the extreme limits of stress on metal should be. It must be observed, though, that the "constant" in the equation must be changed for the different kinds of bridge members, as it would be obviously incorrect to make it the same for primary truss members and for the chords of long spans. Moreover, it is scarcely worth while to apply the formula to primary truss members, stringers, floor beams and plate-girder spans under say 50 feet in length; for in these the item of impact is so much more important than that of fatigue, that, in the limited state of our knowledge, it is well to adopt intensities which we know by experience will be low enough.

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The adoption of the formula involving the factor $1 + \frac{\text{min.}}{\text{max.}}$ is surely incorrect, for it implies that a live load is twice as destructive as a dead load. If this were so for one member, it would not be so for another; or, if it were true for short spans, if would not be true for long ones. Let us take, for instance, an extreme case and assume that there are, side by side, two very long duplicate suspension bridges whose cables are iron eye-bars. In one bridge, the dead load strains these bars 40 000 pounds per square inch, while in the other the dead load strains them to only 20 000 pounds; but once a day there passes slowly over the structure a live load that strains them 10 000 pounds more, making a maximum intensity of 30 000 pounds. Now, which of these structures would fail first? Surely it would be that which is loaded constantly to 40 000 pounds; but the formula would indicate that the two structures under the different loads are of the same strength. Some engineers have even been known to contend that were the live load in the second bridge increased so as to produce a tension of 20 000 pounds per square inch, making the maximum intensity 40 000 pounds, the other bridge would still be the one to fail first. This, perhaps, is an extreme view to take; but it would be interesting to try a similar experiment upon a small scale by loading a vertical tension member with a constant dead load that we know, if left on a certain length of time will surely cause rupture, and loading other similar pieces periodically by loads applied and removed very gradually, producing a series of maximum intensities varying uniformly. If it be desired to obtain results for combined live and dead loads, certain of the test pieces could have dead loads attached, that would differ uniformly. To obtain results with any pretension to authenticity, the test pieces should be made alike with the greatest care and of the most uniform metal obtainable; and there should be not less than three pieces used for each different kind of load. The writer would suggest that some energetic young professor of civil engineering, who has time to spare, undertake this series of experiments, and communicate the results to the American Society of Civil Engineers.

Mr. Cooper, in his specifications for both railway and highway bridges, effectively makes the broad statement that a live load in all cases is just twice as destructive as a dead load. If this be true for a railroad bridge where the moving load is applied rapidly and with impact, would it also be true in a highway bridge where the load is applied slowly and quietly?

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He may reply that he has increased the intensities for highway bridges; but this would be begging the question; for, surely, *the dead load* in a railway bridge is no more destructive than an equal dead load in a highway bridge.

The writer would suggest the following for intensities of working tensile stresses:

Bottom chords and main diagonals (eye-bars),

$$12\,000\left(1+\frac{1}{2}\cdot\frac{\text{minimum stress}}{\text{maximum stress}}\right)$$

Bottom chords and main diagonals (plates or shapes) net section,

$$11\,000\left(1+\frac{1}{2}\cdot\frac{\text{minimum stress}}{\text{maximum stress}}\right)$$

Long verticals (eye-bars)	10 000	pounds.
" " (plates or shapes) net section	9 000	"
Short " (eye-bars)	9 000	"
" " (plates or shapes) net section	8 000	"
Plate hangers	7 000	"
Bottom flanges of riveted cross-girders and stringers and longitudinal plate girders over 20 feet long	10 000	"
Bottom flanges of stringers and longitudinal plate girders of 20 feet length and less.	9 000	"

The principal object in reducing the intensity for flanges of girders not exceeding 20 feet in length is to discourage the use of short panels.

It is proper to use the same formula for bottom chords and main diagonals, because the minimum stress in the former is the dead load stress, while in the latter it is the dead load stress less the live load stress that tends to put the diagonal in compression. When counter ties are used, of course there can be no such reversing stress in excess of the dead load stress, hence the minimum stress will be zero, and the proper intensity of working tensile strength will be 12 000 pounds for eye-bars and 11 000 pounds for plates and shapes. For counter rods, the writer, when he cannot avoid adopting them, still prefers to use iron, and to strain them 8 000 pounds to the square inch.

Some engineers contend that, *for steel*, tension members built of plates and shapes are as strong, per square inch of net section, as are eye-bars; but until ample experiments prove this to be a fact, the writer prefers to make a difference for the two cases, although not so great a

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difference as he would adopt were the metal iron. Discussion on this point would be valuable.

For top chords the intensity recommended would be

$$11\,000\left(1+\frac{1}{2}\cdot\frac{\text{minimum stress}}{\text{maximum stress}}\right)$$

unless the ratio of unsupported length to least radius of gyration exceed fifty, in which case the intensity given by the formula is to be multiplied by the quantity $\frac{50}{\text{ratio of } l \text{ to } r}$. In most cases of well designed railway bridges the ratio of l and r is less than fifty. Some engineers are of the opinion that top and bottom chords can be strained alike, especially if a slightly higher grade of steel be adopted for the former, but the writer will be averse to this until experiments upon full-sized compression members prove it to be legitimate.

For inclined end posts the intensity of working stress should be found by the same formula as used for top chords; but in nearly every design for through bridges it will be found that the bending effect of the wind pressure will necessitate an increase of section. For vertical posts, which should always be hinged, the intensity can be found by the formula,

$$p = 12\,000 - 60\,\frac{l}{r}.$$

This formula was given to the writer some five years ago by Mr. Edwin Thacher, who established it in an eminently practical manner by plotting the results of a large number of experiments, inclosing the points by an oval and drawing an axis through the latter. The equation to this straight line, divided throughout by a certain factor for safety, will give the last equation. The writer wishes to protest here against the further use of $\frac{l^2}{r^2}$ in formulas for compression members, for the reason that it involves unnecessary labor and gives results that are no more nearly correct than those obtained from formulas involving $\frac{l}{r}$ to the first power. The general use of the second power is due to its having occurred in Euler's formula, which was established upon an assumption that does not hold good for compression members of bridges. Some engineers may contend that it is immaterial whether $\frac{l}{r}$ be used to the second or first power, as the results are always tabulated; but there are

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so many slightly different formulas specified which involve $\frac{l^2}{r^2}$ that one often finds that the tables he has at hand will not apply.

For lateral struts, the ends of which should always be fixed, the formula will be

$$p = 17\,000 - 55\,\frac{l}{r}$$

This formula may appear to give too great intensities; but it will be found that the limiting sizes of sections and limiting ratio of length to least dimension, which are given in good specifications, will overrule the objection; and when designing the lateral systems of large and important bridges, the computor who tries it will become convinced that this formula is just what he needs.

In cases of very great wind pressures for long spans, where it takes a a large amount of metal to provide for wind stresses, the writer considers it legitimate to adopt a double system of triangulation for the lateral system, and to assume that each half carries one-half of all the wind pressure allotted to the entire system; and in so doing he is not inconsistent, although condemning multiple systems for vertical trusses; because maximum wind loads are of rare occurrence, while the maximum loads for vertical trusses are liable to occur frequently.

Referring to the latest edition of Mr. Theodore Cooper's specifications, the writer wishes to take exception to a couple of points relating to strut formulas. First, the intensities specified for posts are excessive; for instance, in the case of a long span where, on account of the inclined top chords, the dead load stresses in posts are so small that we may here ignore them (if in fact they do not reverse), his formula for steel would be about $8\,400 - 48\,\frac{l}{r}$. If $\frac{l}{r}$ be assumed equal to 80, which is allowable, the intensity of working stress would be only 4 560 pounds. Surely this is too small for such a large and important member as a vertical post. Or, if we assume $\frac{l}{r} = 50$, the intensity will be 6 000 pounds, while according to Mr. Bouscaren's specifications it would be 12 000 pounds. Surely when two such high authorities differ to such an extent, some explanation is needed. The writer's proposed formula would give an intensity of 9 000 pounds, which is exactly a mean between the two.

Second, the writer considers it to be incorrect to treat a batter brace

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or inclined end post in the same way as a vertical post is treated; for said batter brace is in some extreme cases merely a continuation of the top chord, and in all cases resembles the same in respect to stress and sectional area, therefore it ought to be treated as such. It is hoped that Mr. Cooper will explain in the discussion his reasons for specifying such peculiar intensities of working compressive stresses as those referred to.

Finally, a word in regard to the use of the formula for reversing stresses, viz., $p = \text{constant}\left(1 - \frac{1}{2} \cdot \frac{\text{minimum stress}}{\text{maximum stress}}\right)$

This should be employed where the reversion is of common occurrence, as in web diagonals, and not where it is unusual, as in the flanges of floor beams where the stresses are liable to be reversed once in a great while by highway loads carried on cantilever brackets.

We will now pass to "Group E," viz., "Combined Stresses," and will divide it into two parts: 1st, Longitudinal stresses only; and 2d, Co-existent longitudinal and bending stresses.

Part No. 1 relates principally to bottom chord proportioning when the wind stresses are taken into account. One authority stipulates for this, that when the wind stress exceeds one-quarter of the sum of the live load and dead load stresses, the section shall be increased until the total stress per square inch will not exceed by more than one-quarter the maximum fixed for the live and dead loads only. The writer would prefer, after figuring the direct and indirect wind stresses as explained previously in this paper, and adding them to the sum of the live load and dead load stresses, to use an intensity 40 per cent. greater than that adopted for the live and dead load only, provided that said intensity never exceed 21 000 pounds for eye-bars, or 19 000 pounds for tension members built of plates and shapes. It is better to take into account all possible stresses when finding the maximum total, and to use a high intensity, than to ignore certain important stresses and use a lower intensity.

Part No. 2 relates principally to the proportioning of batter braces or inclined end posts; and it is a peculiar fact that no railway bridge specification yet written begins to cover the ground of designing properly this important member of a through bridge; and, moreover, the matter is seldom given the importance it deserves, even in large

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bridges designed by our leading bridge companies and by expert engineers.

There are three vital considerations in the correct proportioning of batter braces, viz.:

A. What are all the direct stresses, and how are they to be found?

B. What is the greatest bending moment, and how should it be computed?

C. What are the proper intensities to be used, and upon what should they be figured?

The answers to these questions are as follows:

A. Live load, dead load and transferred load stresses, the latter being computed as indicated previously in this paper.

B. The greatest bending moment is found by assuming a division (usually equal) of the total wind pressure upon the structure between the four pedestals, and multiplying the thrust on one leeward pedestal by the distance between the center of the end pin and the section of the batter brace under consideration. Some engineers, in order to reduce the bending moment thus found, which is undoubtedly very great, assume a fixedness of the batter brace at the pedestal, thus cutting down the length of the lever arm, and consequently the amount of the bending moment, by 50 per cent. Although the writer some seven years ago computed in this way, he is now convinced that the method is wrong; for the end of the piece cannot possibly be fixed or held in any way that even approaches fixedness. At the moving end of the span there is certainly enough play between the bottom of the pedestal and the holding down members, whatever they may be, to nullify all assumptions of rigidity; while at the other end of the span, even if there were no play in the end pin-hole, the anchor bolts and the details affected by the stresses therein are by no means strong enough to warrant one in assuming the connection to constitute a fixed end.

C. The intensity of working stress is the total intensity on the extreme fibre of the section considered, and is equal to the sum of the total direct stress divided by the sectional area of the member at the place considered, and the intensity of extreme fibre stress on the compression side, due to the bending moment of the horizontal reaction at the foot of the member. The value of this total intensity should lie between 20 000 pounds for short spans and 24 000 pounds for very long

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ones. Even these apparently high values will often make the sectional area of the batter brace appear excessive.

The most effective way to strengthen the batter brace against bending is to add metal in the form of heavy angle irons just outside of the webs of the built or rolled channels. A number of the latest Missouri River bridges, all built by the same engineer, show a quite unnecessary defect in the design for the reinforcement of the batter brace; for all of the reinforcing metal is placed upon the inside in the form of a built I beam, thus making the section of the member decidedly unsymmetrical, and weakening instead of strengthening it.

In one of these cases which the writer has checked, the addition of 16.85 square inches of section in the form of a built I beam *increases* the intensity of working stress for combined live and dead loads about 18 per cent.; while, if the same area had been added symmetrically, the intensity of working stress would have been *reduced* about 26 per cent. It is true that the member as it stands is all right for bending, even with the worst combinations of stresses, and the metal under live and dead load stresses is not dangerously overstrained; nevertheless there seems to be no good reason for disposing of extra metal so unscientifically as to make it do more harm than good, when it could easily have been distributed advantageously. Possibly the reason is that if the extra metal were divided equally between the two sides of the member, the leeward batter brace would, for combined direct stress and bending, be strained about 10 per cent. higher than the windward batter brace with all of the metal on the inside; but even then the extreme fiber would still be strained within legitimate limits. Moreover, is it not better practice to overstrain metal once in a great while, say once in ten years, by 10 per cent. than to overstrain it 18 per cent. every time that the structure receives its full live load?

We will now pass to "Group F," viz., "Plate Girder Proportioning." In this matter there is quite a little difference of opinion among bridge designers, probably owing to the necessarily unsatisfactory character of all investigations concerning stress distribution in this style of girder. Plate girder proportioning amounts to but little more than rule-of-thumb practice; so it would be well for us all to acknowledge the fact at once, and by a series of experiments to destruction, ascertain how satisfactory are our present methods of design, then, if necessary, modify them.

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The questions of how the shear on the web travels, and how the stress on a flange distributes itself over the section, can never be determined analytically except upon assumptions that cannot be verified even approximately, nor can we tell by theoretical investigation what should be the proper thickness for a web; consequently it would be well to drop these investigations at once and for all time, and content ourselves with ascertaining whether our present methods of proportioning will bear the searching investigation and criticism attendant on exhaustive experiments. In the writer's opinion any web, *if properly stiffened by angle irons* of an economically designed plate girder, will have sufficient sectional area as far as strength is concerned, so there is no use in adopting webs thicker than three-eighths of an inch for long, deep, single track girders, provided that said thickness give ample bearing for flange rivets near the ends of span, which in most cases it will.

As a matter of precaution, the writer is averse to making webs of stringers less than three-eighths of an inch thick, although he believes that five-sixteenths inch plate, when effectively stiffened and kept well painted, will do its work just as well. Does any one know of a case where the five-sixteenths inch web of a well designed plate girder was the cause of even symptoms of failure? Some statistics on this pont and on points of actual weakness of plate girders under traffic would be very interesting, and it is hoped that the discussion on this paper will bring such matters to light.

The clause in Mr. Cooper's specifications which stipulates that, whenever practicable, one-half of the sectional area of any flange should lie in the two angle irons, is a very good one, and in the writer's opinion the number of cover plates used for a flange should be restricted to three, or preferably to two.

There is an important matter neglected in most railway bridge specifications, viz., the proportioning of end stiffeners where the plate girders rest on masonry or other supports. The intensity to use for the steel end stiffening angles should be 8 000 pounds; and the filling plates, if there be any, should not be counted upon as aiding said stiffeners. Both ends of such end stiffeners, and the upper ends of all intermediate stiffeners of deck girders, should be cut so as to bear against the horizontal legs of the flange angles. This is a point often overlooked in manufacturing.

There is some difference of opinion among experts as to the greatest

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allowable length of unsupported compression flanges of plate girders, Mr. Cooper placing it at thirty, and others as low as twenty times the width of the flange. The writer thinks that with the style of floor system described in this paper, where the track ties provide great lateral stiffness for the flanges, the limit of thirty is correct.

It is his firm opinion that the proper way to proportion the section of a plate girder for a given bending moment is to count in the web as aiding to resist bending. It surely does aid in resisting, so why not allow for its resistance? There is no need for going into burdensome calculations involving moment of inertia of cross-section, when the method of assuming that one-sixth of the area of the web is concentrated at the center of gravity of each flange will give almost identical results. Of course, if the web be counted in, the intensities of working stresses for the flanges will have to be decreased accordingly. While it is true that for girders of ordinary proportions it is immaterial whether the flange section be proportioned by one formula, ignoring the web, or by another equivalent formula involving it, it is evident that in girders of unusual proportions, for instance, very deep floor beams where the bending moments are comparatively small, the ignoring of the aid of the web in resisting bending causes a waste of metal in the flanges.

We will now pass to the next "group," and the last, viz., "Group G," which includes "General Details of Construction;" but on account of the large field that it covers, we will have to confine our attention to the salient points of design whereon engineers differ, taking up first those relating to general features and afterwards those that may be more properly termed "details."

The question as to how far the riveting of stringers to floor beams should be carried, is still an undecided point. This method of construction has decidedly great advantages in respect to both rigidity and economy of material, but would be objectionable for very long spans for two reasons; first, the stringers have a tendency to do some of the work that belongs to the bottom chords, for which the connections of the former are unfit; and, second, there is a horizontal bending induced in the floor beams, especially in those near the ends of the span. The writer never hesitates about riveting stringers to beams in ordinary spans up to 175 feet in length, or in any draw span whatsoever; and he

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has lately concluded to adopt the system for spans of any length by inserting sliding points in the floor system at distances of about 150 feet. Stringers riveted to beams really aid the bottom chords in resisting reversed stresses, but upon this fact the writer never relies, preferring to stiffen the bottom chords, although the practice of some bridge builders is to omit the stiffening.

The use of end floor beams for all spans is still an undetermined matter. The old practice was to let the stringers of the end panels bear upon the masonry, and to run a light strut between pedestals; but for several years the writer has invariably used end floor beams with stringers riveted thereto, and is well satisfied with the results of the practice. The advantages of the detail are a homogeneous motion of contraction and expansion for all the metal in the structure with only two places for sliding or rolling motion, a greater rigidity of floor system in the end panels, and a very satisfactory end lower lateral strut. The only disadvantage is a slight one, viz., the necessity for using a little more metal.

The intersection *at a point* of all the axes of bridge members coming together at any panel point of a structure has for years been a desideratum, but as far as the writer knows has never been attained for any structure yet built. The writer, after seven years of trial, has at last solved the problem for large structures, and has applied the method to two or three large bridges that he is now preparing to build. The method, of course, could be applied to short and light spans also; but it would require an amount of extra metal that it does not seem advisable to employ, considering the fact that the objectionable results of eccentric connection can generally be provided for legitimately, either by straining the metal higher or by reinforcing. Unfortunately, however, the injurious effect of eccentric connections is too often ignored in bridge designs.

The connection of an end lower lateral diagonal to the pedestal plate, instead of to the chord and end floor beam is a very faulty detail. To begin with, the connection is made generally a foot below the center line of the chord, producing a bending moment that, when expressed in inch pounds, seems enormous. At the fixed end of the bridge no special harm is done; but at the roller end the uniformity of the pressure on the rollers is seriously interfered with, and a bending moment is thrown upon the end diagonal, if it be a stiff member. In short it is not a

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scientifically designed connection; and there is no need whatsoever for adopting it under any circumstances.

Of late years floor beams for large bridges have been designed with incomplete lower flanges for reasons with which all bridge builders are acquainted; but in such cases it is better to use an intermediate flange running the entire length of the beam rather than to carry the shear to the posts by patch-work. A few tests on full sized specimens of floor beams with incomplete bottom flanges and patched ends would afford considerable information of value, and if the experiments were extended so as to show the effect on the strength of beams of beveling or fish-bellying their ends, the results would be still more valuable.

There is a very common connection for lateral rods that is quite faulty, yet is used at the present time. It is the one where the rod passes between the vertical legs of two short pieces of angle iron and through either a plate or another piece of angle iron for the adjusting nut to turn up against. It is faulty because the rivets connecting the angles to the main member are proportioned for a shear equal to the tension on the rod, while, in reality, there is a neglected bending moment equal in amount to the tension on the rod multiplied by the perpendicular distance between its axis and the main member. This bending moment must be resisted by an equal one, one of the forces of which is a direct tension on some of the rivets. If the length of the connecting angles were made sufficiently great, the detail would not be so objectionable; although rivets should not be subjected to direct tension. The tension, perhaps, might be considered *indirect* in that it comes from an induced bending moment; moreover, it may be made as small as we please by simply lengthening the angle irons.

The writer has designed and adopted a similar detail for making the end of a column truly fixed, running the anchor bolts a couple of feet or more above the pedestal, each between two angle irons riveted to the member, and through a very thick plate resting on top of said angle irons. In this case the tension on the rivets is a bagatelle. The ordinary fixed ends for columns, even when the anchor bolts are figured strong enough, are weak in detail on account of assumptions, made to save metal, which would never be realized were the column bent by the assumed transverse load. One favorable feature of the detail just described is that the pull from the anchor bolts is carried directly into the body of the column, passing by without straining the bed plate and its connecting angles and rivets.

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Beam hangers with screw ends ought to be no longer countenanced, for they are in many respects inferior to plate hangers; and, in fact, suspended beams themselves should be ruled out, except for sub-divided panels, in which case they should be stayed effectively against all motion. In the bridge specifications of the Atchison, Topeka and Santa Fé Railway Company (1889) there is a clause requiring that in double track bridges the floor beams be invariably suspended. The writer is desirous of knowing what is the reason for this requirement. If there be a good reason, the general practice of bridge designers should conform with the requirement; but if not, the clause of said specifications in relation thereto should be changed.

The minimum thickness of metal permitted in certain bridge specifications (filling plates, of course, excepted) is one-quarter inch; but in the writer's opinion this ought to be increased to five-sixteenths, and for all horizontal plates (that do not let the water drain off readily) to three-eighths of an inch. Metal that is exposed very much to smoke from locomotives, especially where the smoke can collect and hang, ought to have a thickness of three-eighths of an inch, besides frequent painting.

In the writer's opinion, any mild or medium steel that will withstand the standard drift test, viz., the enlarging of a rivet hole 25 per cent. without cracking, will not require reaming of rivet holes.

The lengths of stay-plates for compression members are a matter of dispute among bridge builders, some requiring twice the lengths specified by others. The writer thinks that no hard and fast rule will answer, but that each stay-plate should have as many rivets to connect it to the main member as its importance of position demands. For instance, at the hips, the top chord and batter brace stay-plates should be long, while at the main intermediate panel points they should be shorter, and at sub-panel points they might be reduced to 12 inches in length so as to provide four rivets in each line. As no experiments have ever been made upon the requisite sizes of stay-plates, there is nothing for the designer to do but to fall back on his past experience and common sense when proportioning these details.

Finally, there is an important item in detailing that seldom seems to receive proper attention: it is the effect of an eccentric stress upon a group of rivets. Designers do not appear to recognize the fact that there is, under such a condition, an immense bending moment that can only be resisted by an equal bending moment due to the shearing resistance

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of the rivets in a circular direction about the center of gravity of the group. How often we see a lower lateral rod of say 1¼ inch diameter connected to the flange of a floor beam by a pin through a plate that is attached to the angles by four seven-eighths inch rivets, the perpendicular distance of the center line of the lateral rod from the center of gravity of the group of rivets being say 6 inches, and that from said center of gravity to a rivet being say 3 inches. If T be the tension on the lateral rod, one-quarter T will be the *direct* shear on one rivet, and one-half T the *indirect* shear on same; and as one-quarter T is all that the rivet under the assumed conditions ought to stand, it is evident that the total shear on the rivet is three times as great as it ought to be. This is, of course, an extreme case, but it serves to demonstrate the point at issue.

The extent of this paper having far surpassed the limit set by the writer at the outset, he will now close with an earnest request that all those who are especially interested in bridge building discuss thoroughly not only the questions raised herein, but also other disputed points raised by themselves or by others in the discussion; for it is intended to submit all discussions for still further discussion until there shall be practically no more to be said on the entire subject.

DISCUSSION.

THOMAS H. JOHNSON, M. Am. Soc. C. E.—Mr. Waddell treats us to a bit of very pungent satire in his opening paragraph, when, after stating the object to be attained, he leaves us to infer, from his figurative reference to the asymptote, that he has no hope of accomplishing the object sought until the finite shall pass into the infinite, and "time shall be no more." And therein he is, no doubt, correct. So long as there are differences in the attendant conditions; so long as men see through their own eyes, instead of their neighbors'; so long as materials, and our knowledge of them, are both in a state of evolution; so long as the engines, the cars and their lading, which our bridges are to support, are undergoing change—so long will men differ in opinion on many of the points raised in this paper.

To notice all the points raised by Mr. Waddell, would practically amount to writing a full treatise on "Bridge Designing," for which I have neither time nor inclination. I will, therefore, only touch upon a few salient points. First of these is the subject of "Concentrated

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Loads *vs.* Uniform Loads." I have always been in favor of the concentrated load method, because it involves more simplicity, greater accuracy and less labor. I am fully in accord with the quotation from Mr. Cooper which Mr. Waddell gives us on that subject; and while fully appreciating the beauty of his new method of finding the equivalent loads, it fails to convince me that the former method should be abandoned. The new method still requires separately determined uniform loads for moments, and for shear, each variable for different spans. The amount of labor necessary to calculate the equivalent loads, for each type of engine, would have served to make tables of moments and shears, for that engine loading, which would be far more servicable than the uniform load, in reducing the labor of subsequent calculations; and the result would not vary "20 per cent." from the correct amount, as Mr. Waddell shows may be the case with the uniform load method.

Next, as to the types of loading proposed. Turning to Mr. Waddell's diagram, I note that he shows his heavy train-loads with his heavy engine-loads, and *vice versa*. The developments of the engine and train now in progress,. and the future of which it is the bridge engineer's duty and aim to anticipate, are not being carried along on parallel lines. The growth of the engine results from the effort to increase the gross load hauled; the growth of the train load results from the effort to make the paying load a larger proportion of the gross load hauled. For instance, but a few years ago railroads were hauling 10-ton loads in cars weighing 8 tons; now they are hauling 30 and 40-ton loads in cars weighing 13 tons. And these cars, so loaded, go anywhere and everywhere; giving a condition of uniformity to the service on all roads, that renders more than one, or at the most two types of train load, unnecessary. But with the engine the case is different. The type of engine used will depend upon the steepness of the grades to be overcome; and different roads, and different divisions of the same road, will require engines of different class; hence, all of the engine types shown will be needed. I would, therefore, suggest that the diagrams proposed be amended to show a uniform following load of 4 000 pounds per foot in all cases. The 40-ton carloads, before alluded to, bring the actual loads of the present day up to 3 000 pounds per foot, as a general everyday load; while special loads are frequently handled that far exceed this figure. It seems to me, therefore, that allowing for future development of the carrying capacity of the cars, 4 000 pounds is not too high a figure. I would suggest further that Mr. Waddell's engine diagrams will be more in accord with the actual dimensions which the engine builders are following, if he reduces the driver spaces to 4½ feet, instead of 5 feet, and increases the distance from rear driver to tender wheel to 11 feet, leaving other dimensions as proposed. This will more nearly conform to actual fact, without sacrificing the simplicity aimed at.

I cannot agree with the author in his wholesale condemnation of the

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pony truss. There are certain situations in which this form of truss cannot well be dispensed with; and, notwithstanding the apparent ambiguity in the effective column length of the top chord, I have yet to see a pony truss bridge showing signs of failure on account of weakness in the top chord. I have known several such structures, some of which were built in the early days of iron bridges, and for light loads; and I have known them to show signs of failure in the floor beams, hangers, counters, etc., but never in the top chord. In the matter of subdivided panels, I must express myself in favor of carrying the intermediate panel load to the top panel points, which Mr. Waddell condemns. I fail, too, to see how the other method adds to the general stiffness of the truss as a whole, or how the mid-length of the top chord can be better supported than by the King post type of trussing. Furthermore, the form which Mr. Waddell advocates is open to the serious objection that in those panels having counters, there can be no assurance that the load does not follow the counter instead of the brace. When the counter is in action from the advancing load it becomes certain that the load must follow the counter, because a tie and a strut, lying in the same axis, cannot both be in service at the same time.

Passing to group D, it strikes me that, after spending so much time pleading for uniform loads, for the express purpose, as alleged, of diminishing the drudgery of calculation, the author becomes inconsistent when he advocates a complex formula for determining unit stresses, with a so-called "constant" which must have different values for each class of truss-members. The labor supposed to be saved by the uniform load will be more than lost in finding the unit stresses. And to what end? How much better will be the result than if we follow the simpler method of using a definite unit strain for each class of members, varied, if you please, by different unit strains for live load and for dead load? Touching upon the proper allowed stress for "tension members built of plates and shapes," calls to mind the growing practice of using built sections for the end panels of lower chords, and for the end suspender. The compression strains in these members are more or less remote contingencies, while the normal strains are those of tension. A due regard for the "eternal fitness of things" requires that these members present to the eye the forms of tension members, and the change from the eye-bar to the built form is an offense to the eye, which cannot be too severely condemned and frowned upon. Engineering structures are, as a rule, not æsthetic structures; but they have a beauty of their own, due partly to a certain grace of outline and proportion, but most of all because of a higher and more subtle beauty, born of an inherent expression of massive strength and fitness for their work. We can ill afford to sacrifice this beauty; and, so far as pin connected trusses are concerned, I would make the allowed unit strain for built members in tension so low that the resulting quantity of metal required would more than

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balance the slight difference in cost as between angles and eye-bars, and thus remove all temptation to the use of this form of ugliness.

As to the unit strains on columns, it has been my lot to appear in print on that subject several times in the last few years, and I would not enter upon it now, were it not that I believe I can add something to what has been said before. In *Engineering News* of December 22d, 1888, I pointed out the fact that many of those who are adopting the straight line formula, $P = K - c\frac{l}{r}$, give the numerical constants arbitrary values without regard to the relation which should exist between them, which relation had been established in a paper on the subject, read by me at the Deer Park Convention, in 1885, and published in the transactions of the Society for 1886. I now submit two plates, on which are plotted the lines representing all the straight line formulas now in use, which I have been able to collect—those for iron being on sheet No. 1, and those for steel on sheet No. 2. (See Plates XVII, XVIII.)

A glance at these diagrams would indicate that the profession at large has become convinced, not that the law of the column is correctly represented by a straight line, but that any straight line may represent that law. On Plate XVII, line No. 3 is for L. L. on chords, and line No. 8 is for initial strains on lateral struts, and both are taken from one and the same specification. Please note that these lines cross each other at $\frac{l}{r} = 50$, which means that for greater lengths than $\frac{l}{r} = 50$ he would require relatively more metal in a lateral strut than in a top chord. I cannot think that this was the author's meaning, but rather that it results from the determination of the value of c by "judgment" or "guess," rather than by the mathematical relation heretofore mentioned. On Plate XVIII, line No. 2 represents the equation for steel top chords, as used on the railroad lines with which the writer is connected. It has been established with due regard to the law of the column and correctly represents it. Line No. 16 represents the formulas which Mr. Waddell recommends, viz., 11 000 pounds up to $\frac{l}{r} = 50$, and 11 000 $\times \frac{50}{\frac{l}{r}}$ for greater lengths. I trust Mr. Waddell will not take offense if I venture to point out that this formula is utterly without support in reason or experience, and is a purely arbitrary assumption in every particular. It is simply a guess and a very poor one at that, for, comparing the two lines on the diagram. It will be seen that Mr. Waddell's method gives results that at $\frac{l}{r} = 50$ are 27 per cent. too high, and at $\frac{l}{r} = 120$ are 16 per cent. too low. The diagrams speak for themselves and further comment thereon is unnecessary.

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In the paper before referred to it was shown that the correct law of the column is represented by a straight line, tangent to the curve of Euler's equation. It was also shown, or stated, that this curve and its tangent possess the peculiar property that the ordinate at the point of tangency is always one-third of the ordinate at 0 of abcissas. I wish now to call especial attention to this property, as it contains the guiding principle which will enable us to find the proper value of c, for any assumed working strain value of K, in a simpler way than by the formula given in the original paper. In the working formulas, whatever value of K be assumed for the different classes of members, and kinds of stress, the value of c should be so taken that for $\frac{l}{r} =$ point of tangency, $P = \frac{1}{3} K$.

It was also shown that for different forms of end bearings, the points of tangency are as follows:

	Iron.	Steel. 0.12 Carbon.
Square ends at $\frac{l}{r} =$	218	195
Hinged ends " $=$	178	159

To which I now add—

	Steel. 0.16 Carbon.
Square ends at $\frac{l}{r} =$	191
Hinged ends " $=$	156

Calling this distance to point of tangency m, then equation $P = K - c\frac{l}{r}$ becomes $P = K - c\,m$, when $\frac{l}{r} = m$.

But by the foregoing condition, we also have for that particular case, $P = \frac{1}{3} K$. Hence,

$$K - c\,m = \frac{K}{3}$$

$$\text{or,} \quad c\,m = \frac{2K}{3}$$

$$\text{From which} \quad c = \frac{2\,K}{3\,m} \quad \ldots\ldots\ldots\ldots\ldots\ldots (a)$$

Giving to m successively the values just stated, we have the following values of c, in terms of K:

	Iron.	Steel 0.12.	Steel 0.16.
Square ends.....	$\frac{K}{326}$	$\frac{K}{292}$	$\frac{K}{286}$
Hinged ends.....	$\frac{K}{267}$	$\frac{K}{239}$	$\frac{K}{234}$

This will afford a simple, direct and ready method of fixing the proper value of c in any working formula for column strains. I give the formula for square ends and hinged ends both. It is my practice,

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however, to use only the hinged end formula for the compression members of bridges. There is no member in a pin-connected truss so fixed in direction at the ends as to warrant the use of the square end formula.

In regard to girder work, I wish to call attention to a practice which I believe is almost, if not quite, universal in the offices of the bridge companies. Notwithstanding the urgent advocacy of uniform loads, because the results are "near enough," they will carry calculation to ultra refinement in the weights of angles; making the top and bottom angle of a girder, floor beam or stringer to differ by 0.1 or 0.2 pounds per foot. I have even seen these weights written to two decimals. This practice becomes absurd, if we stop to consider that all that the rolling mills undertake to do, is to get within 2½ per cent. of the weights ordered; that is to say, in a 10-pound angle the allowed variation is 0.25 pound, and on a 25-pound angle it is 0.6 pound. These are the limits of accuracy in filling the order, and any greater nicety in the weights ordered is worse than useless, for it introduces shades of difference which cannot be detected by the shop foreman or the inspector, with the calipers and foot rule which they carry. I would like to see a clause in every bridge specification prohibiting the use of any weights of shape iron, except those corresponding to regular intervals of $\frac{1}{32}$ inch. It would then be possible to know that in assembling the right piece had gotten to the right place.

In "half through" plate girders, I notice that designers tend to the use of long panels with deep floor beams. This I regard as bad practice, because the lack of head room which prevents the use of deck girders also restricts the depth of floor; and, besides, the plate girder type always seems to me adapted to receiving small loads at many points, rather than large loads at few points. It will also be found in most cases, that the greater concentration of loads on a few points results in larger flange strains than with the more uniform distribution due to shorter panels.

T. C. Clarke, M. Am. Soc. C. E.—Mathematics is defined as the science which draws necessary conclusions. Such as the premises are, so will the conclusions be. Hence we should avoid the error of the young engineer, described by John Phœnix, who paced off the diameter of his circle, and then calculated its circumference to six places of decimals. Professor Waddell is doing good service in trying to show that the simpler method of uniform loads gives results as accurate as the assumption of loads headed by heavier engines can do, which is more complex to calculate. Certainly this is a matter of proof, and it can be shown whether he is right or wrong. In regard to usual pressures, it is a sad thing to reflect that a violent gale which would unroof houses, blow down chimneys and generally wreck any exposed structure presenting a solid surface, will not injure any kind of steel or iron bridge unless

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already tottering to its fall like the old Tay bridge in Scotland. But, on the other hand, the most carefully designed steel bridge will be sheared in two by a tornado, as if made of rotten pine. We must take every possible precaution to give strength, by making the bridge wide enough and not too high in proportion to width, and then—trust in Providence that the tornado will go somewhere else.

For a wood floor, that described by Professor Waddell is good; but the time will come, we hope, when a steel bridge without a steel floor of plates and gravel filling, will be looked upon as an imperfect structure, except in long spans where a reduction of speed generally takes place. The assumption that a live load is twice as destructive as a dead load is a safe one, and should not be changed until we get more light upon the subject. Engineers, in calling for tenders, would do better, instead of giving a formula, to work out the results, and give the maximum allowable strain upon each part of the bridge in detail, so much for top chords, so much for end posts, etc., etc. There would be no misunderstanding then. I agree with all the writer says about the proportioning of plate girders.

General Details of Construction.—The best rule I know of is to connect the chords, posts and diagonals with pins, and rivet all other connections. Also in spans of less than 150 feet, rivet all connections. The weak point in most bridges is the riveting of floor beams to posts. This should be carefully looked to, to see that the maximum shear is not exceeded. This is a very interesting and suggestive paper, and it is to be hoped that it will be the means of calling forth much useful information in the form of discussion, by those actively engaged in designing bridges.

GEORGE F. SWAIN, M. Am. Soc. C. E.—The writer has been accustomed for a number of years to compute bridges for a uniform load, together with a locomotive excess, believing that such an assumed load will give results that are practically as correct as those which may be obtained by the use of actual wheel loads. In a paper which he presented to this Society in 1887,* he referred to this matter, and advanced in favor of the use of uniform loads, almost identically the same reasons which are now urged with such force by Mr. Waddell. At the same time, Mr. Waddell exaggerates very much the labor involved in computing with the actual loads, and his remarks on this point will not be concurred in by many engineers of experience, who have discovered the short cuts and simplifications that are possible in the use of the moment diagram. For instance, on page 9 of this paper, Mr. Waddell says: "A strong point that can be raised in favor of using equivalent uniformly distributed loads for finding bending moments in plate girder spans, is that the ordinary engine diagram

* "On the Calculation of the Stresses in Bridges for the Actual Concentrated Loads."—*Transactions* Am. Soc. C. E., July, 1887.

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cannot be employed, for the reason that the greatest bending will nearly always occur when some of the wheels at the head of the train have passed off the span. In consequence, one would either need to have at hand a moment diagram with each wheel leading in turn for each standard loading, or would have to discard all diagrams and make a special investigation of moments for each case as it arises." Mr. Waddell has fallen into error here, for if some of the forward wheels pass off of the span, this fact may be allowed for in a very simple and expeditious manner, and the advantageous use of the moment diagram is not in the least interfered with. The principal argument in favor of using uniform loads with a locomotive excess, it seems to me, is that it is simpler; and that by properly choosing the loads, the results will be practically as correct as those obtained with a system of concentrated loads. The use, however, of a concentrated load system involving wheel spacings with fractions of an inch, is a totally unnecessary waste of labor, and cannot be too strongly objected to. Most engineers will cordially agree with Mr. Waddell upon this point.

Mr. Waddell's remarks upon the subject of wind pressure are interesting and valuable. No doubt the actual amount of wind pressure to which bridges are subjected is very uncertain, but this is no excuse for neglecting to compute and to take due account of all the effects, having once decided what pressure per square foot to allow for.

The writer cannot agree with Mr. Waddell in his unqualified condemnation of double system riveted trusses. The general principle of adhering to structures in which the stresses are as nearly as possible determinate, is undoubtedly sound, and will be conceded by most American engineers. No structure, however, not even the pin bridge with one web system, is perfectly determinate, on account of the continuity of the top chord, the friction on the pins, and other circumstances; and when, as in a riveted truss, the joints are firmly connected, and both chords are continuous, the amount of uncertainty in the stresses is quite considerable, and the secondary stresses very appreciable. Under these circumstances, which are well understood; the additional uncertainty introduced by using two systems instead of one, is small in comparison, and the advantages of the double system certainly deserve consideration. These advantages are principally the simplification of the details of the connections—an advantage which will be fully appreciated by any one who has attempted to design a single system riveted truss of considerable span, and the further fact that the destruction of one web member of the truss does not necessarily mean the collapse of the span. The terms "imaginary" and "unfounded" will have to be applied to Mr. Waddell's claims in favor of the single system truss and his unqualified condemnation of anything else, until he advances arguments and facts instead of simple statements in favor of his position. While most engineers will probably

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prefer single system riveted trusses in general, they will admit that the use of double systems under certain circumstances is not without justification.

Neither can the writer agree with Mr. Waddell in his unqualified condemnation of suspended floor beams. The arguments on both sides of this question deserve consideration; and if it is objected that suspended floor beams lack rigidity, it may be replied that this objection may frequently be overcome, while the method of riveting the floor beams to the posts introduces objectionable features which cannot be remedied, namely, the bending of the posts, the twisting of the chords, and the consequent unequal distribution of stress upon the diagonal bars, the inside bars of any one diagonal carrying a greater load than the outside bars. Added to this is the fact that the upper rivets in the floor beam connection are exposed to tension. If Mr. Waddell considers that these objections are met by the statement that he has never heard of the failure of rivets in this connection, we may ask him if he has ever heard of any practical disadvantage arising from the use of a properly constructed plate hanger. No doubt, suspended floor beams with adjustable hangers are objectionable, but a single plate hanger, riveted to the floor beam, transfers the load centrally to the pin, and when combined with proper details with reference to the lateral bracing, such a construction certainly has a great deal in its favor, and is not to be condemned without qualification.

The matter of the stresses in eye-bars due to their own weight is an interesting and important one, but there is not space to discuss it thoroughly here. The writer long ago came to the conclusion that the additional stress due to the weight was largely counterbalanced by the effect of the pull in the bar acting with a lever arm equal to the deflection. The precise condition of things may be found mathematically.

Mr. Waddell's floor system is a good one, though the writer much prefers to have the stringers under the rails, and safety stringers outside. The distance of the inside guard rails from the track rails, where re-railing devices are not used, is a matter regarding which there is much bad practice; and the writer has seen, on some important lines of railroad, these inside guard rails within 2 inches of the track rails. In such case they would be worse than useless, since a wheel could not run between the two rails, and even a 6-inch space, which would allow a wheel to enter, is not sufficient, since the opposite wheel would even then cramp the track rail, especially in the case of a narrow-gauged pair of wheels. With regard to the collision posts, the writer must confess that he doubts their value, and cannot but feel that they may, in many cases, do harm instead of good.

Mr. Waddell apparently considers the "fatigue formula" for determining dimensions, to be unnecessary and burdensome, yet he proposes a form of it which is in principle the same thing. The difficulty which he encounters in considering this matter, appears to arise from the fact

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that he does not separate the effect of impact from the effect of repeated stress. The matter of repeated stress is simply this; if a load of 55 000 pounds, applied gradually and once, is required to break a bar, then any less load will not break it, if applied once, and allowed to remain; but if the load is varied between, say, 45 000 and 30 000 pounds, rupture will finally occur; or if the variation is from 40 000 to 20 000 pounds, or from 30 000 pounds to zero, rupture will in time occur. Now, those who use the repeated stress formula believe that in each case, for a given ratio of maximum to minimum stress, the factor of safety should be allowed on the ultimate strength for this same ratio of maximum to minimum. That is to say, using the above figures simply for illustration, if the factor of safety is five, we should allow:

11 000	pounds	per	square	inch	for a bar in which max. = min.
9 000	"	"	"	"	max. = 1.5 min.
8 000	"	"	"	"	max. = 2 min.
6 000	"	"	"	"	min. = 0.

These results are unquestioned, although the effect of time—both during the application of the load and between loads—has not, until very recently, been carefully studied. Now the effect of impact is a separate matter, and the writer can see no correct method of allowing for this except by adding a certain percentage to the live load stress, adding to this the dead load, and treating the whole as a dead load. If this method is used, all difficulty vanishes. The percentage to be added should be assumed by the engineer according to a sliding scale, using judgment and experience in default of accurate experiments; then the factor of safety for a dead load must be decided upon, and used on the ultimate strength according to one of the repeated stress formulas, or used on the elastic limit, to suit the opinion of the designer. Mr. Waddell's formula will not suit all cases, highway as well as railway bridges, simply because the impact is different in these cases. And, in this connection, the importance of using a sliding scale in varying the unit stresses, may well be again urged. Surely it is unscientific to allow 10 000 pounds per square inch for bottom flanges of flat girders over 20 feet long, and 9 000 pounds when they are under 20 feet long. This practice will result in giving heavier flanges for a girder slightly under 20 feet in span than for a girder slightly over 20 feet span. And surely the allowable stress for chords in a truss 500 feet long could be greater than for chords in a truss 100 feet long, on account of the difference in impact, although by Mr. Waddell's formulas the same would be allowed in both cases. The writer may add here that personally he believes the use of a repeated stress formula unnecessary, on account of the fact that no load below the elastic limit will produce rupture of a sound bar, however often applied. He advocates using a factor of safety for dead load, based upon the elastic limit, and allowing for impact by a sliding scale of percentages.

Mr. Waddell advocates the straight line formula for posts, and pro-

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tests against the use of $\frac{l^2}{r^2}$ for compression members, claiming that it involves unnecessary labor. The difference of labor between using the ordinary formula with $\frac{l^2}{r^2}$ and that with $\frac{l}{r}$ is very slight, and it may not be out of place to point out here that the straight line formula, such as $p = 12\,000 - 60\,\frac{l}{r}$ is incorrect in form, and cannot possibly be a correct formula, though it may give results practically close enough. The correct principle to follow in deciding upon a formula for proportioning members would seem to be, to find first the correct form for the formula, and then to determine the constants according to the results of experiments on materials. These constants will be subject to modification as our knowledge of the strength of materials increases, but the formula will retain the same form. To use a formula whose form is incorrect is a step towards empirical and rule-of-thumb methods, which Mr. Waddell and many others who advocate the straight line formula would in general be quick to deprecate.

A strut is exposed, in any cross-section, to a uniform compression over the entire area, equal to $\frac{\text{load}}{\text{area}} = \frac{P}{F}$, and to a bending moment due to the fact that the strut deflects and the load does not act along the axis, this axis being curved. The maximum fiber stress f, will be the sum of the compression and of the bending stress, which last we may call s. Then,

$$f = \frac{P}{F} + s,$$

and must equal the allowable stress per square inch. Then we have,

$$\frac{P}{F} = f - s.$$

In the straight line formula,

$$\frac{P}{F} = 12\,000 - 60\,\frac{l}{r}.$$

The reason why this formula is incorrect in form is that the term s is made constant, and independent of the load P. That is to say, the fiber stress, due to bending, produced by the load P is independent of that load, an obviously absurd statement. In the usual formula with $\frac{l^2}{r^2}$, s is made to vary with P, as it must, being equal to

$$\frac{P}{F} \cdot \frac{1}{\alpha} \cdot \frac{l^2}{r^2}.$$

Then we obtain,

$$\frac{P}{F} = \frac{f}{1 + \frac{1}{\alpha} \cdot \frac{l^2}{r^2}}.$$

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The only argument that can be advanced in favor of the straight line formula is that it is slightly simpler. It is no more accurate, and is not as reliable when applied to cases beyond the range of actual experiment, because of its incorrect form, and there appears to the writer to be on the whole no sufficient justification for its use.

Mr. Waddell's formula for wind struts appears to be proposed because in most cases empirical rules will make it unnecessary to use it, and for other cases it is "just what the computer needs." Surely, scientific method, and a due consideration of the proper factor of safety, should rule in deciding upon even a formula for wind struts. As regards the proportioning of inclined end posts for bending, due to wind pressure, it is certainly incorrect to assume that the post is hinged at its lower end, and that the moment at any point is that due to the total wind reaction acting at the foot of the post. Whether it is correct to assume the post as absolutely fixed at the bottom will depend upon the ratio between the wind pressure and the vertical load upon the bridge; but in most cases it is, in the writer's opinion, much more nearly correct to assume the end fixed than to assume it hinged. The writer cordially agrees with Mr. Waddell's remarks as to allowing, in plate girders, for the bending borne by the web. One point, however, Mr. Waddell does not refer to, namely, that where the web is not considered as resisting bending, the web splices are proportioned for the shear simply, and the number of rivets in such splices is often so small as to render it certain that those near the top and bottom are much overstrained by the bending. In the writer's opinion, every web splice should have two rows of rivets on each side, the rivets spaced about 3 inches apart vertically in each row.

Mr. Waddell's paper abounds in sensible remarks, which have not been referred to, and the principal points selected for discussion have naturally been those in which the writer's views differ from his.

J. M. Johnson, M. Am. Soc. C. E.—The use of equivalent uniform loads in place of wheel concentration for the calculation of the stresses in bridge members would undoubtedly greatly lessen the labor of bridge computations, and, as Mr. Waddell points out, give results differing but slightly from those obtained by the method of wheel loads. I would be very much in favor of any plan having for its object the abolition of the various so-called standard engine loads, and the substitution of uniform loads equivalent to some such series of wheel concentrations as proposed by the author. However, if any uniform system of loading could be agreed upon by engineers, whether uniform loads or engine concentrations be adopted, a great step forward would be made, and the styles and proportions of bridges and general details of construction might safely be left to individual preference.

I imagine that substantial agreement among engineers can only be obtained upon the following points, viz.:

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First.—The loads to which structures should be subjected.

Second.—Intensities of working stresses.

Third.—Kind and quality of material.

With these points definitely settled and adopted, a partial uniformity of detailing might in time be brought about, although it is probable that personal bias and differences of shop practice will continue largely to influence such matters.

The loads prepared by Mr. Waddell seem suitable, and I would suggest no change. Although the author has developed a set of working stresses, I fail to find in his paper a statement of the ultimate strength and other physical data concerning the material proposed for general use, and in consequence the formulas could not well be criticised. In my judgment all specifications should contain requirements, stating under what conditions both iron and steel could be used, leaving it to the engineer to determine which would be the more suitable in any given case. The author leans very strongly toward steel, and perhaps with reason; but I would not be inclined to accept, as he does, the drift test as indicating where steel could be substituted for iron. A quality of steel that could be subjected in thin sheets to any sort of rough usage without perceptible injury, would probably have its strength seriously impaired if treated in the same way, the thickness being increased to the maximum of the rolls. When steel is used in lieu of iron, in addition to the general physical and chemical requirements, the specifications should, in my opinion, contain a clause limiting the maximum thickness of metal.

Carl Gayler, M. Am. Soc. C. E.—The question of minimum thickness of metal in bridge work is important, and from my own experience I would strongly advocate three-eighths of an inch throughout. Mr. Waddell is willing to stop at five-sixteenths of an inch, except where the metal is very much exposed to the smoke of locomotives, or for horizontal plates that do not drain the water off readily; he therefore concedes that a minimum thickness of three-eighths of an inch is desirable wherever smoke or rust is likely to act on the material. Now in most cases it is impossible to say beforehand which particular portion of a bridge will or will not be exposed to abuse. If we could be sure that the metal work of bridges would receive the proper care, be regularly cleaned, scraped and painted, the case would be different; but the fact is, that after completion of a bridge a careful attention to its proper maintenance, year after year, is the exception, not the rule. There are besides in every bridge some joints which are hard to reach by the painter. Right here might be the proper place to call attention to one feature of our pin-connected bridges in which they are at a disadvantage compared with riveted structures. At every pin joint we have a number of members—bar heads, ends of posts, chord sections, etc.—which are close to each other without being tight together; too close to allow painting and not

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sufficiently close to prevent rusting. In some bridges these interstices have been filled with impervious material, but in ninety-nine cases out of a hundred this is not done. The use of packing rings is for this reason also objectionable, as they prevent painting of the pins, without being water-tight enclosures.

As far as the additional cost is concerned, the following argument seems to be very forcible: Any company or corporation or municipality (for I include highway bridges as well) which is able and willing to keep a bridge in good condition, is also able and willing to incur the small extra expense implied in the three-eighth inch limit; where, however, the available funds for a new bridge are limited, it becomes doubly important for the engineer to insist on the same, as then it is more than likely that the bridge after completion will not receive the necessary attention. For the above reasons, and taking also in consideration the effect of handling of the material in the shop and during shipping and erection, and the slight increase of the cost, we are justified in insisting on a minimum thickness of metal of three-eighths of an inch in all bridge work.

GUSTAV LINDENTHAL, M. Am. Soc. C. E.—With increasing experience the disagreement among experts on the subject of uniform live loads, vice engine concentrations, will probably disappear. Mr. Waddell makes a timely and effective appeal for a simplification in the computation of stresses in bridges, and I wish to endorse the reasons he gives on this point. In my own practice, if not otherwise obliged, I have for years used an uniform live load expressed by a certain number of pounds per lineal foot, according to the class of bridge required, and in addition a concentrated load rolled over the entire span. For instance, for a heavy railroad bridge, I assume a rolling load of 4 000 pounds per lineal foot, and one additional concentrated load of 40 000 pounds on one axle, and the combination of these stresses would give the maximum strains from live load, including the effect of impact. For lighter structures I use 3 500 and 35 000 pounds respectively, and for the lightest railroad bridges of normal gauge 3 000 and 30 000 pounds respectively. My assumptions give much heavier loads for short spans. This is as it should be. Although we assume static conditions of loading, the swiftly moving loads at the rate of 60 to 80 feet per second cannot be without dynamic effects, which we have at present no means of estimating. They are more severe on short spans, but the above load assumptions give safe and durable structures.

While it is highly desirable in the study of bridge strains to have a clear conception of the action of engine wheel loads, and that the computer be fully disciplined in the calculation of their effects, the practicing bridge engineer may well dispense with time and energy wasting methods, which in no wise furnish safer results in the dimensioning of a bridge. It is one thing to have the mathematical ability to analyze

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strains with great accuracy if necessary, and another thing to have the good judgment to avoid unnecessary work, when simpler methods give results just as reliable, more easily comprehended, freer from possible mistakes in calculation, easier of checking, and more promising of agreement by different computers in the competition for work. Besides, the assumption of engine concentrations is not even true as representing the actual facts, since the load from the wheels is distributed through stiff rails and through the ties upon the stringers and floor beams in a more or less uncertain manner. The wheel loads, by the time they have reached the stringers and floor beams, are distributed, and are not the single loads assumed to rest on knife edges, and at the same time moving at the rate of possibly 80 feet per second. I agree with Mr. Waddell, that such nice methods contrasted with the great variations in unit strains, resemble very much the "straining at a gnat and swallowing a camel." When this is better understood by railroad engineers who order bridges, the method of computation by engine concentrations will in practice become obsolete.

Wind Pressure.—I should like to add to Mr. Waddell's remarks on this subject, that the lateral bracing of the bridge is more often and more severely strained by the lateral blows from swiftly moving engines and cars, causing lateral vibration, than from the wind itself. Wind force acting on the bridge through an elastic medium cannot be said to cause impact strains, but the lateral bracing is nevertheless subject to the impact strains from railroad trains, causing violent lateral vibration. We have no experiments on the force of such lateral blows from trains, but that they are comparatively severe will not be disputed by any one who has had the experience of standing on a bridge with a fast train rolling over it. In the proportioning of bracing for wind strains, it is probably one of the rarest things to calculate the lateral deflection resulting therefrom. It would show that most bridges of spans over 100 feet would deflect sideways several inches. The difficulty of making proper connections for the lateral bracing in a pin connected bridge, has also not yet been satisfactorily solved. As a general rule, the bracing ought to be made of rigid members, taking compression as well as tension, and thoroughly connecting and riveting up with the stringers and floor beams to add to its rigidity.

Regarding "Group C," namely, "The Styles and Proportions of Bridges," the observations of Mr. Waddell point out the advantages and disadvantages of certain types, although the improvements made in this respect cannot yet be regarded as final. Formerly the bottom chord of framed trusses was made of eye-bars from end to end; for short spans these members were very light, and a great many bridges exist of 100-foot spans with six light eye-bars in the middle panels of the trusses. After a while the end panels were made of stiffened eye-bars or of riveted members, and in several notable instances about one-

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quarter of the length of the bottom chord at each end was made of rigid members, and there is observable a general tendency to use heavier eye-bars, fewer of them and heavier pins. In larger spans this tendency may be carried to a greater extent than is now the practice. It is obvious that it would be better to have two eye-bars, 12 x 3 inches, than six eye-bars 8 x 1½ inches. The former would require heavier pins, but would permit of more satisfactory details in the connections, the strains would be concentrated into bulkier and stiffer members, the surface for rusting would be very much reduced. Long vibrating diagonals should be avoided, and in important bridge structures, all eye-bar diagonals should be stiffened by bracing between them.

The time is fast approaching when buckle-plate floors with stone ballast will be used in place of the present floor system consisting of oak ties and wooden guard rails. The buckle-plate floor and stone ballast need not weigh more than the oak ties and wooden guard rails. It would add greatly to the lateral rigidity of the bridge, the gravel on top taking the noise out of the structure. The rails could be put in iron troughs to effectually prevent derailment. Re-railing devices and collision piles near the end of the bridge should be added as an effectual protection against failure by derailment.

On the subject of "Intensities of Working Stresses," which is "Group D" of Mr. Waddell's paper, agreement may not easily be reached. Mr. Waddell maintains that mild steel will in the future be used exclusively for all metal railway bridges, because it is practically as cheap as wrought iron and fully as, or more reliable. In this respect the bridge engineer is dependent on the progress in the metallurgical arts of making wrought iron and steel. The improvements of the last ten years were altogether in the manufacture of steel, and none in that of wrought iron, which is still made by the old laborious and costly process of hand puddling. But the time does not seem to be far distant when wrought iron will be made by some improved metallurgical process in larger masses and at a great deal cheapened cost, and it would not be difficult now to predict whether wrought iron or mild steel in that case would be more largely used. In the use of steel for structural purposes, the preponderance is that made by the Siemen's-Martin open hearth process as against the Bessemer made steel. The mild steels will probably always be used for plates and for heavy members made of large single pieces of metal.

Mr. Waddell speaks of fatigue in metal and of the wearing effect of oft-repeated loads. I believe that this theory is based on tests which have no relation to the nature of strains in a bridge. Particularly the theory of fatigue in metal seems to be more romantic than true. If bridges were built to be overstrained, we could with some show of reason speak of physical changes as likely to result from the overstraining of metal. But I cannot see how metal can be fatigued, or in any way affected by

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strains so far within the elastic limit. There is so far not the slightest evidence for it. With ample allowance for impact in certain bridge members, I believe that uniform unit strains can safely be used throughout for members of ordinary spans and modified, respectively, for tension or for compression. It simplifies the method of dimensioning very much. All compression members in a bridge should be assumed pin bearing at both ends, because in practice the conditions for fixed end bearings cannot be realized with certainty. The formula for compression, based on the equation of a straight line, is all sufficient for bridge work, as most practicing engineers will acknowledge.

I firmly believe, that if ever breaking tests were made with full sized pin connected trusses, failure would result from the collapse of the compression members long before any tension member would fail. In other words, the factor of safety (to use this convenient if incorrect term) would be found to be less in compression members than in the tension members. This ought to caution us not to go beyond a moderate ratio of $\frac{l}{r}$. It should not exceed 50. It always seemed to me, that in many of the high trestle viaducts, the long slender columns are of meagre safety, although we may show a theoretical justification for so scantily dimensioning these structures. We are taking chances, perhaps not big chances, but still chances, that we do not take with tension members. A slight lateral force from wind, from a flying tree branch, or slight defects in riveting up the composite pieces of a column, may reduce the safety in such a member far beyond what it is calculated for. For this reason I should, as mentioned before, make it a rule to use as low a ratio of $\frac{l}{r}$ as possible. In regard to other unit strains, the designer will find it necessary and proper in longer span bridges, say over 400 feet, to deviate from hard and fast rules of dimensioning and to exercise his best judgment for the sake of economizing metal. The difference between members having to sustain large dead load strains, and others with no such strains; or between members with cumulative strains and members subject to sudden strains, cannot be expressed by any arbitrary rule or formula for dimensioning. In larger structures it is necessary to discriminate and to use skilled judgment for that purpose. In this, as well as in the designing of details and connections, the aptitude of the best designers will gradually bring about a certain uniform practice which at present does not exist, although we are approaching it.

WILLIAM H. BURR, M. Am. Soc. C. E.—Mr. Waddell's adverse criticism of the use of engine concentrations in the calculation of stresses in bridge trusses is, I think, well grounded. The amount of labor involved in the determination of stresses, with the almost infinite number of different sets of concentrations and wheel spacings which are used in the

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formation of the diagrams usually prescribed by railroad engineers for the governing of their bridge designs, is certainly most excessive for even the best of computers, except, possibly, now and then one with a most rare mental equipment. Even this requirement of excessive labor might be borne with some patience and equanimity, if the resulting computations were entitled to any greater degree of confidence; but as a matter of fact, every engineer knows that locomotive concentrations and wheel spacings for the engines actually constructed upon any one given road will vary from season to season, or even perhaps in the same season; hence if any standard engine or set of standard engines be prescribed with wheel weights to the pound and wheel concentrations to the hundredth of a foot, it is almost a certainty that no locomotive will ever pass over that road and produce the same stresses as that shown with such extraordinary accuracy in the specifications. It is extremely probable that within a short time after the issue of the specifications the load shown will be considerably exceeded, and just why stresses which are absolutely certain to be considerably in error should be computed with such distressing accuracy, is not very clear.

Again, if it were among the possibilities that the bridges of any road should carry precisely the locomotives prescribed in the specifications, it is an absolute certainty that the variations in track conditions from one portion of a season to another portion of the same season, under the rapid passage of trains, will produce vastly greater variations from the stresses so accurately demanded, than those computed from a simple uniform load or from the latter, headed by a single concentration. Again, the sections of members designed to meet the requirements of a given stress diagram are never mathematically equal to the total stresses divided by the unit stress; nor is it of the slightest consequence that they do so vary; and it is not infrequent that the percentages of such variations are considerably greater than the variations of stresses due to a uniform load from those produced by concentrations. It is therefore most unreasonable and most unscientific that such refined and determined efforts should be made to secure extraordinary accuracy in one direction, only to be completely obliterated and displaced by very considerable but absolutely unavoidable errors or inconsistencies in other directions.

It is to be hoped that the observations of Mr. Waddell will induce engineers to give this matter of bridge loading that thorough and common sense treatment which it has never yet received, and without which numbers of long-suffering bridge companies' offices have endured untold torments of excessive computation. Nothing is to be gained by this figment of ridiculous refinement; in fact, much is to be gained by its relegation to obscurity. A solacing memory will always be preserved for its admirers by the awe-inspiring literature which Mr. Waddell has pointed out, and which has been written to show what splendid mathe-

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matical gymnastics can be performed in its treatment. But it can be confidently asserted that no single structure has ever been made a shade better for its purpose, or more creditable in its design by the use of the method.

I do not concur with Mr. Waddell in his suggestion of displacing this over-refinement by what seems to me to be another almost if not quite as bad. If equivalent uniform loads are to be used, then some established position or condition of the engine concentrations must be assumed in reference to the trusses to which they are to be applied, and the load equivalent to them thus placed be found; or else there will be numberless equivalent uniform loads to be determined for each locomotive system. As a matter of fact, there is no such thing as an equivalent uniform load for a given system of locomotive concentrations followed by a uniform train load. Every position of the latter load for every different shear and every different chord stress in a given structure has its corresponding load; that is, such a uniform load as will produce the same shear or the same bending moment as the system of engine concentrations for the position in question. It is true, as Mr. Waddell observes, that such an equivalent uniform load may be found as will make all the truss stresses for a given bridge vary not more than 2 or 3 per cent. from those caused by the concentrated load, but a simple uniform load with a single weight at its head equivalent to the engine excess or excesses, will produce a system of stresses with at least the same degree of accuracy, and will thus avoid the tedious computation of an equivalent uniform load.

I see no reason why Mr. Waddell should not go one step further and abolish the equivalent uniform load at the same time that he relegates the engine concentrations to uselessness. The engine or engines which are taken at the head of any prescribed moving load will give a certain excess over an equivalent length of uniform train load. Now, if this excess, considered as a single weight, be placed at the center of gravity of the locomotive or locomotives, the resulting truss stresses will be essentially identical with those caused by the actual prescribed locomotives and train. But if, instead of taking the trouble to find by computation the center of gravity of the locomotive or locomotives, we place this excess as a single weight at the head of the train, we shall make a very slight error only on the side of safety for all the web members, and an equally small error also on the side of safety for the chord members, if we place the uniform load over the whole bridge and then place the single weight in succession at all the panel points of the span. I say place this excess at "all the panel points of the span," for the reason that in all cases the locomotives ought to be preceded and followed by the uniform train load, although this is not often done. If it is not done, an actual condition of loading is ignored, and the resulting error is on the side of danger.

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All the computations required for the truss stresses with such a uniform load and a single excess, as is here advocated, involve only the simplest possible stress computations, which can be done with great rapidity by any one who has even a slight knowledge of the rudiments only of the theory. It is rational and scientific in that the locomotive excess is recognized and provided for in its proper position; at the same time, every possible degree of accuracy which can be attained by any system of computation is also secured. Finally, it avoids entirely the necessity of computing the purely imaginary quantity called the "equivalent uniform load." If there is any valid objection to the uniform load with a single excess found in the manner indicated, I have not yet been able to discover it, and I have never heard it advanced.

In the preceding discussion I have confined myself to truss stresses only, as it seems to me that a system of concentrations should be provided for the design of the stringers and floor beams of a railway bridge. I do not believe it is necessary to prescribe actual locomotive diagrams for such a purpose, although even such a diagram does not involve an unreasonable amount of labor for the purposes of the short plate girders of a railway bridge floor system. I should say that the essential purposes of any system of locomotive concentrations can be secured by a set of weights, three or four in number, as the case may be, which, while arbitrary to a certain extent, can be made easily equivalent to the general type of locomotives in use on any given road. I do not, however, consider this feature of bridge computations a very important one. The calculations involved for a floor system under any system of locomotive concentrations, may be considered as too small in amount to merit any very serious discussion. It would simply render complete any simplification for bridge loads by using some such arbitrary system of weights as I have just indicated.

I heartily endorse all that Mr. Waddell states in regard to the use of steel. Whether it will be the material to be used in all bridge spans, both short and long, is simply a question of cost. Whenever a short span steel bridge can be produced at the same or less cost than one of wrought iron, the steel will certainly displace the latter. I am aware that my confidence in the use of steel is not shared by many members of the Society, but I am forced to my convictions by my experience with the metal. I have frequently subjected steel plates and rivets to almost every conceivable manner of abuse by hammering at blue heat and when cold, with the invariable result of increasing my confidence in and respect for the metal. I am now speaking of steel with an ultimate resistance of not over 65 000 pounds per square inch, produced by the open hearth process, and with phosphorus not more than about six or seven hundredths per cent. It is true that I have seen counter-sunk heads torn off by a very dull flogging chisel under the blows of heavy sledges, but I have also seen the best wrought iron rivets torn off in the

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same way with a considerably less number of blows from the same sledges.

The processes for producing structural steel are now so completely under control that it may be produced with the same uniformity as, or greater than, that of wrought iron, in the same shapes or bars. The effects that are liable to be produced by the various stages of shop processes, and their influences, are so well understood at the present time that I know of no reason why steel members should suffer under them any more than those of wrought iron; and numberless experiments upon the effect of punching, have shown that structural steel of the grade described is less injured than the best of wrought iron used for structural purposes. It is true that cracks must be guarded against, for when once started, if they receive no further attention, their limits are indeterminate; but it is quite true that no process of shearing or punching with tools in anything like respectable condition will start such cracks, and it is not by any means difficult to discover them, even when punching or shearing abuse has brought one into existence.

In the miscellaneous exigencies of railway and machinery practice it has been found that when a metal is demanded that shall possess the longest life under the most severe loading and use, wrought iron must be displaced and steel used; and the results of my experience have convinced me that resort may be wisely made to the same material of proper grade in bridge construction. If the so-called mysterious fractures which occur in wrought iron, and in the best grades of that material, were as much talked about and as thoroughly written up as those occasional ones in steel, I have no doubt that general confidence in the latter material would be much less impaired than it is at present. I have known so many failures of wrought iron members for various purposes which certainly ought to be put in the same category as those so-called "mysterious failures" in steel, that I think fibrous wrought iron must be classed as at least as uncertain as homogeneous steel. It matters little whether from the producer or the puddling furnace, to the finished bridge member, steel or iron requires the greater amount of inspection; in either case only the most ordinary and reasonable scrutiny is required.

The subject of permissible working stresses for steel is one which can scarcely be properly covered within the limits of this discussion; but I would say approximately that for the grade of structural steel under consideration, the working stresses should not be more than 20 to 25 per cent. in excess of those for wrought iron. The views of wind pressure and resulting stresses taken by Mr. Waddell strike me as being a little too severe. Strictly speaking, we know actually nothing of the quantitative characteristics of wind pressure on large surfaces, but we have every reason to believe that they are much less than ordinarily assumed in bridge specifications. The only possible exception to this statement is that of the pressure of the most intense cyclones; and

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even in such cases the highest pressures probably do not cover more than a portion of the surface enclosed in the outline of a truss 500 feet in length.

I think the suggestion to stiffen the two lower chord end panels at each end of a span, is one that should be followed; but I do not believe it is correct to consider the end post in the computation of portal stresses as resting upon a pin at its lower end. It may not be, and probably is not, strictly a flat end column, but it is quite certain that it is not a pin end column. I believe it to be much nearer the former than the latter, and would so consider it in all cases, as being a sufficiently close approximation. I believe also that so long as the wind stresses in the lower chords of bridges do not exceed three-eighths of those caused by the fixed and moving load, they may be neglected. In computing these stresses, however, for the reasons already given, it does not seem to me essential to consider the transferred truss and moving loads, either for the stringers or the lower chords. The ordinary method of designing stringers and floor beams, gives a sufficient margin of safety over and above the nominal working stresses to provide for any excess due to transferred loads from wind pressures; and although the method is not strictly scientific, it involves no appreciable waste of metal, is sufficiently near for practical purposes and avoids useless refinement and complication. Regarding the wind loads in the upper chords or trusses, I can see no improvement over considering them carried by the upper lateral bracing directly to the portals, and through the portal posts to the wall plates.

L. L. Buck, M. Am. Soc. C. E.—The title of Mr. Waddell's paper is somewhat of a misnomer, inasmuch as many of the points considered are not disputed. I intend, only in discussion, to briefly state my own opinion regarding a few of those points upon which I do not agree with him. The question of using equivalent live loads, considered as uniformly distributed, might, in some cases, simplify computations; and might not be objectionable if we were sure that the computation of stresses would always be performed by competent men, who would carefully watch all changes in distribution of live loads and estimate the proper uniform equivalents. But much of the success which has attended American bridge engineering, has been due to men who were willing to devote the labor necessary to following through, in detail, the stresses produced by the various distributions of the live loads; and it will be an unfortunate condition which shall drive such men from this work, until we have arrived at more permanence in the various classes of loads, and construction has reached a point at which further improvement is not needed. That point is not yet in sight, and more hard work will be necessary before we reach it. When Mr. Waddell comes to the subject of "wind pressure," he appears to have adopted a more complex treatment. Here we are dealing with a force whose effect upon surfaces of large extent we have no means of accurately gauging,

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and it is difficult to see how a suitable gauge can be constructed. But it is safe to assume that rules regarding it which have proved safe during the years that they have been in use, will continue to be so in the future.

As to the styles and proportions of bridges, there are several forms of truss which have given good results. The riveted lattice can be made very efficient, and in its construction there appears to be no objection to multiple triangulation. The Pratt truss, with vertical intermediate and inclined end posts, has undoubtedly met with the greatest favor for railroad bridges. It gives reasonable simplicity of construction and promptness of erection. In arranging the trusses my preference is for single triangulation, and, within reasonable limits, to use as long panels as the length and depth of span and proper inclination of the diagonals will allow. In this connection it does not appear clear why Mr. Waddell proposes to increase the width of the body of the lower chord eye-bar, as he increases the length of the panel. As the span is increased it is necessary to use larger bars to avoid too great length, in proportion to diameter, of pin. But there is no economy in attempting to make the bar act as a beam in sustaining its own weight. The nearer it can come to assuming the catenary due to its own weight and the tensile stress upon it, without acting as a beam, the better. Stiff lateral diagonals doubtless give greater lateral rigidity to the track floor, and if secured to the lower flanges of the track stringers where they intersect them, are economical. It is desirable, as far as possible, to have the lines of the diagonals pass through the intersections of the axes of posts and chords. But it is difficult to accomplish this where eye-bar chords are used—practically, it is apparently impossible. Stiff or riveted-up chords lend themselves to this purpose more readily, and in spans up to 175 feet, where often the end panel and that next to it are made stiff, it might be well to consider the advisability of making the chord stiff throughout.

The suspending of the floor beams of iron truss bridges under the lower chord is objectionable generally, though in the case of draw-bridges it has the advantage of allowing the lower chords to have nearly as great exposure to the direct rays of the sun as the upper, and thus, in a measure, preventing the diurnal fluctuations of the elevations of the ends. The spaces between center lines of iron or steel track stringers is important. If too near together, they are not only dangerous in case of derailment, but the rolling tendency of the locomotive when laboring, and of the cars from wind pressure, subjects them to a greater stress than is often taken into account. My own practice is to place them 7 feet apart. Regarding their fastening to the beams, I have never known any to get loose when secured to the beam webs in the usual manner with a sufficient number of rivets. If the stringers have a sufficient depth, the curvature of their deflection will not exceed that of the span sufficiently to produce any serious stress upon the connecting angles.

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In re-enforcing the upper flanges of transverse beams, where the tops of the stringers are below the tops of the beams, I have sometimes used a piece of channel with trough side up, and its web riveted to the beam flange, instead of a cover plate, and laid a piece of timber in the trough, with its upper surface level with that of the cross-ties. This gives less thickness to rivet through, gives greater lateral stiffness to the flange, and permits of leaving a space between the beam and the nearest cross-tie on each side.

The intensity of unit stresses is an important one. It would be still more so if we had arrived at a maximum intensity of live load, but at present a small unit stress for the live load appears desirable, for the reason that we may reasonably anticipate that increasing intensity of the live loading will make its unit stress large enough in the near future; and if we can deceive ourselves into the belief that a very small unit stress for the live load in proportion to that for the dead load is necessary, our bridges will last the longer for it.

All these questions are so interesting, and so many of us have learned to look forward to continued growth in knowledge regarding them, that we cannot avoid hoping that their final settlement for all time may not be so near as the final clause of Mr. Waddell's paper appears to indicate.

J. B. JOHNSON, M. Am. Soc. C. E.—It would seem that Mr. Waddell has made out a satisfactory case against the further use of wheel load systems in the designing of simple railway truss bridges. Although the technical schools have not in any way been responsible for the introduction of these laborious methods, they have tried faithfully to adapt the instruction in this branch of engineering to the requirements of actual shop practice, and are therefore largely responsible for the great elaboration of many of the details and graphical expedients which now are recognized as essential features of them. The professors in these schools will, however, welcome a return to shorter and simpler processes if they can be shown to give satisfactory results. The two years which our engineering schools can give to imparting strictly professional instruction should be reserved, as far as possible, for the teaching of principles, and as little time given to methods as is consistent with a fair degree of efficiency in field and office on graduation.

An entire abandonment of the concentrated wheel-loads processes would result in a considerable saving of time, which might be more profitably spent in the further study of principles. It has been the writer's custom to prove theoretically to his classes that if a uniform loading be taken, giving the same bending moment as the wheel loads at the "quarter point," or at the joint nearest to this point, the resulting stresses in all members will very closely approximate to those from the actual wheel loads moved across the bridge. Mr. Waddell's table on page 11 is a further proof of this fact. Omitting his 100-foot truss span,

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of four panel lengths, as being too short for such a comparison o methods, but taking his other lengths up to 300 feet, we have:

Load.	150-foot Span.	200-foot Span.	250-foot Span.	300-foot Span.
Mr. Waddell's average unit load.....	3215	3151	3112	3085
Unit load at quarter point.....	3214	3120	3107	3094
Percentage of difference.....	0.0 per cent.	1.0 per cent.	0.2 per cent.	0.3 per cen

It would appear, therefore, that the "quarter point" might well b taken as the point at which to compute an equivalent uniform load giving the same bending moment as the maximum moment from con centrated or wheel loads. The equivalent uniform load per foot fo this position is

$$w = \frac{32\ M}{3\ l^2} \quad \text{.....(1}$$

where m = maximum moment at the exact quarter point for wheel loads This equation to be used where the number of panels is a multiple of fou

If the quarter point does not fall at a panel point, or when th number of panels is not a multiple of four, then take the nearest pane point, preferably that one towards the center from the quarter point and use the following equation:

$$w = \frac{2}{m\ (n-m)} \cdot \frac{M_m}{p_2} \quad \text{.....(2}$$

where w = equivalent uniform load per foot.

n = number of panels in bridge.

m = the number of the panel point taken, counting from th near end and calling the end support zero.

M_m = maximum moment at the mth joint for the concentrate wheel loads.

p = panel length in feet.

Equation (2) reduces to the form given in (1) when n is a multipl of four.

Floor Beam Concentrations.—Mr. Waddell seems to have never rea Professor Swain's paper on "Stresses in Bridges" in the *Transactio* Am. Soc. C. E., Vol. XVII, where, on page 37, the methods of findin floor-beam concentrations from the equivalent uniform loads, redi covered by Mr. Waddell, is fully explained.

Fiber Stress in Eye-bars from their own Weight.—Mr. Waddell, i common with many other bridge engineers, opposes the use of lon shallow bars because of the unknown fiber stresses caused by the dea weight of the bar. This is an interesting question, and so far as th writer is aware it has not been investigated. When an eye-bar is pulle to its working limit, it is still bent downwards by the full force gravity upon it. But when it sags from a straight line, the pull on th bar, as well as the bending stresses, tend to hold it straight. There ar

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therefore, two external forces acting on the bar, one tending to deflect it and the other tending to hold it straight. The residual bending moment is held in equilibrium by the resulting moment of resistance in the bar.

If a section be taken at the center of the bar, we have—

$$\frac{Wl^2}{8} - Pv = \frac{fbh^2}{6} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (3)$$

where w = weight of bar per inch.

l = length in inches.

P = total pull on bar.

v = deflection of bar at center.

f = fiber stress due to this deflection.

b = breadth of bar.

h = height of bar.

But $w = 28\,bh$

$P = pbh$, where p = working tensile stress on eye-bar.

$v = \frac{5}{24}\frac{fl^2}{Eh}$ where E = modulus of elasticity = 28 000 000; whence we have—

$$f = \frac{4\,700\,000h}{p + 22\,400\,000\left(\frac{h}{l}\right)^2} \quad \ldots\ldots\ldots\ldots\ldots \quad (4)$$

If, in this equation, the first differential co-efficient of f with respect to h be put equal to zero, we obtain, for the depth of bar giving maximum fiber stresses—

$$h = \frac{l}{4\,730\sqrt{p}} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (5)$$

If this value of h be substituted in Equation (4) we have, for the maximum fiber stresses for those depths given in Equation (5)—

$$f \text{ max.} = \frac{500l}{\sqrt{p}} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (6)$$

From Equations (5) and (6) the following table of depths giving maximum fiber stresses, and the corresponding stresses per square inch on the extreme fiber, due to bending only, has been computed:

TABLE OF DEPTHS AND MAXIMUM FIBER STRESSES.

Working Tensile Stresses in pounds per square inch.	Length of Eye-bars in Feet.											
	15		20		25		30		35		40	
	Depth.	Fiber Stress.	Depth.	Fiber Stress.	Depth	Fiber Stress.	Depth.	Fiber Stress.	Depth.	Fiber Stress.	Depth.	Fiber Stress.
	In.	Lbs., Sq. Ins.	In.	Lbs., Sq. Ins.	In.	Lbs., Sq. Ins.	In.	Lbs., Sq. Ins.	In.	Lbs., Sq. Ins.	In.	Lbs., Sq.Ins.
8 000	3.4	1 010	4.5	1 340	5.7	1 680	6.8	2 020	8.0	2 350	9.1	2 690
10 000	3.8	900	5.1	1 200	6.3	1 500	7.6	1 800	8.9	2 100	10.1	2 400
12 000	4.2	820	5.6	1 100	6.7	1 370	8.4	1 640	9.7	1 920	11.1	2 190
14 000	4.5	760	6.0	1 020	7.5	1 270	9.0	1 520	10.5	1 780	12.0	2 030
16 000	4.8	720	6.4	960	8.0	1 200	9.6	1 440	11.3	1 680	12.9	1 920

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For other depths, lengths, or tensile stresses, use Equation (4).

From this table it is evident that the depths producing maximum bending fiber stresses are in general those in common use. Also that the bending stresses in the extreme lower fibers, which are to be added to the working stresses, are not inconsiderable, but will vary between 10 and 20 per cent. of the working stresses in lengths of from 15 to 30 feet.

HENRY T. EDDY, C. E.—This paper is a move in the right direction. If an agreement can be secured among bridge experts as to the various points noticed in this paper, it will, I am sure, mark a distinct advance in bridge designing. I am especially interested in that part of the paper which relates to computation and to the adoption of some limited list of typical loads. An agreement as to the amount and distribution of the wheel concentrations which bridges shall be designed to carry, is wanted by those who advocate the employment of equivalent uniform loads, as much if not more than by those who propose to compute directly from the wheel loads themselves; for the only basis for these equivalent loads is the amount and distribution of wheel concentrations in place of which they are taken.

It seems to me that Mr. Waddell's reference to my paper is based upon a misconception which I desire to remove, and at the same time assist, if possible, in deciding the question whether computations may be best based upon assumed wheel concentrations or upon so-called equivalent uniform loads. The sole advantage of employing uniform loads is found, I take it, in the ease and simplicity of computation. The disadvantages are, that the equivalent load is only approximately equivalent, and owing to the way it is found, the degree of approximation and amount of the error are unknown and in fact vary from point to point of the span. Furthermore, the magnitude of the equivalent load varies for a given train with the length of the span. The general opinion at present is, that the principal members of long span bridges can be computed satisfactorily from equivalent uniform loads; but in short spans and in stringers and other details of long spans, where the wheel concentrations exert a more preponderating influence, I am led to believe that it will still continue to be required that the designer and computer shall take account of the wheel concentrations, however much he may dislike the work, or however certain he may be in his own mind that it involves unnecessary labor.

It was in view of a probable continued use of concentrated loads that I attempted* to explain graphical methods of treating such loads, methods which I think I am justified in saying are as simple and as expeditious as that of equivalent uniform loads, but which involve no approximation whatever. If I have succeeded in doing this, then the whole burden of objection urged against employing concentrated loads

* *Transactions* Am. Soc. C. E., May, 1890.

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vanishes. As I understand it, computers would gladly use concentrated loads in case the labor were no greater than in using uniform loads. Now I distinctly claim that the methods proposed in my paper enable the computer to do this, and I have only to refer in corroboration to those who have tried both methods. Notice, however, that I do not claim at all that the algebraic proofs which I have given of these methods are in any sense simple or expeditious. In all candor I must acknowledge that I could wish these were less tedious, and I am sorry that I have been unable to discover less intricate demonstrations of these simple and convenient processes. I am well aware that such demonstrations are likely to stand in the way of the ready diffusion of a knowledge of the methods proposed, for no engineer will be willing to adopt methods of whose truth he is not satisfied. I trust that more simple demonstrations will in time be found, or that the ability to read mathematics will become so general as to do away with this obstacle.

The misconception into which I think Mr. Waddell has fallen respecting my paper, is in supposing that the formulas I have used were intended for computation. Such is not the case; they are simply used, most of them, to convince the reader once for all of the correctness of the process he is directed to use, otherwise they might have been omitted. There are, however, certain side developments in my paper, intended specially for those who wish to compute by the method of equivalent uniform loads. The developments I refer to are simple approximate formulas for finding equivalent uniform loads, and the approximate maximum shears at the head of a train. These formulas derived on mathematical principles permit us for the first time, so far as I am aware, to do the very things which Mr. Waddell desires to do, and to do it in some other way than by "cut and try." It may become possible to gain some idea how closely the results of such a formula approximate to those obtained from the concentrations themselves.

EDWIN THACHER, M. Am. Soc. C. E.—*A.* I fully agree with the author, that the substitution of equivalent uniform loads for wheel concentrations in bridge calculations is a great saving of labor, and also that the results are practically as accurate. I have calculated stresses both ways, for hundreds of bridges with all the aids to calculation that are available, and I should judge that the time required for the calculation of stresses, using uniform loads, does not exceed one-tenth the time required if wheel loads are used; in fact, with a good slide rule the stresses can be found, using uniform loads, while one is getting ready to make a start with wheel loads. It may be asked, if the results are practically as accurate by uniform loads, why not always use them? One reason for not doing so is that an engineer naturally has a pride in having his strain sheet conform strictly to specifications; but another and the principal reason is that wheel loads give somewhat less material in the chords and counters, and under close competition economy is necessary. For spans

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up to 150 feet the difference is not very important, but for long spans it is quite considerable. If the train load precedes as well as follows the engines, however, which is not uncommon, the stresses by uniform and wheel loads for spans of any length, including cantilevers, give results almost identical.

As I have already in previous discussions given my views quite fully on the question of uniform *versus* wheel loads (see "Specifiations for Strength of Iron Bridges," by Joseph M. Wilson, M. Am. Soc. C. E., 1886, and "Stresses in Railway Bridges on Curves," by Ward Baldwin, M. Am. Soc. C. E., 1891), I do not wish to make any repetitions here. In finding the stresses by wheel loads or in making a table of equivalent uniform loads, the first thing to do is to prepare a moment table, viz., a table giving the moment about each wheel of all preceding wheels. With such moment table at hand, it is a matter of only two or three minutes to find the maximum end shear on a stringer or plate girder, and I have never felt any great need for a table to cover this case. It would be well, however, when preparing tables of uniform loads corresponding to maximum moments, to make one also corresponding to maximum shears, and thus dispense with moment tables altogether. The method given by the author for finding the concentrated load on floor beams and suspenders by using the equivalent load corresponding to a length of two panels, is not new. It has been used extensively by me for eight or ten years. An example of such application may be found in his paper, *Transactions* Am. Soc. C. E., May, 1884.

The author appears to have based his equivalent loads for all spans upon the maximum bending moment. I do not think it advisable to do this for spans exceeding 90 to 100 feet in length, or for spans greater than the longest plate girder used. A latticed girder I would never use unless forced to do so, and for trussed spans I consider it preferable to base the equivalent load on the maximum shear, in which case the stress in end brace and end chord by uniform load and by wheel loads will be identical, the stresses at center of chords and in counters being slightly in excess by uniform loads. The results will vary somewhat with the number of panels used, so that in preparing a table for spans of 100 feet and upwards it would be advisable to give a choice in a number of panels.

In the general specifications of the Keystone Bridge Company are given diagrams and equivalent uniform loads for ten different classes of engines. For spans up to 70 feet, the loads correspond to maximum moments; and for spans of 75 feet and upwards, to maximum shears. Since this table was prepared (1887), some of these loads have dropped out of specifications, and others have come in, and the table could be profitably revised accordingly; but a choice of ten different and well selected classes of loading, it appears to me, ought to satisfy any railway company. I consider that the calculation of stresses by uniform load is

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really better practice than by wheel load; for it not only saves nine-tenths of the time in calculation, but is always safe, and provides, as before noted, for a possible condition of loading not usually provided for when wheel loads are used. When required to make a large number of estimates in a very limited time, as all bridge calculators frequently are, I always use uniforms loads, let the specifications be what they may, as it becomes simply a matter of choice to do this or not to bid on the work. There is no question, in my mind, but what uniform loads could be adopted in railway bridge specifications with great profit to the bridge companies and without injury to the railway companies.

I do not think the typical live loads proposed by the author the best selection that could be made. I would prefer to take the actual engines and loads in use and specified by railroad companies as far as possible, say the following selections:

1. Erie. 2–80.75 ton consolidation engines followed by 2 240 pounds per lineal foot.
2. B. & O. 2–86.0 ton consolidation engines followed by 3 000 pounds per lineal foot.
3. P. C. & St. L. 2–92.3 ton consolidation engines followed by 3 000 pounds per lineal foot.
4. L. & N. 2–97.75 ton consolidation engines followed by 3 500 pounds per lineal foot.
5. Penn. 2–100.0 ton consolidation engines followed by 4 000 pounds per lineal foot.
6. E. T. V. & G. 2–105.0 ton consolidation engines followed by 4 000 pounds per lineal foot.
7. R. & D. 2–112.0 ton consolidation decapod engines followed by 4 000 pounds per lineal foot.
8. Lehigh. 2–126.0 ton consolidation engines followed by 4 000 pounds per lineal foot.
9. L. & N. 1–80.5 ton, 8-wheel engine, 68 000 pounds on drivers, 9 feet centers.
10. Penn. 1–89.0 ton, 8-wheel engine, 80 000 pounds on drivers, 8 feet centers.

Loads 9–10 apply only to cross-ties and short spans.

Railroad companies are not likely to change their specified loads unless they find it to their interest and advantage to do so; neither do they care how much work and expense is borne by a bridge company in estimating on their work, so long as they do not have the bill to pay. For these reasons I do not think it probable that the author's proposed loads will be adopted to any great extent, and if not adopted the tables prepared therefrom would be useless. On the other hand, tables of equivalent uniform loads, corresponding to the engine and train loads above given, would be very useful to all calculators and a valuable contribution to bridge builders, independent of any action that may be taken by

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the railroad companies; for they could be safely and profitably adopte by the calculators in all cases for spans up to a 150 feet; and if railroa companies would provide (as does Mr. Bouscaren in his specifications for a train load to precede as well as to follow the engines, they woul get a better bridge, and calculators would be relieved of a great burden as uniform loads can then be adopted in calculation for spans of an length. Railroad companies can then continue to specify wheel load if they prefer to do so, and all parties will be satisfied.

B. For wind pressure, I believe an allowance of 150 pounds pe lineal foot for unloaded chords, and 450 pounds per lineal foot for loade chords for spans up to 200 feet, is ample for ordinary cases. Fo longer spans greater allowance should be made, and for special cases c double decks, high fences, etc., 30 pounds per square foot of expose surface, appears to me sufficient. To resist these forces, I woul allow 20 000 pounds per square inch on iron lateral rods. It is not likel that one bridge in ten thousand is ever exposed to a wind force of 3 pounds per square foot, or one bridge in a thousand to a force of 1 pounds per square foot. Even if it should be exposed to a forc greater than 30 pounds, the bottom chords of the great majority c trussed bridges would be buckled before the stress in the rods reache the elastic limit. My ideas on the wind question are given quite fully i discussions of Paper No. 335, Volume XV, wherein is given an examp of a 200-foot span through bridge. In this example, the trusses are 1 feet 9 inches center to center, better designed than the average throug bridge and far better than the average deck bridge to resist wind; y it is found that a force of 20 pounds per square foot will buckle th second panel of the bottom chord; a force of 23 pounds, the thir panel; a force of 31 pounds, the fourth panel; a force of 35 pounds, th fifth panel; and a force of 39 pounds, the center panel.

Specifications frequently make excessive provision for wind an initial stress, which, if observed, leads to much waste of material. M Cooper, who is entitled to much credit for originality, and whose spec fications bear evidence of careful study, makes a very wild provision fo the force of wind on iron trestles, and if any trestle has ever been bui to conform therewith, I would be glad to make a note of it. Applied t a recent estimate for a trestle 60 feet high, with alternate 30 and 37.5 fe spans, the bents should have a width at base of about 109 feet to pr vent tension; or, allowing the usual batter of one horizontal to six ve tical, the tension will be sufficient to lift 280 cubic feet of masonry—a absurd result in either case.

C. I would leave the style of bridge to the designer, as nearly an style can be made to do its work well. Plate girders much exceedin 90 feet in length are troublesome in most shops and liable to injury i transportion and erection. Pony truss railroad bridges are not calle for of late years to my knowledge, and many of the old ones have falle

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down. They may be considered structures of the past. I prefer to have floor beams riveted to vertical posts in through bridges, provided the rivets are in double shear; but floor beams suspended by plate hangers admit of good details and a quite rigid lateral system. I do not see the force of the author's remarks that, in the subdivided Pratt truss, rigidity is increased and the top chord sections are more perfectly supported, by sub-strut than by sub-tie. I prefer the tie for appearance sake, though in all comparisons made, the strut is least expensive, as stress travels in a much more direct route to the supports.

If the width between centers of trusses in long span bridges is one-twentieth of the span, the span becomes practically a trussed column of 20 diameters, and the top chord sections (whatever their dimensions) should not be proportioned for a less number of diameters. This consideration alone is sufficient to govern the width between trusses after obtaining the necessary clearances. No doubt an increase of minimum clear width to 14 to 16 feet, would make a safer bridge, but it rests with the railroad companies to decide whether the extra safety is worth the extra cost. I prefer a space of 9 feet between centers of track stringers. This allows guard rail, cross tie, and stringer to be connected conveniently by the same bolt; it is cheaper than any less width, and less effect or inequality of stress is produced by train oscillation. It requires a heavy timber floor, which is an advantage and materially assists the bottom chord to resist reverse stress from wind, and we have found this needed if only 30 pounds per square foot pressure is specified.

D. I doubt whether steel will ever be used in bridge work more extensively than it now is. There is no economy in reamed steel for small trussed spans. Steel of the mildest grades is less reliable than iron. In the dropping or overturning of girders in wrecks, careless handling, or abuse of any kind, steel suffers much more severely than iron. Steel is unquestionably safe under ordinary conditions of stress, but I do not believe it can ever be made the equal of iron under all conditions.

As remarked by the author, the intensities of working stresses that should be used on bridge material remains much a matter of individual judgment on the part of the engineer. The author appears to have adopted for tension members the fatigue formula recommended by Launhardt, instead of a modification providing for impact, and asserts that a formula involving the factor $\left(1 + \frac{\text{minimum}}{\text{maximum}}\right)$ is surely incorrect. I do not agree with him. Whether bridge material does or does not suffer fatigue is a matter of no great consequence. All engineers are agreed that greater provision should be made for a live load than for a dead load, and my own opinion is that about double provision should be made; as, for example, I consider a member subjected to a constant

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stress of 15 000 pounds per square inch, as safe as a counter rod in a bridge figured for 7 500 pounds per square inch. Having fixed on the minimum and maximum limits of stress, it appears to me reasonable to draw a straight line between them, and let all intermediate values of $\frac{\text{min.}}{\text{max.}}$ be found on this line; the formula does this and nothing more.

If an engineer considers that members subject to dead loads should be allowed only 25 per cent. greater stress than for live load, he can use the formula $a\left(l + \frac{\text{min.}}{4\text{ max.}}\right)$.

If he wishes to allow 50 per cent. greater stress for dead than for live load, then he can use $a\left(l + \frac{\text{min.}}{2\text{ max.}}\right)$.

If 100 per cent. appears correct, he can use $a\left(l + \frac{\text{min.}}{\text{max.}}\right)$, as I make a practice of doing.

These formulas recognize fatigue only to a small extent. The formula $7\,500\left(l + \frac{\text{min.}}{\text{max.}}\right)$ for iron tension members appears to me good enough. Mr. Cooper allows 8 000 pounds for all live load and 16 000 pounds for all dead load, which gives a variation from the formula of 6 per cent. at extreme limits and less than this at all other points. The application of Mr. Cooper's specifications in their present form are burdensome to computers, owing to the necessity of finding the stress for dead and live load separately for each individual member. This may be avoided, using only maximum stresses, and the same results be obtained by stating the requirements as follows:

Eye-bars..... $P = 16\,000 \div \left(l + \frac{\text{live load}}{\text{max. load}}\right)$

Plates and shapes. $P = 15\,000 \div \left(l + \frac{\text{live load}}{\text{max. load}}\right)$

Chord segments.. $P = (16\,000 - 60\,\frac{l}{r}) \div \left(l + \frac{\text{live load}}{\text{max. load}}\right)$

All posts..... $P = (14\,000 - 80\,\frac{l}{r}) \div \left(l + \frac{\text{live load}}{\text{max. load}}\right)$

The above modification will be found to save considerable labor. The ratio of $\frac{\text{live load}}{\text{max. load}}$ for web members may be taken from the shears; and for chords, from the assumed dead and live load per lineal foot. If the views of S. M. Robinson, M. Am. Soc. C. E., in regard to the effects of cumulative vibrations be accepted as facts, all of our specification will need material revision. The example given by the author of two long duplicate suspension bridges does not sustain his point. The formula which will allow a stress of 40 000 pounds per square inch in the first case is $20\,000\left(l + \frac{\text{min.}}{\text{max.}}\right)$, which for the second

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case allows 33 333 pounds per square inch, or practically if 15 000 pounds is allowed in the first case, 12 500 pounds will be allowed in the second, which appears to me about right. As the second case requires but nine-tenths as much material as the first, it is of course the stronger under the assumed condition of loading. In discussion of Paper 335, Vol. XV, previously mentioned, I proposed the following formula for the ultimate strength of iron columns:

$$\text{Flat ends } P = 45\,000 - 150\,\frac{l}{r}, \text{ Pin ends } P = 45\,000 - 200\,\frac{l}{r},$$

these formulas representing nearly mean results of all experiments recorded and plotted up to that time. I do not think there have been a sufficient number of tests made of the compressive strength of steel bridge members upon which to base formulas, but if we assume that for short blocks the strength is in proportion to the tensile strength of the material, and agree with Mr. James Christie, M. Am. Soc. C. E., that mild or medium steel has the same strength as iron when $\frac{l}{r} = 200$, we readily derive the following formulas for steel columns:

Ultimate strength, 60 000 to 68 000 pounds per square inch or 64 000 pounds per square inch mean;

$$\text{Flat ends } P = 57\,500 - 215\,\frac{l}{r}, \text{ Pin ends } P = 57\,500 - 260\,\frac{l}{r}.$$

Ultimate strength, 68 000 to 75 000 pounds per square inch or 72 000 pounds per square inch mean;

$$\text{Flat ends } P = 65\,000 - 250\,\frac{l}{r}, \text{ Pin ends } P = 65\,000 - 300\,\frac{l}{r}.$$

The above formulas both for iron and steel err on the side of safety for very long columns, but as the effect of the weight of the column itself is not always considered in calculation, this is desirable.

E.—I do not consider it necessary to add to the section of bridge members on account of wind stresses unless they exceed the amount due to dead and live loads; for not only is the bridge very unlikely ever to be subject to anything approaching the wind assumed, but should this happen the stress would still be well within the elastic limit, and safe. I have made a practice of adding to the sections of trestle legs when the combined stress exceeded by 50 per cent. that due to dead and live loads only, but believe that 100 per cent. would be better in this case, as there is no merit in wasting good bridge material. In my paper, Vol. XIII, Plate I, *Transactions* Am. Soc. C. E., are given the stresses in three varieties of bridge portals. I investigated the subject at that time and have used the results since. They are correct according to my views, and as I have not seen them in print elsewhere, they may be of some service to bridge designers.

F.—I do not agree with the author that plate girder proportioning amounts to but little more than rule-of-thumb. To be sure, some

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peculiar and untenable theory is occasionally advanced, but in all important particulars I believe the stresses in plate girders are as well known, and admit of as clear demonstration, as in other structures.

G.—I would never use unreamed compression steel of any grade under any circumstances. It may stand the drift test and bend double upon itself, yet, in the careless upsetting of a girder, crack like glass. As regards the use of steel, the question as to the most desirable grade to use has more interest to me than any other; if very mild steel is used in compression, the cost of reaming prevents in a great measure its economic use as compared with iron, and unless some very decided saving can be effected by its use I would much prefer iron. I have favored, both for tension and compression, a steel having a mean ultimate strength of about 70 000 pounds, although I believe a majority of engineers favor a lower grade. I am in favor of the grade mentioned for several reasons; its use effects a material saving in the cost of structures; it can be furnished of uniform quality; its elastic limit and reduction of area are superior to iron, and in cold bending it is nearly or quite the equal of iron, and when annealed after forging and when reamed after punching, it appears to me to be as safe as the milder grades. If this is true, why throw away a strength of 10 000 pounds per square inch? and if not true, I would be glad to hear some good and substantial reasons why. I would be glad to hear an expression of opinion on this important question of economy.

C. L. Gates, M. Am. Soc. C. E.—In the discussion on the valuable paper entitled "On Specifications and Strength of Iron Bridges," read by J. M. Wilson, M. A. S. C. E., in *Transactions*, Vol. XV, 1886, the fact was forcibly brought before the Society by several of our more prominent and well known bridge engineers, that in preparing strain sheets for bridge trusses a great amount of practically useless labor was imposed upon the contracting engineer to provide for the maximum strain caused by any one of the various engine wheel loads specified; and I am glad that Mr. Waddell at this time has again taken the initiative to bring this matter before the Society. Ever since the matter of engine concentration from wheel loads was considered in connection with iron bridge building, it looks as though it had been the ambition of railway engineers in writing up a bridge specification, to adopt an actual or typical engine load differing in wheel base and load from previously accepted ones, and yet in its resulting strains varying but slightly from the engine load of the rival railroad. Yet this slight variation requires an amount of labor not appreciated and hardly compensating for the accuracy in result, all of which Mr. Waddell clearly points out and proposes to remedy in substituting arbitrary formulas closely approaching correct results.

To me it seems we might at this time agree upon three or four typical engines or equivalent concentrated loads covering nearly all cases of

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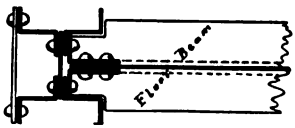
loadings, and I should be pleased did Mr. Cooper so modify his standard wheel load to approach those of Mr. Waddell's proposition, Mr. Cooper's specifications having already so wide a reputation and having been adopted extensively by railroad engineers as a standard. Very likely the gentleman has already anticipated this necessity. I have for the past six or seven years slightly modified such wheel distances and loads for personal convenience in computation without varying from the general result except on the safe side.

Similar conditions of things at present exist in the various compression formulas. Without further touching upon the great divergency of established rules, I simply mention that Cooper's formulas for steel compression members in long length columns give smaller resulting unit strains than for precisely equivalent iron sections, and to this time I have never been able to fully explain the very small unit strain allowed for long posts compared with the large unit strains permitted for end chord sections by the above mentioned author.

With regard to requirements of stiffeners in plate girders it has been my habit to formulate for distances between stiffeners, also for the required area of the same and the number of rivets per stiffener, providing for the intensity of shear at the point in question compared with the ratio between the web depth and web thickness and total net web area, disregarding the usual clause in specifications to space stiffeners at distances equal to the depth of girders and never less than 5 feet apart. I believe, however, that not much is gained by hair-splitting differences in discussion, if we at the same time in designing and building diverge widely from good accepted practice and standard. I am surprised to find at this date in one of our large cities, a long three and four track railway viaduct in process of construction, consisting of 30 feet, 40 feet and longer deck plate girder spans designed for very heavy traffic and engine load, in which vertical web stiffeners for 4½-foot depths have been placed at the certainly extraordinary distance of 15 feet from the end stiffeners. If this viaduct is being built in accordance with modern practice I should like to know it, and have the engineer and designer, a member of our Society to inform us; and if it is not, discussion on the subject would be beneficial and perhaps remedy such incongruity in details in an otherwise careful and skillful design.

Another point I wish to bring up is this: Why are hip joints at intermediate panel points (between the inclined and horizontal top chord sections) in some of our most important long-span structures designed as though the transferred strain were through the "inclined" bearing surfaces instead of through the medium of properly proportioned riveted reinforcement plates and the pin such as we all very properly design for the end hip joint? With reference to general details of



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construction, it has been the writer's favorite custom to connect floor beams to Z (zee) iron posts as per sketch; this connection having the advantage that the beam is attached to the post through its neutral axis, doing away with the diaphragm plate ordinarily used for the purpose of equalizing the load over the two channels of the post. In conclusion, I would suggest that when Mr. Waddell establishes a formula for unit strain in steel under compression he should at the same time specify the grade of steel proposed to be used, a matter of the utmost importance.

CHARLES S. CHURCHILL, M. Am. Soc. C. E.—I am much interested in the paper under discussion for the following reasons:

First.—There is certainly entirely too much variation in the specifications of live loads for new bridges.

Second.—There are no good grounds for this great variation.

Third.—I think that the present varied practice will soon be simplified by the suggested discussion.

Fourth.—This subject has been one of very great interest to me and to all the members of the Engineering Department of the Norfolk and Western Railroad for over three years.

In 1888 it was found necessary to make some very radical changes in our bridge specifications, principally on account of the adoption of heavier engines and cars, and, further, from the discovery that we had ahead of us an extremely large amount of bridge work. At the outset it was found that we could not adopt any special type of engine for a maximum live load, because many changes were probable, and, indeed, were made, before any specifications were completed. It was also thought that the weights of our engines might be further increased in a few years. It was determined, therefore, to adopt the simplest plan of typical loading we could devise, that would secure to us bridges having a proper per cent. of excess strength; and to arrange that loading so that a change in the wheel base of any of our engines or the weight on any wheel would not destroy the adopted per cent. of excess strength of our bridges. The problem was very soon found to be a very difficult one and months were spent upon it.

The attempt was made at first to use a single concentrated head load, followed by a uniformly distributed train load. This plan, when adjusted to give the proper excess strength to bridge members governed by the maximum bending moments, would give entirely too small an excess to the strength of members governed by the maximum shearing forces. The conclusion was reached, therefore, that the proper plan of loading is a series of concentrated loads, followed by a uniformly distributed train load, and it was determined to make this arrangement of concentrated loads as simple as possible. To this end, diagrams were made up showing the equivalent uniformly distributed load per foot for various spans, that would produce the same maximum bending moments and shearing forces as our heaviest engines and train. Engines

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used by other roads were diagrammed in the same manner, also the typical loadings of various bridge specifications. The resulting diagrams were like Plates XIX, XX attached to this paper, which are copies of the Norfolk and Western Railroad standards. Such diagrams not only furnished a means to the making of our conclusions; but, also, means of properly presenting the matter to the management of this road.

On May 1st, 1889, standard specifications were adopted having a very simple type of loading, in form like those of our specifications dated January 1st, 1891, but a trifle lighter. On January 1st, 1891, specifications were adopted, having a loading as follows:

All bridges shall be designed to carry (in addition to the dead load) a live load of 4 000 pounds per lineal foot of track, of which 136 000 pounds is liable to be concentrated on four axles, as in Fig. 1, or 80 000 pounds to be concentrated on two axles, as in Fig. 2.

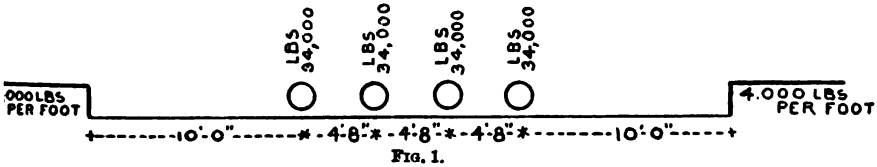

FIG. 1.

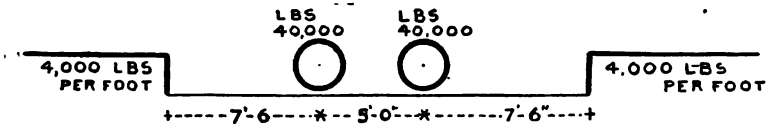

FIG. 2.

Plates XIX, XX clearly show to the eye what a uniform excess of strength we secure by this type of loading; in fact, for all spans the excess strength is practically 20 per cent. A large amount of bridge work has been erected by various bridge companies for the Norfolk and Western Railroad Company under this form of specifications during the last two and one-half years. All the bridge people have expressed themselves as pleased with the simplification secured by this loading; and, I think, we have taken a very long step in the right direction. Although the great simplification gained by doing away with the tender wheel load and adopting a uniform spacing of the four concentrated loads employed, makes the direct calculation of the truss strains a comparatively easy matter, still further simplicity is gained in the calculation of all short spans and plate girders by the use of the equivalent loads appropriate for each particular span as given by the diagrams; and these diagrams having once been plotted, it has been our practice to use the equivalent uniform loads in this manner, with the result of a great saving in time.

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The advisability of removing the tender wheels of the typical loadi was made evident from the fact that the weights on tender wheels are greater than the weights on wheels of loaded freight cars ; and that t increased train load per foot, which it is now found necessary to adc generally, more than covers any concentration to be found under c tenders. This plan of forming diagrams like the above, originated wi my principal Assistant Engineer, Mr. Morgan E. Yeatman. The requi ments of the problem at hand speedily showed to us its uses and brou about the above results. There is no reason why, by the use of su diagrams, a simple type of loading, like that of our specifications, c not be adopted by every railroad company to suit the requirements their traffic. These diagrams are simply graphical representations the tables of Mr. Waddell, and show the same result. I know it is practice of several bridge companies to make just such tables as th of Mr. Waddell to conform to a given specification ; in fact, the ab mentioned diagrams have been checked partly by a table from the E Moor Bridge Works. The rule mentioned by Mr. Waddell for find floor beam concentrations, has been used on this road for the last th years, and, like many other points, the proof of it was discove through the use in bridge calculations of diagrams like those hereto referred to.

I trust this discussion may be continued until it results in the en abandonment of a multiple form of engines for use in bridge speci tions ; and I hope that the above statement will indicate the useless even of the six types of locomotives proposed by Mr. Waddell for purpose. I would recommend rather the use of a uniform load concentrations at not more than four points, which can be so sele as to cover the present and future requirements of the traffic of any road. The loading having been adopted, it is a simple matter to pre diagrams to expedite the practical work of calculation of bridge str

ROBERT MOORE, M. Am. Soc. C. E.—Prof. Waddell's remarks u the unwisdom of adhering to the details of some actual or typical en in determining the loads and stresses in railway bridges meets writer's most hearty approval. The actual engine loads must, of cou be the ultimate standard of reference; but this is no reason for decli to use in all ordinary work a simple system of equivalent uniform lo we might as well insist that all measurements must be made with official standard yard.

When pressed for time in which to work out the proper equival the writer has specified the loads given by a particular engine; but w he had not this excuse he has specified a uniform load per foot wi single concentrated load to represent the excess weight of the loco tive, using one standard for the floor system which receives its full l with every locomotive; a second for the web system, in which the m mum stresses are developed by a partial load, and a third for the ch

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members, in which the maximum stresses are developed only by the full loading of the entire span. If Prof. Waddell, or other parties interested, will work out a complete system of uniform loads equivalent to various actual or typical engines, to be selected or approved by a committee appointed by the American Society of Civil Engineers, the writer, with many others, will take great pleasure in abandoning the use of engine concentrations altogether. The writer also agrees quite fully with Prof. Waddell's remarks upon suspended floors, and when, some years ago, a railway manager gave the writer for revision a bridge specification in which suspended floors were required in all cases, he amended it by forbidding their use in any case. This rule he has since had occasion to relax in but a single instance, and then for reasons purely local and special. The gain in lateral stiffness by the rigid attachment of the cross girders to the posts is so great as to justify this method, even at the cost of a good deal of attention to the connecting rivets, which, however, in the writer's experience, is not required. And if the attachment be made, as is now frequently done, by extending the web of the cross girder through a post made of Z bars, he sees nothing further to be desired.

As regards the spacing of the stringers, the writer very strongly prefers to place them exactly 6½ feet between centers. With this spacing the outer guard rail can be fastened by hook bolts to the outer flange of the stringers at such a distance from the track rail (12¾ inches from the gauge side) as to insure that a derailed wheel will drop on the ties without climbing the guard rail, and yet keep it at the greatest possible distance from the truss. By this arrangement the wider spacing of the trusses recommended by Prof. Waddell is not required.

Referring to Prof. Waddell's remarks upon the use of steel in bridge work, the writer fully agrees with him that, except for screw bolts and minor members, it should be insisted upon for all first-class work. When the utmost economy of time and money is demanded, the writer is sometimes forced to allow the use of iron—excepting for eye-bars, pins, rivets and connecting angles—but he does it with great reluctance. But in using steel he differs from Prof. Waddell in believing that the reaming, or still better, the drilling of rivet holes after the parts are assembled, is something that ought always to be required. This should be done not only as a precaution against injury to the metal and consequent fracture, but more as a means of securing smooth and closely matched holes which the rivets can completely fill. In holes for field rivets, where tight work is so hard to get, the reaming is a matter of much importance, for iron no less than for steel.

Mansfield Merriman, M. Am. Soc. C. E.—I agree with Mr. Waddell that the specification of typical locomotives with wheels whose distances apart are expressed in inches and fractions, or even in whole inches, is unprofitable. It is like computing the area of a circle with

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the value of π expressed to ten or more decimals when two or three would answer every purpose; indeed, it is worse than this, for in figuring on the circle a true result is obtained, whereas the typical locomotive gives stresses that can never exist. When, however, such locomotives are specified, it is the duty of the bridge designer to use them in his computations, and it is my opinion that usually it will be better to do this directly according to methods which are well known to be logical and accurate, rather than obtain equivalent uniform loads and then find the stresses due to these. Of course, errors of two per cent., or thereabouts, on the side of danger are not important, and I would not oppose the method proposed on this account, but there appears to be little gained in simplicity by its use. Computers should adopt the method which suits them best, regarding not merely saving in time, but also simplicity in the logical understanding of the processes. I should judge that most students would find the theory of concentrated loads easier to understand than that of computing and using several uniform "equivalent" loads to attain the same results. Those who write specifications and invent typical locomotives should be strongly urged to express the distances between the wheels in feet, or at the most in feet and half feet. This was done in Wilson's specifications for the bridges on the Pennsylvania Railroad published and discussed in *Transactions* for 1886, and if such were the case with all typical locomotives, most of the difficulty in computing stresses would disappear.

In the *Annales des Ponts et Chausses* for September, 1891, is promulgated a new order concerning the computation, design and testing of railroad bridges in France. It specifies a typical train load, consisting of two locomotives and two tenders, followed by loaded cars. Each locomotive weighs 56 tonnes on a wheel base of 3.60 meters, there being 4 axles equally loaded and equally spaced. Each tender weighs 24 tonnes on a wheel base of 2.50 meters, there being two axles equally loaded. Each car weighs 16 tonnes on a wheel base of 3.00 meters, on two axles. This is a very simple arrangement, and it is particularly praiseworthy in having no uniform load following the locomotives, all the cars being on wheels, as they certainly ought to be. "Logic is logic," and those who persist in specifying typical locomotives ought not to rest content unless the entire rolling load is concentrated on wheels. However, if a uniform load and a single concentrated load be specified instead of typical locomotive wheels, simplicity in every direction would be attained. The uniform load would range from 5 000 pounds per foot, per track, for short spans, down to 3 000 for long spans. The single concentrated load might be the same for all spans, say 100 000 pounds, and it should be given any position, either at the head of the uniform load, or elsewhere. I trust that the paper before us will lead to the adoption of such simple methods of loading.

A. J. Du Bois, Jun. Am. Soc. C. E.—Mr. Waddell gives us a rule

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for finding floor beam concentrations directly from the equivalent uniform live load. For this rule he claims that "the principle involved was unknown to the engineering profession only two years since," and he declares that he is "amazed that the principle was not discovered long ago." The rule is thus put forward and emphasized by him as the statement of a new principle and a noteworthy addition to *engineering knowledge*. Mr. Waddell is an expert bridge engineer, and his record shows that he understands his business. To make a slip occasionally in a paper written under pressure is no disgrace to such a one; and if, therefore, I venture to point out a slip in this case, it is simply in the interests of accuracy. No one, I am sure, will be more ready to candidly acknowledge such a slip, than the author of the paper under discussion, and few can better afford to.

Let me say then, that after careful examination of Mr. Waddell's demonstration of the rule in question, I fail to find any addition to our knowledge or any new principle. I do not even find any principle stated at all, but only the statement of a rule. Now a rule is not a principle, but the result of the application of a principle. The principle which lies back of Mr. Waddell's rule is apparently not recognized by him at all. Yet it is well known to him and to all of us. If it had only occurred to him, he could never have written about it or its consequences as he has. The application of this principle gives us immediately without the least algebraic work Mr. Waddell's rule, and in a more general form than he puts it himself.

By the expression "equivalent uniform load for a point," is always meant that uniform load which causes at that point the same moment as a given system of concentrated loads, to which the uniform load is thus "equivalent." Premising this, the principle which is really involved in Mr. Waddell's rule, which he has failed to use, but which is, however, long known to him and to all of us, is simply as follows: A uniform load causes the same moment at any point of a horizontal beam supported at the ends, as half that load concentrated at the point. Mr. Waddell knows this as well as all the rest of us, and if it had only happened to occur to him, he would have saved not only all his algebraic work, but also all his remarks about his rule, the ignorance of the profession up to that time of writing, the newness of his principle, and no little unnecessary amazement. He would have simply stated his result as a directly obvious conclusion, and made no further comment. For of course it follows at once, that if any given uniform load is the equivalent uniform load for any point of a span, half of it concentrated at that point and acting down, gives the same moment at that point as the given system of concentrated loads to which it is equivalent. If, however, it acts up, and the given system also acts, the moment at the point will be zero. In other words, the original span is now converted into two adjacent, independant spans, with intermediate support at the

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point, and the reaction at the point is equal to half the equivalent uniform load for that point, for a span equal to the original span.

Here, then, is the rule more generally stated than Mr. Waddell himself gives it. He takes his point in the center or supposes equal panels. This is not necessary. The result and principle hold good for any point, and are the direct and immediate result of a well known principle.

When Mr. Waddell reflects that this is really the principle at the bottom of his unnecessary algebraic work, that he has actually gone through that work without recognizing it as an old friend, that it is really this principle which he claims the entire engineering profession were ignorant of two years ago—it is, I think, simple justice to him to say that his amazement will be no less than ours, and no less than it was before, although the grounds for it may be changed. It is indeed true, that so far as I know, his rule has never been explicitly stated before. But then, since equivalent loads can only be found when the reactions are already known, it naturally enough has not seemed worth while to any one to state how to reverse the process. If, however, the equivalent loads are given, it would indeed be a source of legitimate amazement to find any engineer at a loss to reverse the process, and I am sure it will now seem equally surprising to Mr. Waddell himself, that having thus reversed it, he should have for an instant thought that any new principle was necessarily involved in working a problem backward, which was not already involved in working it forward, or that he was making any noteworthy contribution to engineering knowledge in so doing. It must also now seem equally surprising to him, how he could write that he had "just ascertained" that "equivalent loads can be used in finding the reaction"—knowing as he did that they can be found from reactions—and not at once see that it should have taken no time worth speaking of to ascertain that the reverse was also possible.

As to the use of uniform loading, with or without engine excess, in place of the present laborious, heart-breaking method of computing stresses, I wish to agree most emphatically with Mr. Waddell, and I sincerely hope that this discussion may lead to such reform. Nothing is more certain than that the stresses as now found for a specified wheel load system are not the exact stresses even for the system specified, and still less for other systems for which the structure must also be adapted. Any specified system is thus only typical, and intended at best to give stresses greater than any actual loading, present or future, which the structure may have to carry. Impact has to be allowed for even then, and thus the apparent accuracy is a delusion. It is also certain that uniform loadings, with or without engine excess, can be easily chosen so as to give always a very close approximation to the stresses due to any assumed system as at present found, and in nearly every case somewhat in excess, as they ought to be. Scientifically, there is no reason for preferring one method to the other. Both are

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approximations, both are in error for the same reasons, and both give essentially the same stresses. Ease of calculation should therefore decide the matter. The present method can claim no advantage over the other, and is incomparably more laborious and time wasting. The writer has been obliged to teach and use it simply because it is in vogue. But he still retains and teaches the other also, believing firmly, as he has long since stated, that sooner or later engineering practice will return to it. May the day be hastened! The present tedious method is not so new as seems to be supposed. It may be that when those responsible for it find that they have been anticipated so far as discovery and introduction is concerned, they may be less interested in their fad.

Long before 1879, Prof. Asimont, of the Munich Polytechnic, taught, and in 1879 he published the identical method which was worked out independently here in 1880. Prof. Asimont's diagram is in a different and much less convenient shape from that usually employed here, but the method is precisely the same. So far as priority of discovery and publication is concerned the method is his and should bear his name. I received Prof. Asimont's method in July, 1879, and after examining it laid it aside as a defective refinement, offering only apparent accuracy at the expense of unnecessary labor. Such I still consider it, under whatever name it goes. If I had been told in July, 1879, that in a few years it would be specified here by our railroad companies and advocated by our most eminent builders and designers, I would have listened in utter incredulity. I would have replied that our designers have left that sort of investigation exclusively in the hands of "theorizers," the "mathematicians," the "x, y, z engineers." That in America that sort of thing wouldn't "go." That our "practical" men would regard such investigations as an outcome from that scholastic Nazareth out of which in their opinion cometh no good thing. That they were the last men in the world to look with favor upon scrupulous refinement in calculation, the accuracy of which was neutralized by fundamental ignorance of the actual conditions. That, in short, they would care little for a laborious method of calculation, the validity of which was impaired in the start by unknown factors not accounted for. And yet since then that very method has become the fad of the day. If to get up an elaborate mathematical treatment which goes farther than actual conditions warrant is to theorize, then the theorizer leads the crowd to-day. The method "goes," and, as in duty bound, I have had to teach and use it—but always under protest. I have never had a student whom I have not encouraged to do his best to show it up. I am rejoiced that at last the time seems getting ripe for a general protest all along the line. I hope it may be so general that it will have to be regarded, and so forcible that it will be effectual.

Gentlemen of the Asimont method, the present paper makes a square issue. It has been made before and has been ignored every time. Mr. Waddell is not the first by any means to show and prove that for any

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given wheel load system you please, uniform loads, or uniform loads in the engine excesses, can be specified, which will in all cases give stresses almost identical. That, considering the necessary inaccuracy of the present method, no man can say that these stresses are not every whit as accurate. They may even be more so. They are in general slightly greater, and they ought to be. I object to Mr. Waddell denoting errors on the wrong side of the present method by the word "danger." It is an unnecessary concession. It would be as just to make the other method the standard, and to denote errors on the wrong side of it by "danger."

Here, then, is the square issue which has been so persistently ignored. Are those deviations from the results of the present method, small as they are, on the "danger" side or not, and are they worth the drudgery of their evaluation? If so, your reasons, gentlemen of the Asimont method! If they are not, why in the name of common sense must we continue to submit to such drudgery?

J. P. Snow, M. Am. Soc. C. E.—I am glad that the subject of bridge designing is again before the Society for discussion. I consider Mr. J. M. Wilson's paper on "Specifications for Strength of Iron Bridges" and its discussion, published in Vol. XV of *Transactions*, page 389, the best treatise on details of bridge design that has been published in the English language. A full discussion of the present paper will be of great value as supplementing that and bringing it to date. The practice of using uniform loads for calculating strains seems to be making substantial progress. This is as it should be, and is a natural result of the fact that bridge buyers are getting more and more to make their own designs. Calculating by wheel concentrations was a great step in advance of the old method of using a certain uniform load per foot for all spans, and it still gives the Chief Engineer or Manager, who does not know how to design his bridge, and who does not employ an assistant who does know how, an easy and precise way to specify for the bridge that he wants.

It is really more complex to specify how a bridge shall be designed for uniform loads than it is to say, "All parts of the structure shall be calculated to resist the maximum strains produced by a string of class so and so engines." On the other hand, in designing the bridge, it is much simpler to use uniform loading, that is, the bridge buyer who does not know how to design his bridge had best specify the engine diagram. If he attempts to specify uniform loading he must state carefully how it is to be used for chords, shears, stringers, floor beams, etc., and the chances are that he will either make a botch of it, or leave bidders much to assume at their individual convenience. In the nature of things, typical engine diagrams and standard specifications, so-called, are not for engineers who know how to design a good bridge in their own right.

For the past seven or eight years, that is, since I have been a bridge

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buyer, I have used uniform loading exclusively, and see no reason now for doing otherwise. I do not object to the amount of labor *per se* involved in calculating by wheel concentrations, but it violates one of the fundamental precepts of good engineering to put so much more precise and laborious work into one part of the design than into another; that is, the labor of making a strain sheet ought not to overshadow the using of it. At present I am using a live load curve giving loads as below:

Span.	Load per foot.	Span.	Load per foot.	Span.	Load per foot.	Span.	Load per foot.
4	30 000	12	10 430	30	7 280	80	4 750
6	20 000	14	9 700	40	6 380	100	4 460
8	15 000	16	9 180	50	5 710	120	4 360
10	12 000	20	8 470	60	5 270	150	4 340

It is based on a string of consolidation engines with 35 000 pounds on each driving axle and a tender with 20 000 pounds per axle; to this is added $\frac{25\,000}{\text{span}}$ to cover the imperfections of track, wheels, etc., generally denominated impact. The short span end of the curve is touched up with a pair of axles 8 feet centers, with 45 000 pounds each plus $\frac{30\,000}{\text{span}}$. I take issue with the author in thinking this latter engine should be used, as more than half the bridges with which I have to deal have spans less than 20 feet. To state how the above loads are used for moments, sheers, floor beams, stringers, reactions, etc., would be too long for this place; but if the author of the paper will look up a communication from me published in *Railroad Gazette*, December 10th, 1886, he will see that his claim that no one knew of the principle which demonstrates that the floor beam load is equal to the panel length multiplied by the load per foot, corresponding to a span of twice the panel length, is a bad one. I there say, "For floor beams use $l \times$ load per foot corresponding to span $= 2l$, where l is the panel length." The editor of the *Gazette* volunteered a parenthesis which marred the sense, but the above are the words as written. I have used this "principle" constantly, since 1884.

A bridge that is unsatisfactory to the trainmen who use it and the bridge crew who take care of it, is not a good bridge, although it may satisfy the engineer who computes it. The converse of this statement, however, does not always hold; that is, a bridge may be satisfactory to the men who use it and take care of it, and still may have some glaring fault of design that causes some part to have but a fraction of the strength of the other parts. Still, the bridge engineer, if he wishes to escape being called a crank, must be very cautious in condemning a bridge on the score of unscientific design, that is doing its work in a satisfactory manner; especially if he be not the possessor of a gray head. Riveted

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pony trusses of 90 feet span can be made perfectly satisfactory, and careful study of many of them under the action of trains, convinces me that properly designed, they make good bridges. I prefer them to through plate girders of that length, from the fact that such heavy girders are very likely to get badly abused in getting them into place. For deck spans the whole can be riveted at the shop, and the handling is then a simple matter and can be done without injury to the iron work. The very best form, in my opinion, into which metal can be put to make a bridge, is a deck plate girder with cross floor beams and stringers; the stringers to be under the rails and their tops to be level with the tops of the girders, the girders to be spaced as wide as can be shipped on cars. I have had trouble in getting them 9 feet on centers. In this form of bridge the girders support the ends of the ties and the latter project outside of the girders sufficiently to allow convenient bolting of the guard timbers. I have designed very many multiple system riveted trusses and the only substantial objection to them is that symmetric chords with channel-shaped sides cannot be used. I have dropped them for this reason. For a web system simply the double cancellation is far peferable to the pure Warren type, and more economical. Do we not use it continually as the best lateral bracing that we can devise?

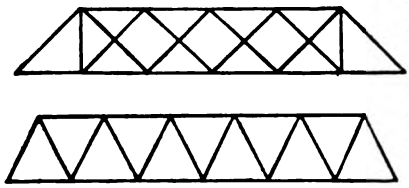

FIG. 1.

In the six-panel truss shown above, the double system has the same number of web members as the Warren, and the shear in each of the former is but approximately half what it is in the latter. The inclination and length is greater, to be sure, but it will work up more economically if five-sided chords can be tolerated. The ambiguity as to which system the one-sixth of an end panel load travels over to get to the farther abutment is of but little moment. If the designer is very thin-skinned he can allow for it on both systems. A modified Warren, as shown below, has many advantages, and I am now using it exclusively on short spans.

I would use pin connections for shorter spans than the author names. A double track span of 100 feet makes a good solid pin bridge. I agree with the author in his condemnation of suspended floor beams and rigid bracing. The specification under which I am now working says: "Floor beam hangers free on the pin will not be allowed;" and "The laterals in the loaded chord shall in all cases be rigid bracing." Regarding bridge floors, the practice on the system with which I am connected, is to use flat ties 6 x 8 inches on wooden bridges, and 7 x 8 inches flat notched one-half to three-quarter inch over the stringers on iron bridges. These ties are 12 feet long and laid 12 inches on centers; guard timbers

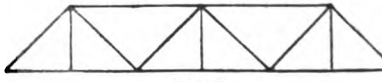

FIG. 2.

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6 x 8, notched 1 inch over the ends of the ties and bolted every fourth tie. Our main stringers are 5 feet on centers, with outside stringers in through bridges 10 feet wide. I prefer Southern long leaf pine for bridge ties. It outlasts the hard woods and shows plainly when it is decayed. I object strongly to the wide spacing of stringers advocated by the author. If we are building an iron or steel bridge, let us make it complete in itself, and leave the ties to do only their legitimate duty of holding the rails to gauge and line. The idea that some engineers seem to have that the wheels should be cushioned by the spring of the ties between the stringers, belongs on the shelf alongside of the old idea that iron bridges must be set on wooden wall blocks, that they would be shattered like glass beads if they rested directly on the unyielding masonry. I know of no instance where the flange angles of stringers have been bent down from the deflection of the ties, but can remember two bridges where ties have been found broken on account of the wide spacing of the stringers, and this by trains on the rails. No doubt the ties mentioned were cross-grained and not large enough, but the design is poor that looks to the rapidly decaying ties to help out a floor beam of scant dimensions. In New England a great majority of the bridges are limited in the matter of depth of floor, and the necessity of using a deep tie would often interfere with making a good design. The thickness of tie named above is sufficient to take the spikes, and experience shows that they last as long as larger sticks. Personally, I should place the ties farther apart, say 14 or 16 inches. I have known many instances of derailed cars crossing bridges with the latter spacing and there was no bunching of ties. I abandoned the practice some years ago of bolting the tie floor to the iron work. If the ties are notched over the stringers and the guard timbers over the ties, there need be no fear that the floor and bridge will part company, unless it be in a cyclone region, with which I confess I have had no experience. On our wooden bridges the ties are not notched, but are planed one side to even thickness and spike bolted to the stringers.

"Impact" and "fatigue" measure the bulk of our modern ignorance in iron bridge designing; they are the present-day representative of the old bugbear that used to be downed by the "factor of safety." It seems reasonable that "impact" should be covered in the loading, and the addition to the engine loads previously mentioned is my attempt to do this. As a matter of fact the deflection of a bridge, even though it be of very short span, is but slightly more, under fast speeds than it is if the engine stands on it at rest; this depends, however, on the condition of the track surface and also on the wheels. I have known freight cars at speed to deflect a bridge more than the engine, evidently on account of wheels which were worn out of a true circle. "Fatigue," if we believe in it, should be covered in the unit strains. That the deductions from the German experiments on the effect of oft-repeated strains

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on metals are not applicable to bridge designing as stated by the author I thoroughly believe. Still, supposing our strain sheet to show the true relation between the strains in all parts of a structure, it does not seem satisfactory to use a constant unit for both those parts which are loaded to their calculated maximum at every passage of a train and other parts which get their maximum only under the unusual circumstance of two typical trains meeting. Nor does it seem just right to use the same tension unit for parts which are strained from zero to the calculated maximum many times a day, and other parts in which the varying load is no more than perhaps 50 per cent. of the total. That oft-repeated strains within the elastic limit hurt a member in compression, I do not believe. My practice is to use 10 per cent. greater unit strains on the middle truss of a three-truss bridge than on the side trusses, and for general tension a curve the ordinates of which give tension units, the abcissas being values of $\frac{\text{min.}}{\text{max.}}$. This curve might as well have been sketched by eye through certain points that were assumed as satisfactory, as to have been constructed from an equation. It seems to me that a curve should be more satisfactory to use than a list as suggested by the author, which jumps 1 000 pounds per inch between long and short members.

I see no sound reason for assessing plate girders below 20 feet span, as recommended by the author. His desire to discourage the use of short panels by so doing is hardly good engineering; the panel length unless governed by head room considerations, should be a function of the truss depth and distance between trusses. In short spans we are right in using short panels; and as stated before, more than half the bridges under my care are less than 20 feet span. We cannot discourage these legitimately by such units. The practice with us is to use one-sixth of the web as flange section in plate girders when the web is in one sheet without splice; but if the web is in several sections and the splice plates are proportioned only for shear, it seems hardly justifiable to count much on it to resist deflection. I do not understand the reason for the author's statement: "Of course if the web be counted in, the intensities of working stresses of the flanges will have to be decreased accordingly." In my judgment the plate girder is the best form in which we can build up metal to carry a load across an opening, and I allow the same tension units in them that I do in eye-bar members of a truss. I find that nothing less than three-eighth webs in stringers will give proper bearing for rivets. The wheel load cannot be reckoned as producing less than about 2 500 pounds vertical pressure on a single rivet, and this, combined with the horizontal pressure from the flange strain, will call for a three-eighth-inch web, at least, in the majority of cases. The fact is, good machine driven rivets are worth more than we generally allow, but if we have a set of bearing units we should take ac-

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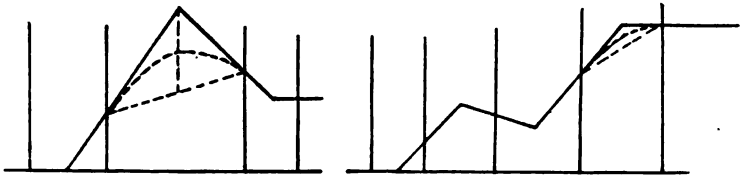
count of all the loads coming on our connections and stick to them. End floor beams are a good detail if the stringers are extended beyond the beam enough to take one tie; otherwise the distance between the last bridge tie and the first ground tie will be too great.

In calculating pin moments I think the load delivered to the pin by each member should be considered as distributed over its thickness. It surely is so distributed in fact. If the maximum moment occurs at the middle of the pin the result will be the same whether the load is so considered, or concentrated at the center of the member, but if the maximum occurs under some particular bar, the above consideration reduces the moment sometimes quite materially.



In the moment curves sketched above, the full vertical lines represent the sides of members packed on a pin, the full broken line the moments calculated with all load concentrated at centers of members, and the dotted curves the allowable change if the loads are considered distributed. It will be seen that the reduction is one-half the distance from the vertex to the line joining the intersections of the sides of the bar with the moment line. In some cases this perfectly legitimate consideration will materially help out on the size of the pin.

It is not likely that the discussion of Mr. Waddell's paper will exhaust the subject so that there can be no more said. Each one can state his opinions and beliefs, but it is not good manners to insist that others shall believe our creeds. My rules and methods are for my office; I will not attempt to force them on brother engineers. If they like them they are at liberty to use them. If they do not believe as I do, I dare not say they are wrong.

W. R. Hutton, M. Am. Soc. C. E.—On the resistance to bending of the web of a plate girder.

First.—First as to our author: He proposes to add one-sixth the area of the web to each flange, and to decrease "accordingly" (*i. e.*, proportionately) the intensity of working stress. As the result in this case will be the same as if the web were neglected, it is difficult to see a reason for it.

Second.—It is well known that in a rectangular beam the maximum shearing stress is about the neutral axis, diminishing to zero at the top and bottom fibers. In an I beam it is more nearly uniform throughout the web; still almost nothing in the flanges. Beyond question, then, the web must take the entire shearing stress.

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Third.—As to the bending stresses in the web, they exist and cannot be got rid of. Their intensity may be reduced by increasing the area of the flange, and this, in fact, is the practical effect of neglecting the web in computing the resistance of the beam. It would seem to be more rational as well as more exact to consider the entire section and proportion it for such intensity of bending stress as may be best for the particular case. The former method is an additional factor of safety, of variable, and, perhaps, uncertain value in ordinary practice, but there are cases, as mentioned by our author, in which it cannot be applied.

Fourth.—The progress of exact methods is well exemplified in our author, who, a few years ago, was a partisan of the flange-only-for-bending theory in its most extreme applications. We welcome his change of mind.

Fifth.—The proportioning of flanges to take the whole bending stress, while the web takes the shear, is not uncommon with French contractors. The following line of reasoning in support of the practice is from one of their authors. The moment of inertia of the beam enters into the formula for maximum shear. Now, if we use the moment of inertia of the flanges alone, neglecting the web, it results that the shear is evenly distributed over the section of the web. In other words, if we assume the bending stresses to be borne entirely by the flanges, it follows that the shear is uniform throughout the web.

W. L. Cowles, M. Am. Soc. C. E.—There are a few portions of Mr. Waddell's paper to which I would like to add emphasis while heartily approving nearly all the suggestions made. With regard to the using of uniform live loads, there can hardly be any question that any provision whereby the labor of computation can be decreased, while at the same time a satisfactory approximation to accuracy is reached, will meet with the approval of all engineers; as the only result will be to lighten labor, and thereby decrease cost. This is a matter of more importance to the computing engineer of a bridge company than to the engineer of the railroad for whom a design may be prepared, as the direct cost to the railroad company is not affected materially by the difficulty of making the computations after a specification which may be new to the designer, and the engineer of the railroad having always to use the same specification, can prepare tables which will make the work of checking and designing simple for himself; but there would be an indirect gain to the railroad company through the decreasing of the cost of designing, which must in the end be paid for by those who buy the bridges. It is impracticable for the computer of a bridge company to make tables which will fully cover all the different specifications now in use by different railroad companies, although some of the most common can be provided for in this way. It certainly can occasion no additional labor on the part of the railroad engineer, and will surely lighten the labor of the

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computer, if a system of uniform live loads such as suggested can be universally or even widely adopted. When it is considered that the engine and train loads on any given road differ very widely, and that the specifications for bridges are, or should be, so written as to provide for the very heaviest load, with something of a margin for possible increase in the future; it seems that there should be no objection to adopting a uniform load which will give strains within a reasonable per cent. of those caused by the heavy typical load, and the result of Mr. Waddell's investigations seems to indicate very plainly that such loads can be determined and used without the large number of different assumptions which have been considered necessary.

In the tables presented on pages 86, 87 and 88, it will be observed that the error on the side of danger is rarely much over 2 per cent., and it may be well to call attention to the fact that as this is for the live load simply, the actual error on the side of danger in the resulting section will only be from 60 to 70 per cent. of this amount, or 1.2 to 1.4 per cent. altogether, since the dead load can be calculated accurately. In case it were deemed essential that there should not be even this per cent. of danger, it would be easy to estimate the percentage to be added to different members of spans of varying numbers of panels which would make the strains right, as these percentages would be practically constant for spans of the same number of panels, and they could be easily applied. This, however, would be in the direction of additional work which it is sought to avoid, and an easier method would be to make such an addition to the assumed uniform load as would cause all results to be on the side of safety as referred to that load, which would be equivalent to a satisfactory concentrated loading. It may be objected that this would add materially to the cost of the bridge, but upon investigation it will be found that the difference is not great. In tension members, where the area cannot be provided for exactly by ordinary regular sizes of bars, the difference arising from using an extra 2 per cent. is rarely such as to cause a difference in the size, and will sometimes be provided for by the excess of section used, and generally will not increase the section enough to add to the thickness of the bar, although in a few cases it may have this effect. In compression members where it is customary to make the sections exact by varying the weight of channels or angles, there will be some increase of section, but the amount is indicated by the result of a calculation on the 300-foot span mentioned on pages 87 and 88, where the total additional cost was less than $40, an amount which a railroad engineer might be willing to pay in one or two cases for the advantage of a uniform system of loads.

I cannot pass the section concerning forms of trusses without expressing my thorough approval of Mr. Waddell's objection to pony trusses. The uncertainty with regard to the strength of the top chord and the

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lack of rigidity in the structure should cause every engineer to discountenance its use. I cannot quite agree with the writer in his opinion that it is better to carry the intermediate panel load of a Pettit truss by a strut toward the pier instead of a tie toward the center, although I am willing to admit that the former method will give somewhat more of inertia and consequent rigidity to the truss, for the reason that in the center panels the dead load strain, carried to the top of a post by the tie referred to, is generally necessary to prevent reversal of strain on the post at a point where the line of the top chord is broken. This I consider is essential to avoid if possible, and I think this is more important than the slight additional rigidity afforded by the strut.

I am glad to notice that the question of unit strains on end posts is mentioned, as it has always seemed to me that it should be regarded as a continuation of the top chord system rather than as an intermediate post. The reason for Mr. Cooper's specifications for this member, I have understood, was to provide additional section and weight as a means of resisting any possible shock or blow from a passing train which might be derailed at that point; but this can be much more effectually provided for, if, indeed, a bridge should be designed to resist such a shock, by adding a collision strut running from the center of the end post to the second bottom chord panel point; and this construction, in addition to providing for this contingency, assists the end post by decreasing its effective length, and thereby stiffening it and decreasing its section. Another consideration against using so small a unit strain in designing the end post is that, as fully shown by Mr. Waddell, the end post should have ample provision for carrying the bending strains induced by wind pressure, and if designed with this in view, will certainly be heavy enough to satisfy any feeling that it should have excessive strength to provide for such a shock or blow as suggested. I believe that the end post should always be calculated to resist the bending moment referred to, although I cannot say that it has been my practice to do so, except where such provision is called for definitely in specifications.

I do not consider that a bridge is in danger if the end post is not thus designed, except for especially long spans, and it will hardly be expected that such a large amount of extra material as is required in such a design will be provided by a bidder in competition where it is not called for, unless in a bridge where its omission amounts to a danger. I think bridge builders would prefer to build work designed in this way than to build a lighter section, and this is therefore a matter for the railroad engineer who prepares the specifications, and I would be pleased to see such a specification become universal. The general specification that all wind strains shall be provided for is not sufficient to cover this point under the present practice in designing, and it should be made a special point. One difficulty which competitors have to meet is the un-

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certainty as to whether the engineer who compares their plans and checks them over is sufficient of an expert in bridge work to notice defects of this kind in any designs when such points are not particularly noted in the specifications. It is well understood that while many railroad engineers may have the highest ability in many directions, they are not necessarily experts in this line, although many are, and it is therefore extremely desirable that there should be confidence on the part of bidders that all plans would be examined by some engineer thoroughly competent to examine into all details of the designs and form an accurate opinion as to their relative merits.

The question of connection of stringers to floor beams is an important one, and the objection to riveted connections is a valid one, both on account of the horizontal bending induced in the floor beams, and also the tendency to cause the stringers to act as continuous girders, producing tension on the rivets at the top of the connection. The point in some specifications requiring that the connecting rivets shall all be included in the lower two-thirds of the beam avoids this danger largely, and is a good rule, although sometimes difficult to follow unless the stringers have considerable depth. The same objection applies to stringers resting on the top of the beam, where they should be braced in some effective manner, and where, if both stringers are attached to the same brace, there is a similar strain on the rivets at the top of the beam. In such a case each stringer should be separately braced, so that there may be no pulling apart at the tops.

Paul L. Woelfel, Assoc. M. Am. Soc. C. E.—Prof. Waddell objects to the use of concentrated loads for computing the live load strains in bridges, and prefers to use uniform loads per lineal foot, changing with the length of spans. While I can see no practical objection to this, I must object to his reason, viz.: that the use of concentrated loads involves a larger amount of labor and complications than uniform loads. By applying the modern graphical methods (moment curve, shear curve and secondary shear curve), such as have been in use in the Engineers' Office of the Pencoyd Bridge and Construction Company for many years, there is practically hardly any difference whether we figure with concentrated or uniform loads, and we can use them in all cases, as it makes no difference how many wheel loads may leave the bridge. With a little practice one can easily get all the live load strains for a six or eight panel bridge with straight chord in about one-half hour; for a curved chord, single intersection bridge in, say, one and one-half to two hours. I admit that there are a few cases, like draw-bridges and arched ribs, where we save time by using uniform loads, and where we are the more justified in doing so, as the assumptions upon which our calculations are based are more or less arbitrary. The proof given, that the equivalent uniform load for floor beams is the same as that for the center moment of a span equal to twice the panel length, is mathematically

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very interesting. This fact, though, has been known and taught in our engineering schools for many years; anybody familiar with the use of curves of influence must know it. As the floor beam reaction and the center moment of the above spans have the very identical curve of influence, the equivalent uniform load in both cases cannot help but be the same.

I cannot agree with what is said in the paper in reference to plate girder webs. I admit that it will be hard to prove that the failure has been caused by using too thin webs. The working stresses in our structures are in general so far below the elastic limit that those cases where a bridge fails by the weakness of the body of the member are very rare, and eventual failures can usually be traced to faulty construction of details and weakness of the connections. The shear in the web of plate girders, however, is just as much there as the strain in the web system of a pin or lattice bridge. Very often, though, it is misunderstood, as we see clearly in some specifications, like those of the Pennsylvania Lines west of Pittsburgh, which allow 4 000 pounds shear when the fibers are vertical and 5 000 pounds when they are horizontal. This ought to be just the reverse. If we say that the shear is transferred by the web only, we give an empirical rule, nothing else. This shear, as vertical shear in the web only, does not exist; it is taken up by the flanges just as well; but a shear equal to this exists in the middle of the web as horizontal shear, as we can easily prove theoretically. If we put two beams loose on top of each other, and let them deflect under a load, they will slide on each other. To make them work as one beam we have to apply horizontal tensile and compressive strains, which are nothing else but the horizontal shear in our girders. In plate girders not too deep I very often prefer even to use a heavier web and leave the intermediate stiffeners out altogether. In many cases this will not only simplify the shop work without weakening the structure, but also give a lighter and stiffer structure. The heavy web will help diminish the deflection; the stiffeners will not.

I heartily approve of what Prof. Waddell says in reference to end stiffeners, as I know from my own experience that this is a point which is very often not called for in the specifications; and thanks to the customary lump sum bidding, is therefore, often not considered by competing bridge companies. I also thoroughly agree with what is said on wind pressure, as Mr. Cooper's specification gives for long spans decidedly too low figures. The author states that the fatigue formula for bridge members is unnecessary, because the metal in bridge members has time to recover itself between the applications of live load. It may be worth while to draw attention to the fact that Prof. Bauschinger's experiments have clearly proved that there is no such thing as fatigue of metal within the elastic limit; that you can load and unload a piece of metal below the elastic limit as often as you want without being able to break it. As

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all our working strains are below this limit, we can see that we need not consider fatigue of metal as such at all. Nevertheless, the fatigue formula, as Mr. Wilson has used it, for example, for the Pennsylvania Railroad Company's specification, is a very practical formula, and gives very good proportions, allowing higher strains for long spans and lower strains for shorter ones.

We determine our sections usually by the dead and live load strains only. The impact strain is just as important. It is usually only considered indirectly in most specifications. These impact strains will occur in the structure whenever a train goes over, and yet I am sorry to say that we know very little about them. The fatigue formula partly provides for it, as does Mr. Cooper in his specification, by allowing twice the permissible strain per square inch for dead load he allows for live load, or, in other words, by adding 100 per cent. of impact to the live load throughout. It is generally known that a short span or a member whose maximum strain is produced when only a short distance is loaded, must get more impact than the long span or corresponding member. The load comes on more suddenly, and will in its effect be nearer a fully dynamically applied load. The vibrations, horizontal and vertical, while the load goes over, are more likely to add together for a few wheel loads than they will for a very long train, where probably quite a number will compensate each other. Mr. C. C. Schneider, in his specifications written for the Pencoyd Bridge and Construction Company, provides for that by allowing different percentages of impact for different lengths of loading. This seems to me a step in the only right direction. We can only determine the exact proportion of the impact for different lengths of loading by a series of experiments, which could be made without too much difficulty.

Prof. Dr. Fraenkel, of the Royal Polytechnicum in Dresden, has constructed a very ingenious instrument with which we can measure fiber strains down to 85 pounds per square inch, and which draws out a complete diagram of the strain in the fiber. Let us now figure the strain in a member for the most unfavorable position of a given load. Let us then apply this loading statically in the same position on the bridge, and measure the strain. Let us further run it over the bridge with different velocities and measure the strains again, and I think we would have no difficulty in getting at a fair average for impact and vibration for different spans and velocities, upon which, undoubtedly, the condition of the track and the rolling stock would have some influence. We could even go further than that. Let us measure the fiber strains in four points of a chord or post, and we can find out at the same time what bending stress our chord or post got from eccentricity and stiffness of the connections. Connecting our instrument with the lateral system, we can see what lateral stress the train produced in our structure. I have used these instruments with great success in many cases, and am

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convinced that we should see a great deal clearer in regard to many important and doubtful points, if some prominent bridge company or railroad company would take up a series of experiments in a systematic and scientific way. The question of impact settled, a great deal would be gained. If we knew from our dead load, live load and impact, the greatest direct strain a member could get, we should surely much sooner be able to settle on a uniform permissible strain with a fair allowance for secondary stresses and safety; not for main members only, but also for the connections; for if we allow different working stresses in different members, we should surely do the same in our connections.

We could make, then, such assumptions for the lateral forces, that we could use the same permissible strains also for our lateral system; and in this way surely simplify our calculations, and come nearer the ideal structure, in which all parts and connections are uniformly strong. Mr. Cooper says in his specifications that the rivet shear shall not exceed 7 500 pounds per square inch, or three-quarters of the allowed strain per square inch upon the member; the pressure on rivet and pin bearing not to exceed 12 000 pounds per square inch, or one and one-half times the allowed strain per square inch on the member. According to this, 7 500 and 12 000 pounds would be the upper limits, and the second conditions would only be used whenever they would give smaller values. I do not think that this is quite the intention of the author. A member strained with 10 000 pounds per square inch corresponding to the maximum shear of 7 500 pounds surely ought to have its connections designed with a smaller permissible shearing stress than a member strained with 15 000 pounds (laterals and members strained by dead load only). Why should ten rivets in single shear in one case be equivalent to 4½ square inches of metal, in the other case only to 3? Another disadvantage is that we make our calculations unnecessarily complicated by introducing two conditions. Very often we have to consider two cases, and must, besides this, figure out the average stress per square inch in the member especially for the purpose, as the section of the member is determined by the different strains for dead and live load.

For pin bending, Mr. Cooper allows 15 000 pounds all the way through. A 3 x 1-inch eye-bar as a member of the lateral system is worth 45 000 pounds. In a short truss where we would get about 10 000 pounds per square inch, we have to use a 5 x ⅞-inch bar for the same stress; the same pin would, under the same circumstances, otherwise do for both these bars. Mr. Wilson determines his shear and bearing stress in proportion to the compressive unit stress in the members, at the same time allowing a constant fiber stress for bending on pins. This somewhat complicates the calculations, and is under the same disadvantage in so far as pin bending is concerned as Mr. Cooper's method. As soon as we make our bearing and shear dependent on the varying unit stresses in the members, we give

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up the great advantage of tabulating our shear and bearing values of rivets and pins, and have a great deal of mechanical work to do over and over again. All this is avoided in Mr. C. C. Schneider's (Pencoyd) specifications, where we figure with a maximum stress containing different amounts of impact for different lengths of loading, and use constant unit stresses throughout for main sections and connections. This is a point in saving time, which, I think, is more important than the introduction of equivalent uniform loads as proposed by Prof. Waddell.

Prof. Waddell declares himself for figuring with high permissible strains, at the same time assuming the highest possible load a structure can get. I thoroughly agree with him on that. As the impact, however, is one of these strains, let us try to find out something more about this yet unknown quantity, introduce it in a scientific and yet practical way in our calculations, and by thus reducing our factor of ignorance, allow higher strains. In determining our permissible unit strains, we ought to consider the secondary bending strains our members may get from the stiffness and eventually from the eccentricity, of our connections. Even with central connections these secondary strains may be very considerable. We can reduce them to a minimum by proper designing and by selecting the proper systems. Prof. Dr. E. Winkler ("Theory of Bridges—II.," Vienna, 1881) investigates a double intersection lattice girder of about 90 feet with a maximum eccentricity of only three-sixteenths inch, for a load in every other panel point. Although this extreme case is not likely to happen in practice, we may draw some conclusions as to the relative value of different systems from his figures. He gives us the additional secondary bending stresses expressed in percentages of the direct stress, as follows:

1. Girder without verticals, diagonals connected at intersections: chords, maximum 113 per cent., average 65 per cent.; web members, maximum 90 per cent., average 36 per cent.; total average 51 per cent.
2. Girder without verticals, diagonals not connected: chords, maximum 88 per cent., average 68 per cent.; web members, maximum 25 per cent., average 18 per cent.; total average 45 per cent.
3. Girder with verticals, diagonals not connected: chords, maximum 19 per cent., average 12 per cent.; web members, maximum 30 per cent., average 13 per cent.; total average 13 per cent.

Even for all panel points loaded, these additional strains average about 12 per cent. in the first case. In a trestle bent I found the same figures for some members to be as large as 200 per cent. for central connections and nearly 300 per cent. with eccentricity. ("Civil Ingénieur," 1887.)

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These secondary strains often cause quite peculiar effects. I remember the case of a suspension bridge, where the cables were made of pins and eye-bars. Experimenting on them with Prof. Frankel's instrument, we found compression in some of these bars when a heavy street roller passed over. A second instrument applied to the other side of the bar showed the tension, which we had figured, proportionately increased. We had a clear case of bending in our cable, caused by the friction of the pins in the pin-holes. The whole cable from the abutment over the piers to the other abutment, worked not only as a tension member, but at the same time as a continuous girder; so that while the additional strains produced by the street roller in the upper and lower fibers of the eye-bars in the middle of the span were compression and tension, our instruments showed them to be tension and compression at the piers.

The question whether it is correct to apply different formulas for buckling for fixed and hinged ends, is a very difficult one to decide. All our buckling theories and experiments are more or less uncertain. All experiments refer to the ultimate strength. It would have been of much more value to us in those experiments had we known any fibers were strained beyond their elastic limit. Experiments of this kind could be made with the same instruments I spoke of in connection with the impact. But even supposing we had derived a really perfect formula from our experiments, is there not quite a difference between the member with fixed ends in our testing machine and the same member in a bridge, where the stiff connections may not only not prevent the buckling, but where the secondary bending stresses, which are caused by these stiff connections, may even start the same? I think it hardly worth while, therefore, to make a distinction between fixed and hinged ends. Mr. Cooper gives different formulas for chords and posts, as I understand, to provide for the strains the posts may get in case of derailments. Do we not waste, then, a lot of metal in all those cases where we have, for example, deck bridges with ties resting on the top chord and where the posts are at least just as much protected against these strains as the chords? It also seems to me that by applying this formula, we make in a long span bridge with heavy posts a much more ample provision for these accidental stresses than we do in a short bridge with a very light post. I should think it more advisable to figure with one formula for posts and chords, but make the posts in through bridges strong enough to withstand a bending moment in case of an accident. This moment would naturally be the same in long and short spans, though it might be advisable to fix on a higher moment for the end posts, as these are more liable to get a square shock, while intermediate posts would probably get it under a flat angle.

I do not see that the danger of the top chords sideways buckling in pony lattice girders is so very great, if the girders are properly designed.

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These girders ought to have wide chords with double webs, and correspondingly wide posts with a solid web, well connected to the chord. It requires very little strain to prevent a member from buckling, and we can easily make the posts strong enough to do this work; they naturally will bend over when the floor deflects and tend to distort the chords; but I very much doubt that these additional strains will be more than the additional strains in the posts of through bridges from the same cause. For single track spans from 80 to 110 feet I prefer a lattice pony truss to a through bridge. The latter always has a more or less flimsy appearance, and has hardly sufficient lateral stiffness in its main members unless we throw in a great deal of extra metal. In reference to combining stresses, it is undoubtedly correct to allow higher permissible strains for the combination of dead and live load with wind and momentum. I do not think it right, however, to allow higher strains for the combination of dead and live load with the centrifugal force corresponding to an average speed, as the horizontal centrifugal force is just as much of a regular load in our bridges as the vertical dead and live load.

PALMER C. RICKETTS, M. Am. Soc. C. E.—Mr. Waddell has brought out in a very interesting manner some of the disputed points in bridge design, and it seems to me advisable that every member of the Society who is particularly interested in this branch of the profession should at least express an opinion on the questions considered in his paper.

I have always thought that the method of wheel concentrations, as used in calculating the stresses in single system truss bridges, would sooner or later pass into disuse, not so much on account of the increased labor involved in the calculations, which I do not think is great, as because it would finally be realized that the accuracy attained by its use is more apparent than real. Who would venture to say that the stresses actually induced, by the varying loads of ordinary traffic in the members of any railroad bridge designed with typical engines, even closely approximating those in actual use, would be identical with those shown on the stress sheet of the original design; or that they would generally come within a considerable percentage of those given by the stress sheet?—and as a matter of fact no engineer is justified in specifying loads which do not considerably exceed those in general use on the road for which the bridge is designed. That is, he must assume a load which will make provision for unknown increments due to future traffic. The method looks and is what may be called elegant, but whether the metal is more properly placed by its use is entirely questionable.

I have considerable sympathy also for those who object to the use of an equivalent uniform load, namely, because there is no such thing. Mr. Waddell has made out a very good case for the average equivalent uniform load obtained from the bending moments due to typical engines and their following load, but it seems to me that the use of a uniform load made heavy enough to cover a certain probable future in-

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crease in car weights with uniform excesses taken large enough to cover a certain probable future increase in engine weights; is a more simple and all things considered more rational method of providing for the proper distribution of the metal in the trusses, than the use of an assumed typical engine or two followed by a uniform load, or than an assumed equivalent of this assumed load. In any case, as long as so little is known of the dynamic effect of the moving train on the various pieces of the structure, I believe that it is not scientific to split hairs on either the relative position or the magnitude of loads which are only assumed anyway, and which, in view of the future, never should be assumed to give the same stresses as those induced by existing traffic even when statically considered.

In specifying wind pressure for large spans, it is well to remember that observations so far made, show that very great wind pressures occur only over very restricted areas; and that such great pressures are extremely unlikely to be found distributed over areas as large as the vertical projection of the longest spans now being built.

I am glad to see the opinion expressed that lattice bridges longer than 100 feet should be used. It was, perhaps, natural that the reaction from tubular and long lattice bridges should carry the advocates of pin bridges in the opposite direction, to such an extent as to cause the writer of a well known set of specifications in a former edition, to require all through structures of more than 90 feet in span to be pin connected. In a later edition he has raised this limit to 100 feet, and I think it should be still further raised, though, perhaps, not to the extent indicated by Mr. Waddell. For a single track, I think that all bridges over, say, 140 feet in span should be pin connected; and for a double track, all over, say, 120 feet. Pin bridges may be preferable for somewhat shorter spans, and for longer ones, I think, they certainly are so, for well known reasons — because the ratio of dead to live load becomes more considerable and the members larger, while in lattice bridges of longer spans the difficulty in making proper connections becomes greater and the loss of material becomes considerable.

The spindly, four-paneled, pin connected through bridges 100 feet long, which have been turned out in the past even from the best shops, have not been admirable specimens of design; and though present practice has modified some of their defects, they are still not by any means the best that can be used. Of course there is ambiguity in the determination of the stresses in bridges of more than one system, and it is more rational as far as the determination of these stresses is concerned to use one system only; but since the best bridge is that one which carries a given load most safely and economically, it is most rational to use for any span that kind of a bridge which most nearly satisfies this definition; and as in all other engineering work, observation of existing structures may modify theoretical conclusions. Observation of well

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designed and well constructed lattice bridges 100 feet long has shown them to be stiffer and safer structures than pin bridges of the same length, and it has yet to be shown that the best designed bridges of this character cost more for maintenance, or need renewal sooner than pin bridges. Because lattice bridges as they used to be built, with webs connected by one leg, no angles on the lower sides of the top chord webs, etc., are not good bridges, it does not follow that those of the best present designers should not be used. The writer does not mean it to be inferred that he does not consider single systems advisable in most cases, but only that the use of multiple systems is excusable in certain cases.

As to the use of pony trusses, the increase in the length of plate girders rendered commercially possible by advancements in mill and shop practice, will probably soon do away with them to a great extent.

There seems to be no question of the correctness of the present tendency toward riveted instead of hung floor-beam connections, that is, when these connections are so made that both channels of the post act together or nearly together; and as giving greater rigidity, riveted, instead of adjustable lateral connections, seem generally preferable. So long as stringers are spaced so that the inside of the guard timbers are outside their webs, I can see no sufficient reason, as far as safety is concerned, for spreading them further apart; for the flaring of the guard timbers at the ends of the bridge should bring any train off the track inside these timbers, and the wheels will then be inside the stringers also. If the train is so far off the track that it cannot be brought inside the guards before it reaches the bridge, or if it mounts the guards while it is on the bridge, an accident is almost sure to result, whatever the flooring may be. The floor system adopted by Mr. Waddell is certainly a safe one, but it seems to me to be unnecessarily expensive. As a matter of fact, with ordinary 6 by 8-inch guard timbers, flared at the ends of the bridge, notched 1 inch over closely spaced ties and bolted to them and to the stringers at frequent intervals by hook bolts; there seem to be few cases of derailment in which the train could not be safely guided over the bridge, that is, of those derailments which could be taken care of by any floor. Most engineers know of cases in which derailed trains have been kept between such guards on such a floor for great distances.

I do not doubt that the time will soon come when steel will wholly replace wrought iron, not only in bridge work, but in almost all structures and machines in which such iron is now used. It seems to me, though, that there are few engineers who would care at present to use this material only in bridges, in exactly the same way that wrought iron is used—with holes punched and not reamed in tension as well as compression members—and unless it is so used it cannot be said that it is practically as cheap as iron, for spans of ordinary lengths at least. But

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even though wrought iron should not be ruled out by the specificatio there hardly seems to be much doubt about the advisability of usi steel properly throughout, even at an increased price.

I think Mr. Waddell's allowed unit stresses, good ones, for this materi and agree with him in believing that it is irrational to double the allow live load unit stresses for the dead load. The theoretic deduction to the effect of a suddenly applied load cannot, I believe, be prope applied in this way, as the application of a train to a bridge is not sudd in the sense above used, and the induced stress even in the floor syst cannot be said to be due to a suddenly applied load.

G. Bouscaren, M. Am. Soc. C. E.—*Live Load.*—In the light of p experience, the proper live load to be assumed in the computation railway bridges, is a matter well worth considering. The old pract was a uniform load of 1 ton per linear foot of single track; this, with short panels commonly used then, gave very weak primary members a floor connections, even for the light engines and cars of the time. T breaking of floor beam hangers was of common occurrence in those da

The substitution of a train diagram composed of one or two typi locomotives followed by a uniform load, was an improvement on the method. The writer introduced it, he believes, for the first time in specification for bridges on the Cincinnati Southern Railway in 18 The engines of the diagram were the heaviest which were expected to used on the road at that time.

With the past now before us, it may be said that it was a mistake to provide for the future by a heavier load; this necessity was not evident then as now; besides, it has ever been a popular principle wit certain class of wise managers to "take care of the present and let future take care of itself." The outcome of this policy has been the newal of a large number of iron bridges, which had their life—suppo to be eternal—shortened to that of wooden structures, by the evolution American locomotives and rolling stock; a great many more only c their prolonged existence to a depleted condition of the compan treasury.

It seems to be a matter of common sense that the live load assumed calculations should not be exceeded in its effect on the different me bers by any actual load ever likely to come upon the structure. In t respect the usual arrangement of two typical locomotives followed b uniform train load, is defective, inasmuch as it does not give the grea stress in all cases. For instance, the chord stresses will be materi increased in long spans if the locomotives are supposed to be prece as well as followed by the uniform load; again, in iron bridges, ca lever bridges and continuous girders, larger stresses will occur at cert points of the web and chords when the live load is supposed to be tributed in a discontinuous manner over the bridge. This conditio liable to occur, and does occur quite frequently, on bridges where tr

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follow each other closely, and when from their position in proximity to a yard, switching has to be done over them. The arrangement and distribution of the load should therefore be prescribed as that giving the greatest result in each case.

The distinction between engines and train in the load diagram is not as important now as it was, for the reason that the train weight per linear foot has increased more rapidly than the engine weight per linear foot, the excess of the latter over the former having been reduced from 40 or 50 per cent. to 20 or 25 per cent. The fact remains, however, that the present form of load diagram is the most convenient to represent the intensity of the load desired, and it is not likely to be abandoned by railroad engineers except for a train of typical engines, which is being done in some specifications.

As regards the use of equivalent uniform loads for the purpose of computation, it is simply a matter of choice with engineers. The particular method of calculation employed is immaterial, provided it leads to correct results, and no reasonable man will object to the use of equivalent uniform loads or to graphical construction, if the stresses are accurately determined thereby. It is always well, however, to check one method of calculation by another; an exact coincidence of result is unnecessary, a variation of 2 or 3 per cent. is quite admissible, but it is advisable to keep on the safe side by adopting the larger figure in every case. Uniform loads are generally used in the computation on arch ribs to save labor, but the advantage to be gained in the case of ordinary trusses is much less. The writer must take exception to Mr. Waddell's remarks to the effect that the ordinary engine diagram cannot be used conveniently for finding bending moments in plate girders; he has used this method for many years. The weight and moment diagrams, such as given by Mr. Ward Baldwin in a recent paper read before the Society, having been once constructed, can be used very expeditiously to determine, not only the maximum moment at any point of a stringer, but the stress as well, and the maximum reaction on floor beams for any length of stringers. These diagrams, which need not be so elaborate as shown by Mr. Baldwin, can be readily drawn in a very short time, and their usage reduces very much the labor complained of.

The avoidance of the great variety of engine loads now being used in specifications is a great desideratum; a great number of them differ only slightly, but require, nevertheless, the construction of independent weight and moment diagrams, which is an unnecessary burden and annoyance. Five or six typical engine loads would cover the range of requirements for all classes of roads. The writer's criticism of the series of types proposed by Mr. Waddell is—1st. That it starts with too heavy engines for the lightest load, and ends with too light engines for the heaviest load. 2d. That all his engines are given the same length, whereas, as a rule, actual lengths increase with the weight. 3d. That

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his driving wheel box is somewhat longer than obtains in actual practice. The addition of a heavy passenger engine to the ordinary load diagram is a good provision which should be retained in the specifications. In spite of the tendency to long panels, short ones will occur occasionally, when the greater weight on a single pair of drivers will be the ruling factor. The application of this wheel load as a check in special cases, is certainly no great burden on the computer.

Wind Pressure.—American practice with regard to wind pressure was pretty clearly shown by C. Shaler Smith's paper on that subject and the discussion thereof published in the *Transactions* of the Society for 1881.

The practice is about the same now as it was then, viz., 30 pounds per square foot on the loaded structure, the pressure on the train being considered as a moving load; 50 pounds per square foot on the empty structure, the greatest result to rule in every case. No new facts have come to light since then warranting a decrease in these pressures; on the contrary, bridges continue to fall down from time to time in the track of tornadoes. The increased weights of trains and engines have increased the pressure of wind necessary to overturn them. If the pressure of 30 pounds on the loaded structure has not been increased to meet this altered condition, it has been, I suppose, on the ground that no train would be likely to venture on a bridge in such hurricanes; still it might be caught there, which would be a reason for assuming a heavier pressure than 30 pounds for small spans at least.

After all that has been said on the subject there seems to be no good reason for reducing the pressures. The fixed amount per linear foot given in certain specifications is based on the pressure of 30 pounds, and is understood to apply only to small spans. As the actual surface exposed to the wind is not proportional to the length of spans, and will vary quite materially with the design for the same span, the writer has always preferred to compute the surface for each case; it requires but little time to do so.

There is no longer any doubt that a pressure of 50 pounds and over is frequently reached by wind storms in this country. Shaler Smith in his paper gave several instances of well observed facts to prove this, and no one would be disposed to question the results of his observations who has seen the effect of some tornadoes which have swept over different parts of the country within the last few years. The width of zone of greatest intensity in such storms is admitted to be small; but surely this is not a sufficient reason for omitting altogether the consideration of the effect of these great pressures on empty structures, especially when it is known that they are often accompanied by a lifting force of unknown power which tends to increase their effect on the stability of the structure. In the writer's opinion it is certainly risky to assume less than 50 pounds per square foot on an entire empty span, whatever may

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be its length. In France and England, where the intensity of the wind is not likely to be greater than it is here, the pressure assumed on empty structures is generally above 50 pounds. With regard to the effective areas for wind pressure, the writer's method of computing them concurs with Mr. Waddell's.

In the computation of wind stresses the principal points of difference seem to be: 1st. Whether the transmission of effects in the upper half of the truss should be considered as going through the top lateral bracing or through the vertical transverse bracing. 2d. Whether the bottom of end posts in through bridges should be considered as fixed or hinged laterally. The writer's practice is to consider the transmission to the abutments or piers as taking place exclusively through the top lateral system, this being the most direct course, and to consider the end posts as hinged at the bottom, there being always enough play on the pins to allow free elastic deflexions laterally. These assumptions are on the safe side.

As stated in the paper, the effects of the wind on the chords are often a disagreeable surprise to bidders who neglect to take it into account in the first place, on the assumption that with the larger working stress allowable for the wind, no addition to the sectional area will be required. For large spans the necessary increase of section may be very material. It is well also to test the effect of the lateral pressure of the wind on long and slim struts; the bending moment due to the wind may require an increase of sectional area or the remodeling of the section.

Styles and Proportion of Bridges.—The writer agrees with Mr. Waddell, that for small spans which can be shipped whole, riveted girders are preferable to pin connected trusses; they contain more material, but this is more than compensated by their staunchness, and the fact that they can better stand neglect, a point which, unfortunately, should always be taken in consideration.

Pony trusses should not be condemned without reserve; there are cases where their adoption is dictated by circumstances and a proper regard for economy, as, for instance, where the crossings of several small streams occur on a stretch of level grade, with a small elevation of grade above high water which does not afford the necessary room for deck girders. Raising the grade for its entire length might be too costly; on the other hand, short approaches to each bridge, introducing several humps on the level stretch, would be very objectionable, especially on a road where fast trains are to run. Pony trusses are the best solution in such cases; they can always be made entirely safe by a liberal treatment of the top chord.

The Pratt and Whipple systems of web bracing, with vertical intermediate and inclined end posts and an inclination of diagonals approximating 45 degrees, are probably the best forms of trussing for economy and simplicity of connections with the floor system. In his later prac-

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tice the writer has avoided the use of the Whipple form on account the theoretical uncertainty of even distribution of strains between t two sets of web bracing, although he believes this objection to be mo specious than real. Beyond 200 feet the Petit truss is advisable, reduce the length of stringers and maintain the proper inclination diagonals.

The writer agrees with Mr. Waddell in giving preferences to t strut over the ties in subdividing the panels; although it is done at t expense of more metal, the use of the strut avoids all ambiguity as the distribution of the load and adds to the rigidity of the structu The fact that one-half of the floor beams are suspended in the Pe truss is not an objection if they are properly connected to the chord.

Beyond 250 feet a polygonal shape of the top chord is advisable the ground of economy. To better accomplish this object the ang should be suitably selected to maintain a continued increase in t stresses of the web members from the center to the ends of the spa Polygonal top chords when properly designed are, in the opinion of t writer, more pleasing to the eye than straight chords in long spa The ordinary rule of 14 feet clearance between trusses will genera give a ratio between distance from center to center of trusses and leng of spans, greater than $\frac{1}{16}$; this ratio can safely be reduced to $\frac{1}{20}$ for lo spans; beyond this the working stress per square inch in the top cho should be reduced in proportion to said ratio by the formula for co pression members.

The proportion between the depth and the distance from center center of trusses cannot be formulated by a fixed rule; the econom stability and rigidity of the structure, are the principal elements of t question, which should all be duly considered. The limit of 3 to seems high enough; with a width equal to one-twentieth of the spa this would lead to a depth of truss equal to three-twentieths of the spa which is not generally exceeded for long spans.

The necessity of properly designed guard rails and rerailing devic is universally recognized, but the advantage of increasing the clearan from 14 to 16 feet on spans of moderate length is questionable. Su increase would add materially to the cost of the lateral and floor sy tems, and would not be likely to prevent the class of accidents agai which guard rails and rerailing devices are ineffective, such as c lisions, broken axles and broken tracks.

In deck spans it is very desirable, when practicable, to carry t masonry to the top chord, to secure greater stability and rigidity.

All adjustable parts in a bridge should be avoided if possible, for t reasons that they seldom stay in adjustment, that they are consequent a source of expense in maintenance and that they are often overstrain by incompetent men. The writer has seen the end bars of the bott chord in a 150-foot span buckled by the overstraining of the end late

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rods; he makes it a rule now to use riveted stiff diagonals in the bottom lateral system of through bridges and the top lateral system of deck bridges; he also avoids adjustable counter rods by making all the web members, where a reversion of stress is liable to occur, able to resist tension and compression.

The riveted connection of floor beams to posts is open to the objection of inducing tension in the upper rivets and a bending moment in the post. On the shore spans of the Ohio River Bridge and on the Kentucky River Bridge, on the Cincinnati Southern Railway, where this form of connection was adopted, the top rivets get loose and have to be renewed from time to time. This can be avoided by a top tension bolt of sufficient strength to resist the flange stress of the beam considered as fixed at the ends to be on the safe side; the bending moment induced from the beam should also be duly considered in proportioning the post. Unsupported tension rods or bars of great length are injurious to the rigidity of the structure and should be avoided. It is not always convenient to preserve a fixed proportion between the length of bars and their depth of section, and there seems to be no necessity for doing so if the fiber stress induced by the weight of long bars is taken into account in proportioning the bars; the best plan, however, is to support the bars properly.

The first requisite of a safe deck is to be impenetrable to a derailed train. The ties should be strong enough to resist individually the impact from the heaviest wheel load; they should be firmly fixed in position and spaced close enough to prevent bunching under the raking of the derailed wheels. The writer uses a 4-inch space between ties; every tie is locked laterally by the stringer web projecting 1 inch above the flange angles; this is better than dapping the ties on the stringers, it requires less labor and avoids the danger of splitting the ties at the shoulder; every tie is fastened to the outside timber guard by a five-eighths-inch lag screw and every third or fourth tie is bolted to the stringers. Lag screws are preferrable to bolts, which will lose their nuts and drop off.

On the Cincinnati Southern Railway bridges the inside face of the timber guards were all lined with 3 x 3-inch angles; these angles had to be taken off: they got loose in time by the effect of temperature, curving up and out of line at the ends, and becoming a source of danger instead of a safeguard. In addition to the outside timber guard, the writer uses an inside guard rail made of old or second-class rails laid with inside shoulder braces. Both guards are 7 inches from the head of the track rail, a sufficient space for wheels to run in without skewing. It is safer to use long ties with auxiliary side stringers, the main stringers being placed directly under the rails. Where this is not allowable on the ground of economy the main stringers are spaced from 6 to 8 feet, according to length of floor beams, and the ties proportioned

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accordingly with a minimum depth of 8 inches. When practicable the deck should be of hard timber. For the reasons given by Mr. Waddell, long leaf yellow pine is perhaps preferable to oak, but the objectionable features of oak are very much lessened if care is taken to lay every stick with the heart side down; in that position it will not split and check under the action of the sun, and will last much longer.

Intensity of Working Stresses.—There seems to exist without sufficient reason therefor a great diversity of opinion among American engineers with regard to the proper limits of working stresses.

The experiments made by Wöhler and Spangenberg, under the auspices of the Prussian Government, and the Launhardt's formulas which have been deduced therefrom by a generalization extending beyond the scope of the experiments, seem to be responsible in a large measure for this state of things. We have apparently been too hasty in adopting these formulas without sufficient investigation. The engineers of England, France, Belgium, Holland and Prussia, who follow very closely what is being done by their neighbors, have, as a rule, left these formulas severely alone. All that can be claimed as legitimate conclusions from Wöhler's experiments are the facts, heretofore suspected but not proven:

First.—That an intensity of stress intermediate between the ultimate strength of the metal (as measured by a single application of load) and its limit of elasticity, will always produce rupture, provided the number of applications is sufficiently great.

Second.—That within the limit of elasticity, the metal will stand, without breaking or being injured in any way, an indefinite number of applications, whether the stress be always in the same direction or be alternately in tension and compression.

If the writer is correct in this statement, he is forced to the conclusion that Launhardt's formulas deduced from experiments on crippling stresses cannot apply by any scientific process of reasoning to elastic stresses, the only ones with which we are concerned. The single teaching of the German experiments is, that the working stress should never exceed the elastic limit. Now comes the question: If the elastic limit is the theoretical maximum of working stresses, how near to this limit can we come in practice? The question can only be answered by practice itself, of which we have to guide us a length of thirty or forty years in the construction of metallic framework for bridges and buildings, and a great deal more in the construction of machinery.

Before going further, it is well to have a clear understanding as to the precise meaning of the term "working stress." Where there is no motion the effect of the external forces is entirely static; where motion exists, a dynamic effect is added which is due to vibration and impact.

Impact itself is due directly to shock or to the rapidity with which the force is applied. We have, unfortunately, no accurate method of equating dynamic into static effects as occurring in bridge members,

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but we know that the maximum effect of impact, leaving out shocks, is to double the static stress, and we know that the effect of vibration increases as the mass of the strained member decreases, and is also affected by the length of the member. With this information and the result of observation and experience to guide us, we can always approximate for each particular case the equivalence of dynamic into static effect and deal finally with the resultant static stress. This resultant is what we shall call the "working stress." With this understanding, it can safely be assumed that a working stress equal to one-half of the elastic limit will always give ample security. This assumption is justified by the writer's own experience and that of others, as far as he is informed. Who has ever heard of a sound piece of metal, working ordinarily within that limit, giving away?

In a paper read recently (September, 1891) before the French Society of Civil Engineers, M. Contomin, Engineer for the Northern Railway, gives some reliable instances of iron and steel working successfully for a number of years under these conditions:

First.—In the case of two iron bridges built in 1854 and 1855, and recently renewed on account of increase in the rolling load, tests on specimens taken from the principal members of the old structures showed that the iron had preserved all its original qualities after a service of thirty-five years under a working stress of a little more than one-half of the elastic limit.

Second.—Similar tests made on specimens cut out of the heads and flanges of old rails which had been twenty-three years in the track and finally taken out on account of wear, showed that the steel (60 000 pounds elastic limit) had preserved in every particular all the qualities required of it in the specifications for those rails; its working stress in service had been 30 000 pounds.

Third.—By a careful calculation, taking into account all the elements tending to increase internal stresses beyond what they would be under a static load, M. Contomin shows that the working stress in locomotive axles on the Northern Railway exceeds but slightly one-half of the elastic limit of the steel, which in this case is 39 000 pounds, the steel being of about the same grade as our medium structural steel. This metal in ordinary service is subjected to nineteen reversions of stress per second, passing from zero to equal tension and compression. These axles never break, being always finally condemned on account of wear.

If one-half of the elastic limit may be considered as a safe maximum for the working stress in practice, on the other hand the margin which it gives to the theoretical maximum is not too much to cover the probable errors made in converting the dynamic effects of the rolling load into static values;—the secondary stresses due to the deformation under load, the unequal distribution of stress through large sections, the possible hidden defects of material, and other accidental causes of increase

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of stress. Therefore, in the writer's opinion, one-half of the elastic limit is the proper working stress to adopt, and he hopes to see Launhardt's formulas and their derivations dropped by every one concerned or interested in bridge building, because they are not only burdensome in application, but lead to incorrect proportionings which, in extreme cases, amount to absurdities.

The assumption, made by some engineers, that the effect of the rolling load on all parts of a railway bridge is twice as great as that of an equal dead load, is manifestly erroneous, and the assumption that the effect of the live load in highway bridges is the same as that of an equal dead load, excepting the variations due to the transient nature of the load, is equally incorrect. It is evident that the effect of impact and vibration must vary with the rapidity of application of the load; that it must be greater on the floor members and on the primary web members, and decrease for every member as the stress in that member is composed of a greater number of accumulative parts from different panels. The writer's assumption in his specifications is that the dynamic effect of the load in railway bridges will exceed the static effect of the same load by 100 per cent. on the riveted connections of stringers and floor beams, that this excess will be reduced to 50 per cent. on the primary members, and will decrease to zero at the end of a 500-foot span. The percentage to be added for impact can then be readily found for each member by the formula for interpolation,

$$50\left(1 - \frac{d}{250}\right)$$

where d is the distance of the members under consideraiion from the center of the span. If the formula $12\,000 - 60\frac{l}{v}$ used by Mr. Waddell for hinged vertical posts represents fairly the results of actual tests, it would be more convenient to apply it than Rankine's formula. This is a matter of investigation. We now have a sufficient number of tests on record of full size parts of different shapes to verify the formula.

We should not lose sight of the fact, however, that no reliable formula for the working stress of compression members can be deduced from past experiments, for the reason that all these experiments have been made to determine the ultimate strength of the struts experimented upon; less care and uniformity of method having been used to determine their elastic limit, which should be the proper basis for the working stress. There is no greater desideratum at the present time for engineers interested in metallic construction than an exhaustive and systematic set of experiments on the elastic limit of large compression members, where the influence of the four principal factors, viz., quality of material, shape of cross-section, ratio between length and radius of gyration and nature of end attachments, would be correctly determined.

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With regard to Mr. Waddell's reference to the value of the working stress for a steel post, deduced from the writer's specifications, which he considers as excessive, it seems sufficient to say that this value is well within one-half of the limit of elasticity of the metal, and therefore entirely safe; considering that the ratio $\frac{l}{r}$ being supposed to be less than 50, the flexion formula for posts does not apply. It is also well to bear in mind in comparing values of working stresses of different specifications, that where they include the effect of impact reduced to static value they must necessarily be greater than where they do not.

Combined Stresses.—For wind stresses or a combination of wind and other stresses, the writer specifies a maximum working stress of 25 per cent. in excess of the working stresses without wind, which is equivalent to five-eighths instead of one-half of the elastic limit. This is considered allowable on account of the rare occurrence of winds of such intensity as are assumed in the calculation. Formerly this working stress was taken as three-fourths of the elastic limit; it has been reduced by the writer to five-eighths, in consideration of the rhythmic action of the wind, which causes a certain amount of impact and considerable vibration.

The writer concurs with Mr. Waddell's method of treatment of the end posts in through bridges, excepting as to the maximum value of the working stress under the combined effect of the wind and other loads, which he thinks should not exceed five-eighths of the elastic limit as explained above. It seems inconsistent to admit 20 000 and 24 000 pounds for the combined action of the wind, when the working stress without wind is limited to 9 000 pounds. I hope Mr. Waddell will give us his reason for doing so.

Referring to the subject of plate girders, the web, if made thick enough to give sufficient bearing for the flange rivets, will generally with proper stiffening be found to be of ample strength. The only point likely to be weak is at the ends, especially if the height there has been reduced for a shallow seat; it may be necessary to reinforce the web with thickening plates in addition to the stiffeners. Deep plate girders with thin webs properly stiffened, act more like trusses than solid beams; although the web plate participates to a small extent in resisting the bendings, it is safer to assume that the whole duty falls on the flanges. The web is generally spliced with regard to shearing only; if it is relied upon to resist any part of the bending moment, it should be spliced accordingly, and wider plates and a greater number of rivets will be required than are generally used. When flange plates have to be used, it is important to have as much metal as practicable in the flange angles, as they are certainly strained more than the plates, owing to the eccentricity of the web attachment and the unequal distribution of stresses.

General Details of Construction.—In the writer's opinion, the details and the general design are of equal importance, because all the ad-

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vantages of a good design may be lost through faulty details. Greater progress has been made perhaps in the design of details then in any other branch of the art. If we are yet too far from perfection to rest on fixed standards, even for ordinary spans, we should, for the purpose of criticising our own work, keep in mind the principal characteristics of good details, viz.: 1st. An arrangement such as will secure the independent action of the several parts, as intended in the general design, and prevent one class of members from sharing in the duties of another, or doing work which it was not designed to do. 2d. The avoidance, as far as practicable, of secondary stresses, such as bending moments in tension and compression members, which are liable to be overlooked, causing an excess of working stress in these members, or if properly considered, leading to an expensive increase of sections. 3d. A form and disposition of parts that will insure as nearly as possible a uniform distribution of stress through the sections, which requires a close coincidence of the resultant lines of stresses with the centers of gravity of sections. 4th. If the above conditions cannot be entirely fulfilled, the details should be such at least as to permit a correct analysis, that will leave no ambiguity as to the distribution of stresses. 5th. Tension in rivets should be entirely prohibited, and the position of field rivets should be such as to give ample room for good work.

The 1st and 5th conditions should prohibit the riveting of both ends of stringers to floor beams, excepting for very short spans. It is best to have a riveted connection at one end only of each stringer, with a slip joint at the other. This is in no way prejudicial to the rigidity of the structure, which is entirely secured by the horizontal lateral system. A continuous length of 150 feet of rigidly connected stringers is too great; the elastic elongation of the chord for that length due to the live load would be in the neighborhood of one-third inch, which is sufficient to create a very objectionable tension in the riveted connections and undue stresses in the stringers and floor beams.

The writer agrees with Mr. Waddell in condemning beam hangers, with screw ends, but does not see any objection to the suspended floor beam if properly connected otherwise with the chord to transmit the wind stress; it secures a better distribution of the panel load at the panel point than a riveted side connection of the beam with the post. In double track bridges the riveted connections to posts are open to the further objection of unduly straining the rivets and causing undue bending moments in beams and posts, owing to the unequal deflexion of the two trusses when one track only is loaded; this is probably the reason for prescribing suspended floor beams in double track bridges on the Atchison, Topeka and Santa Fe road.

The writer would not dispense with the reaming of rivet holes in mild steel, and believes in reaming iron as well when a full duty is expected from the rivets. Reaming is the only sure way of securing an

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exact coincidence of rivet holes, which is an essential condition for good work. Another reason for not dispensing with reaming in steel plates is the well known fact that hard spots are of frequent occurrence in the softest steel.

The length of stay-plates at the ends of compression members is a very important item; their function is to distribute the stress uniformly through the section; and they should be long enough and have a sufficient number of rivets to do so in the most unfavorable case that can and does happen, where the bearing is altogether on one side of the post. The influence of the end plate on the carrying capacity of built posts was clearly illustrated in several instances of column tests made by the writer.

The latticing between posts should also be made stronger and more substantial than is generally done; it should be proportioned to the weight of the post, and the lattice bars made stiff enough to preserve their shape in handling members before erection. Bent lattice bars are a noticeable feature in nearly every bridge through the country; this is proof enough that they are generally made too weak.

H. H. Filley, M. Am. Soc. C. E.—Although bridge designing has been one of the leading subjects of investigation among the engineers of this country for a generation past, the subject is by no means worn threadbare, and from this résumé of disputed points by Mr. Waddell, the conviction is strengthened that enough still remains to keep the subject well to the front for a long time yet. There is one fact which the author states concerning live loads in bridge calculations that should not be lost sight of in considering different methods of calculation, viz., "It must be acknowledged, therefore, that all assumed loads for computing stresses in bridges are merely typical." The engine diagram nicely drawn to scale, with its careful spacing, its large driving wheels and its small leading and tender wheels, look so rational and real that it is little wonder that we find ourselves looking upon it as showing the exact loads to be taken care of; but it is, in fact, only an approximation. Probably the nearest approximation in use, and yet not near enough to warrant the condemning of a simpler substitute on account of discrepancies of 2 or 3 per cent. in the results.

I am not disposed to the extreme views entertained by some, that the use of engine concentrations has been an unmixed evil in this country, but am strongly of the opinion that the average railroad bridge built during the past ten years, is better proportioned on account of it. The results obtained from its use seem to give satisfaction, and are what the advocates of uniform loads are trying to obtain, and they are willing to accept them as the standard for checking their shorter methods. For that purpose, I believe this method should still be retained; but for the every-day work of the engineer in dealing with ordinary girders and trusses, it seems analogous to the taking of ground readings to the

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nearest hundredth of a foot in ordinary earth-work cross-sectioning. The result is a vast amount of office work with quantities carried out to three or four places of decimals, but actually no nearer the true volume than could have been obtained from half the work. If to the expert, who from continuous practice is enabled to apply numerous short-cut rules in his computations, the engine concentration method is a "burden grievous to the borne," what is it relatively to the railway engineer, who, in addition to his other manifold duties, is called upon to check the calculations of half a dozen bridge companies every time he is allowed to replace an old wooden with a new metal bridge—this permission usually occurring at such rare intervals that at each "letting" he finds himself as rusty on the subject as he was at the former. The results which Mr. Waddell has obtained for truss spans are especially gratifying. If the variation is found to be no greater with different panel lengths, ranging from 20 to 30 feet, it would seem amply accurate for ordinary use.

Much routine labor may also be saved to both the railroad and bridge engineer, by the more extensive adoption of standards. There are, of course, cases on every railway system of any considerable extent, where the most complete set of standards must, from local considerations, be discarded, but such cases are not numerous. In preparing such standards no attempt should be made to include skew and such other exceptional structures as bridges for spanning large navigable rivers, but all ordinary cases may be fully covered. The length should vary by not more than 5 or 6 feet, up to clear spans of 30 or 40 feet; by not more than 10 or 12 feet to spans of 150 or 160 feet and for longer spans by not more than 15 to 20 feet. Some engineers have adopted standards, good so far as they go, but varying by so great a length that in too many cases they lead either to useless extravagance or dangerous economy. With spans varying by 30 feet, there is, perhaps, as much danger of making, in any particular location, the opening 15 feet too short as 15 feet too long, depending, of course, somewhat on how prominently the economic needs of the company are impressed upon the mind of the engineer. The proposed increase of clear roadway for through bridges, would doubtless give additional security, and may well be carefully considered by engineers in preparing standards. Where piers or abutments are high, the increased roadway, of course, adds quite an item to the cost of substructure.

Probably the next consideration in order of importance, after determining lengths of spans and widths of roadway, is depth of floor system and number and spacing of track stringers. These dimensions should all be fixed before proceeding to design standards for substructures. There should also be standard designs for the flanges of stringers, to facilitate the sizing and framing of cross-ties in the shop. This is especially desirable where timber is to be creosoted.

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Where roads pass through thickly settled districts, it will be well to design a special floor system of the least practicable depth for overhead crossings. With our present knowledge and experience, the standardizing of bridge superstructure, need not stop with the four or five sets of dimensions already mentioned. Before commencing the permanent bridging of a line, the Chief Engineer, together with an expert, if necessary, should take up the subject systematically and prepare complete designs for standard spans. Avoid such unusual shapes and details as cannot readily be produced at any well equipped bridge shop.

In any such set of standards there will, of course, be defects, and it will be found in time that the designs are behind the times and a revision is necessary. In the meantime the engineer will, however, have gained an experience from the use of several bridges of a kind under identical conditions of service, which will enable him to determine whether the defects developed are faults of material, workmanship, or design, with much more certainty than he possibly could if each structure was made from a separate design. He would also avoid the unsatisfactory task of wading through a half dozen or more sets of plans and checking as many sets of calculations for every bridge contract let (usually in a great hurry, as when an iron bridge is decided upon, it is wanted right away), and finally adopting, on account of price, a bridge less desirable than he intended, because some bridge company had taken advantage of some loophole in the specifications and "skimped" in places without actually violating their requirements. That our American method of bridge letting, which has stimulated competition in designing, as well as workmanship and prices, has been a potent factor in bringing the American railway bridge to its present state of excellence, is doubtless true; but it is also equally evident, that in very many cases, this competition in design has been carried too far. There seems now to be sufficient uniformity in the designing of ordinary spans to justify the calling of a halt in this branch of the competition. Let the railroad companies spend some of the money which such designing costs, in more liberal details and better floor systems.

A rational formula for the "intensities of working stresses," it seems to me should begin with strictly static loads. Starting from this basis, the effect of impact and vibration caused by different kinds of variable or moving loads may be provided for, either by additions to calculated stresses, or by subtractions from dead load intensities. By comparing several sets of specifications in common use, I find the ordinary intensities, in tension members, of railway bridges, to be, for "eye-bars" about 54 to 64 per cent., and for "plates and shapes" about 46 to 56 per cent. of the specified minimum elastic limit of the metal.

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By comparing specifications for iron work only, the deviations of practice are found to be somewhat less; with long compression members, of course, practice differs by a much greater amount, yet very rarely to the extent shown by the author in comparing Mr. Cooper's with Mr. Bouscaren's specifications, in the rather exceptional case of a post which carries no dead load. But even in that case, as I read Mr. Bouscaren's specification (1890 Edition), the actual working intensity would be reduced (from the 12 000 pounds per square inch as stated) by his addition to calculated live load stresses, to provide for impact and vibration, viz.:

"For web members of trusses 50 $\left(1 - \frac{d}{250}\right)$ per cent., where d = distance of member from center of truss." If, therefore, the post under consideration is at the third panel point of a 250-foot span with 25-foot panels, or 50 feet from the center of the truss, the added allowance is 40 per cent. of the live load stress and the actual intensity becomes $12\,000 \div 1.4 = 8\,571$ pounds per square inch, or about 43 per cent., instead of 100 per cent., over that obtained by Mr. Cooper's specifications—still, however, quite a serious discrepancy. We can hardly hope that all differences of opinion concerning "intensities of working stresses," will be reconciled in the near future; at least, till there is more uniformity in the quantity of metal used as well as workmanship in construction, followed by a systematic series of tests on full size members to establish a safe mean for all desirable lengths and shapes. But if all bridge engineers could agree to begin the solution of the problem at the same end, and consider the strains from each kind of loading separately throughout, as Mr. Cooper is now doing in truss members; it seems more than probable that many of the present differences, which are often more the result of individual taste in the use of formulas than of evidence obtained from independent investigations or experience, would gradually disappear.

If any part of a bridge must be skimped, it certainly should not be the floor system, and this should apply to ties and guard rails as well as the stringers and cross beams. The author's practice of bolting down through the stringer flanges instead of using hook bolts is good; not that the hook bolts are inefficient, but one of the important features of a floor should be the absence of bolt ends, nuts and other projections which would catch anything sliding over it, as a brake beam or broken down truck or car; therefore the bolt should pass down through the flange, with nut on the lower end, while the head and upper washer should be countersunk into the tie or guard rail whenever the stringer spacing will permit. The bolting of every tie to the stringer seems unnecessary, especially with stringers 8 feet apart or more. The dapping of guard rails 2 inches over ties with only 5-inch spaces between daps is objectionable, although the 2-inch projection, acting as

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a separating block, gets a better hold on the tie than a less depth would; but it is more likely to split off and be lost entirely. Probably the surest way to prevent the bunching of ties on a bridge is by means of separating blocks securely fastened between the ties. Where the stringer spacing is wide, as on long deck girder bridges and viaducts, requiring a tie of considerably more depth than breadth, very satisfactory results have been obtained by uniting them in pairs by means of a bolt and separating block at each end, with separating blocks also between the pairs.

Oak timber is more apt to warp and check in seasoning than pine. These defects can, to a great extent, be obviated by a little attention to the felling of trees at the proper time and protecting the timber from sun and wind while seasoning. Oak ties should be nearly square in section up to nine or ten inches, as the timber warps less than when one dimension is much greater than the other. Oak holds a track spike better than pine, which is a desirable quality, for, besides the danger of the track spreading when spikes get loose, water follows them into the wood and decay begins. I do not understand that pine is exempt from dry rot, at least some kinds of mountain pine are so affected.

If re-railing devices and collision piles are used, they should be placed so far away that whatever of good or evil they do to a derailed car may be fully accomplished before the car reaches the bridge. Neither the number nor efficiency of guard rails should be reduced on account of such devices, for it sometimes happens that a truck is so slewed that it will not keep the track for any distance, even if effectually replaced by such means.

Bridge ties should be ample in dimensions and closely spaced, with at least inside and outside guard rails made smooth and continuous the full length of the structure—for the same reason that bolt ends, nuts, etc., should not be allowed to project above them. When ties are close spread, 12 or more feet in length, of suitable, well seasoned or chemically prepared timber, they aggregate a considerable item of expense, and their preservation becomes a subject worth careful consideration. Bolt and spike holes are doors for the admission of moisture and their number should be reduced to a minimum. The cuts and bruises from the flanges of a single pair of wheels off the track may reduce the life of a set of ties by one-half. This fact, together with the desirability of reducing the shocks produced by derailed wheels, has led to the consideration of this proposed plan (Plate XXI), which is, for the most part, a combination of several well tried features picked up from the practice of different railways. The separating blocks of cast iron for 4-inch clear spacing will weigh about 7 pounds each, and may be secured to each tie with a wood screw, or in pairs to each alternate tie, with a machine bolt. If ties are to be creo-

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soted these holes may be bored before treatment. Where separating blocks are used and stringer spacing will permit, the holding down bolts may merely pass between the ties, engaging only the guard rail and stringer flange.

The turning of the horizontal edge of the angle iron used for the inside guard rail toward the rail, providing a smooth path for the flange of a derailed wheel, is not new; but the addition of a plate for the same purpose outside the rail and riveting both to angle iron cross bars instead of bolting or spiking to the ties, are only the propositions for the consideration of those who may be interested in the improvement of bridge floors. If the company can afford old or light rails for additional inside guards, as shown on the left half of the plan, the security will be increased. Longitudinal planks covering the ends of the ties may also be of service.

JOHN STERLING DEANS, M. Am. Soc. C. E.—The paper rccently presented to the Society by Mr. J. A. L. Waddell embraces subjects of the greatest interest in an important branch of engineering and as such should be thoroughly discussed as requested by the author. The first point in the paper on which there is certainly great difference of opinion, is in fixing the limit at which plate girder construction shall be used, at 100 feet. In the opinion of the writer this limit is much too great and it is not a wise expenditure of money to pay for the extra cost of construction in spans of over 75 feet. Above this length the chords and diagonals of a well designed lattice girder are necessarily of such a size as to admit of a satisfactory arrangement of rivet spacing at important connections, and also to make it possible to have the centers of gravity of the various members intersect at one point, so that the question of secondary stresses is reduced to one of little practical importance. An engineer with the latest improved system of power riveting at his command does not seem warranted in adopting plate girder construction of over 75 feet lengths. This extra expense is not one simply of extra metal, but of extra shop cost, handling and erection in the field. Again, in a lattice girder internal forces act along well defined lines, and in this respect it more nearly approaches the perfect "American" pin-connected truss and differs materially from the plate girder, in which, as Mr. Waddell suggests, the exact manner of stress distribution is an open question. The lattice girder design can be economically and satisfactorily used up to spans of 135 feet; beyond this the sections become unwieldy for riveted connections and the pin style of truss should be used. At this day, with so many numerous examples about him, no engineer should be upheld in using anything but a single intersection main web system for the longest and heaviest truss spans.

Live Loads.—Under this heading Mr. Waddell strongly advocates the adoption of standard uniform live loads, as against wheel concen-

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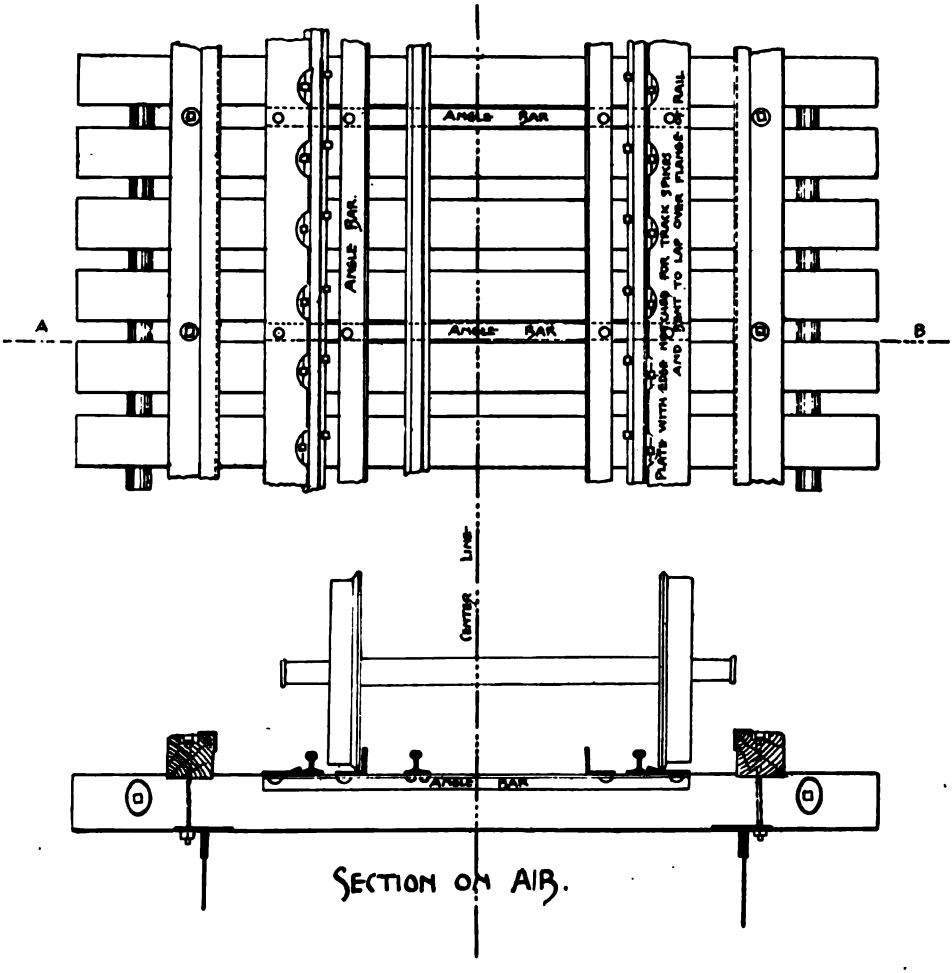

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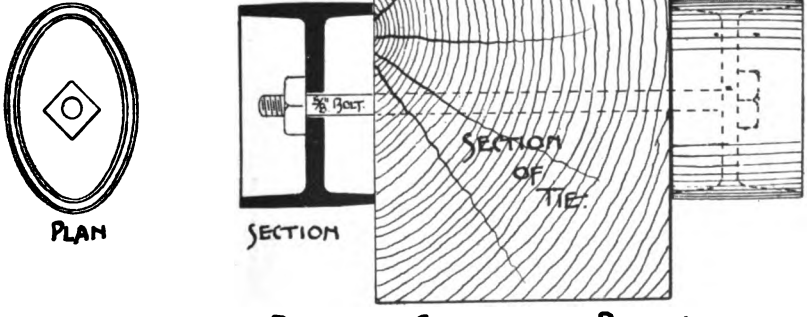

DETAIL OF SEPARATING BLOCK.

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trations; not that it is the more exact method, but in order to relieve the burden of the bridge computer. The writer is well aware of the character of such computations and of the really enormous amount of unnecessary work put on the bridge engineer, but thinks it is not caused, as Mr. Waddell intimates, by simply being called upon to use wheel concentrations in determining the stresses in various members, but by the innumerable kinds of loading, differing by an inch or two in wheel spacing and by a few pounds in weight on axles. That computations by wheel concentrations for a fixed load, give the more exact result is not disputed, and it is certainly the manner in which loading is applied to a railway structure. It would seem to be more logical to adopt certain standard engine and train loads, and wheel spacing.

By adopting a heavy and light type for the "Consolidation," "Mogul," and "Passenger" engines, and light and heavy train loads, all cases would be covered. With these two types settled upon and moment tables figured, the computer would soon be familiar with the loading which would give maximum results without trial, and it is a question whether it would not be the easier method, as it is certainly the better one to present to the average railroad officer. The latter would always question the propriety of using a uniform load, even though it might really closely approximate to an equivalent load, when he is aware that his bridge is subjected to the action of loads placed on it through axles and wheels. The writer believes that much more can be said against the ridiculous variety of types than against the method of computing stresses; he also believes that the great number of specifications which every railroad consulting engineer, and inspection bureau, seem to think it is absolutely essential should be used in designing their particular structures, are the cause of much the greater part of the bridge engineer's work. No one not actually engaged in the designing and constructing of bridges has any conception of the mental labor involved nor the wearing effect of being compelled to work with these myriad specifications, differing often in really minor particulars; but these differences must not be overlooked, as they are each and all considered of vital importance by the author of the specifications. As a sample of these differences, a prominent railroad company calls for its steel eye-bars to fill the following specifications:

> Ultimate strength at least 56 000 pounds per square inch.
> Elastic limit at least 33 000 pounds per square inch.
> Minimum stretch of 10 per cent.

While another equally prominent company believes it is essential in order to secure first class work to have its eye-bars, having precisely the same duty to perform as those first mentioned, manufactured according to the following specifications:

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Ultimate strength at least 60 000 pounds per square inch.
Elastic limit at least 30 000 pounds per square inch.
Minimum stretch of 15 per cent.

This is only one instance out of hundreds of the differences met with and in fact is hardly a fair sample in order to make my point, for in such an important member as an eye-bar there may be some excuse for great difference of opinion as to the best specifications and best mode of manufacture to follow. There are many cases known, in which there appears to be not the slightest reason or excuse for differing from the regularly accepted practice. The parties or society that are instrumental in securing a standard bridge specification, or say two or three standards (as it would probably be well to provide for first, second or even third class construction and loads), would indeed be entitled to the gratitude of not only the bridge engineers of the country, but that sorely burdened class, the bridge constructors. It is next to impossible for these constructors to so arrange their shop practice and mill processes, as to meet the various whims of all authors of specifications. It would seem as if this matter of a standard specification could be settled upon much easier than a standard uniform loading, and in fact if specifications continue to be added to the present long list, the question must be met. As long as engines and cars are used to load structures, it will always be more rational to use typical engines and cars in designing them, and in the opinion of the writer we must look to the "Standard Specification" to relieve the sorely tried computer in whose interest Mr. Waddell has taken up the subject.

Frederick H. Smith, M. Am. Soc. C. E.—In discussion of Mr. Waddell's timely paper, I would say, that his remarks on the flimsiness of web systems resulting from excessive curvature of top chords recall the controversy on this subject which raged in the early fifties. The builders of arched or bow string bridges thought the world could get along with spider web systems in bridges; the Howe truss men admitted that arches had their uses occasionally, but only as auxiliaries to trusses with heavy web systems, and McCallum combined the two plans by mounting an arched top chord on end arched braces, and lightened up his web system about proportionally. In 1852 a committee of engineers examined and reported on a McCallum bridge of 200 feet span over the Susquehanna River at Lanesboro, Pa., and in 1852–53 much of our engineering class-work at Pittsburgh was devoted to the discussion of this subject. In the fullness of our wisdom we decided that McCallum's combination of the two plans was worthy of our valuable commendation, but that he was not the first to build bridges embodying such combination; as we measured, illustrated, computed and discussed several small bridges in Allegheny County which had laminated curved top chords, vertical end posts, and variously inclined braces and rods all reported by the neighbors to have been built by "Jim Finley," the man who first built iron suspension bridges in this country.

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Some of the Howe truss men were so impressed by McCallum's business success (if not by his arguments) that they began arching their top chords, and a notable example of this practice was the Rock Island Bridge over the Mississippi River, which was removed when the present iron bridge was built by the late Baltimore Bridge Company in 1871–72. In the Rock Island Howe truss the web systems were as heavy as they would have been had the chords been parallel, and this is also the case in the curved top-chord bridge built in 1867–89 over the Missouri River at Kansas City, by President Chanute. The practical aspects of such curvature were carefully looked into in 1869–70 by the late C. Shaler Smith, M. Am. Soc. C. E., and myself, in the Western office of the Baltimore Bridge Company, in connection with plans for a proposed tripartite bridge at Pittsburgh; and we found that a double intersection truss with radial posts and parabolic top chord required theoretically the least material. The attenuated web system, however, seemed to us to need the addition of about as much gratuitous metal as would wipe out this advantage, and as the shop and erection costs were excessive and the raking posts necessitated a suspended floor system, we dropped the subject in a business way, although I patented the radial posts as being the only feature that had not been anticipated by the Jim Finley and other curved top bridges. The radial posts, by the way, had been first used by me in designs for a canal bridge to be manufactured by the Tredegar Works in Richmond, for Shaler and Fred. Smith, and this bridge, together with three 100-foot spans of Fink truss, which we had in hand for Chief Engineer H. D. Whitcomb, M. Am. Soc. C. E., for Jackson River, Virginia Central Railroad, were stopped by the outbreak of war in 1861.

In the matter of suspended floor systems, I note that some men who advocate this practice for modern beam trusses have omitted the "saving clause," so to speak, which was used by the builders of the old suspension trusses and the earlier beam trusses, viz.; the use of a compensating suspension link and of longitudinal strut-ties between the ends of the floor beams, all so fixed that the lateral vibration was carried directly to the masonry without affecting the truss, and the truss could expand without affecting the floor system. In the absence of compensating suspension links, a suspended floor system, if bolted up tightly against the bottoms of the posts, will develop sooner or later some openings between the ends of stringers, and such openings had best be included in the design, as Mr. Waddell states. This is equally true when the floor beams are riveted in between the posts, either above or below the chords, unless indeed they should be on the neutral line of the truss; but trouble from such openings between stringers need not be looked for except in very long spans.

The north span, 255 feet long, in the Susquehanna bridge of the P. W. and B. R. R., at Havre de Grace, designed and built in 1873–74 by the

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late Baltimore Bridge Co. (the shop work being the first output of the Edge Moor Iron Co.), has floor beams riveted or bolted in between the posts above the chords, with stringers between the floor beams fished to each other over the beams, and resting on and bolted to end floor beams which ride on the end post shoes. This north span flattened its rollers somewhat, but the remaining eleven spans of this bridge built subsequently with the same floor details by the Baltimore Bridge Company and the Phœnix Bridge Company rolled freely, and I have never seen or heard of any serious distortion or other trouble arising from differences of expansion or contraction between the floor system and the chords. These stringers, however, are bolted (not riveted) together, the purpose being to allow for expansion at each joint. I have always used the above described end floor beams riding on the end shoe, and fully agree with Mr. Waddell that it is very necessary. When the conditions are such that a suspended floor beam is necessary, I rivet it to extensions of the posts below the chords and rest the stringers on sliding shoes on the wall, and attach them and the lateral rods to struts which move with the end shoes, and thus keep everything drawn taut.

Mr. Waddell is undoubtedly right in advocating long panels and single intersections, and in subdividing these panels in long spans, but I am not at all convinced that by carrying the sub-panel load to the rear, or by any other device, he can effectually support his top chord with a sub-post at mid-panel. There have been removed and replaced this year several spans of what we called the "sub-truss Quadrangular" bridge, built by us in 1868–69, on the old Cincinnati and Baltimore (now B. and O. S. W.) Railroad. These spans were built to carry such wheel loads as iron rails would carry, and although they had recently been crowded beyond double duty, they never let go. The sub-truss eye-bars ran fore and aft to the heads of the main posts, and we had to use full panel lengths in figuring top chords in overgrade spans, as we found that the sub-posts slacked away from the top chords under loading, just as in Fink trusses. The sub-truss does its work independently of the main truss, and puts a local and secondary girder strain on the top chord if the sub-post is attached thereto. While on the subject of top chords, I wish to express my satisfaction that so many specialists are now using chord joints in which the joint line cuts the pin center and the sections are held together by the passage of the pin through inside and outside overlapping plates with no field riveting. When I first used this joint in 1878, in New River Bridge, A. M. and O. R. R. (now Norfolk and Western), it was irreverently described as a moderately good "Highway" bridge joint, but it has made its way, and is now in very general use.

Mr. Waddell's position on the matter of using equivalent uniform live loads is well stated and argued by him, and I, for one, will be very glad to welcome his proposed contribution of tables therefor to the

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Society; but I hope he will include approximate percentages of error on the safety or the danger side of the line, and also some modifications in engine loads to cover the case of the decapods. The rule for finding floor beam concentrations by using in one panel length the equivalent uniform load per foot for a span of two panel lengths the discovery and announcement of which is made by Mr. Waddell on his pages 6–7, has long been in use in my office, and was fully explained some years ago by Mr. Edwin Thacher in a paper to a Western Engineering Society in connection with his very valuable tables of equivalent distributed loads published in the Keystone Bridge Company's specification of 1887.

Mr. Waddell's proposition for us all to join in abolishing such superfluities as the second powers of lengths and radii, is one that I would very much like to see carried out if he will suggest a substitute which will change the method without materially changing the results. A good many computers have attacked this problem in the hope of lightening their own and others' labors, but no consensus has been reached, and there are yet at work some men (engineers, too), who, after proportioning by squares of radii, do not sleep well until they have again gone over their work in terms of diameters. Possibly their brain matter has crystallized.

In the matter of intensity of working stresses constituting Mr. Waddell's Group "D," I offer some suggestions in the way of items from a specification upon which I have just received tenders for about 1 600 tons of riveted lattice girders and columns, and a couple of pin connected spans for the North street viaduct in Baltimore. Under the combined live and dead loads, working strains per square inch in tension in steel and iron members having net sections of 1 square inch or less, shall not exceed 34 per cent. in the case of riveted members, nor 37 per cent. in the case of eye-bars of the herein prescribed elasticity of the material of which the member is made; and this percentage shall be reduced by one-tenth of 1 per cent. of the elasticity for each additional square inch of net section in built members, or in eye-bars taken singly.

Working strains per square inch in compression in steel and iron shall not exceed 32 per cent. of the herein prescribed elasticity of the material of which the member is made, for sections of 1 square inch or less, and this percentage may be increased by one-tenth of 1 per cent. of the elasticity for each additional square inch of gross section in the member, the strains thus determined to be reduced by division by $1 + \frac{L^2}{(18\,000\ R^2)}$; by $1 + \frac{L^2}{(24\,000\ R^2)}$; or by $1 + \frac{L^2}{(30\,000\ R^2)}$ for members with two pin ends, or one pin and one flat end, or two flat ends respectively. This percentage of decrease in allowed tension per inch, as the sections increase, is a recognition of the well known fact,

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that as rolled metal increases in section and especially in thickness, it does not get so much mechanical work put into it by the rolling (measured per pound of product), and full size tests show very generally that its actual tensile strength decreases about proportionally. Shearing strains in field rivets of 20 per cent. and elsewhere of 30 per cent., bearing strains of 50 per cent. and bending strains of 50 per cent. respectively of the elasticity. In tension members composed of plates and shapes riveted together, the local stresses produced by driving rivets one at a time close to each other, and the uncertainties about filling the holes of interior plates when long rivets are used, especially in splicing heavy sections in the field, combine to reduce the certainties in the case of large riveted members far below the certainties in the case of eye-bar members, which is my reason for taking eye-bars singly when decreasing percentages.

It will be contended that there really should be no decrease in allowed working strains in tension, on the ground that the larger the sections the longer the span, and therefore the less frequency in the application of the full load; but this was a good argument before modern railway practice confined freight traffic to full cars and long trains. Another argument against the decrease of working strains in tension as the sections increase is, that as the sections increase the proportion of dead load to total load also increases; but I submit that when the span is loaded, it is loaded, and the combined live and dead loads are working together (with some suspicion of impact from the acquired momentum or drop of the dead load also), and the main system should at that moment have metal enough in it to do the work. The formula for proportioning by minimum and maximum stresses needs further examination, and I hope Mr. Waddell will persuade himself and others to put through his proposed series of experiments. I will help him.

The percentage of increase in allowed compression per inch as the sections increase is in recognition of the fact that compression (instead of pulling things apart, as tension does) presses things together, and of the further fact of the greater resistance to flexure and shock as the metal thickens and sections increase. The larger sections are thus in every way more reliable than small ones, and can do proportionally more work and with greater safety. It is but fair that after specifying working strains per square inch based upon percentage of the prescribed elastic limit, I should also submit some of the items governing quality of the materials upon which I impose these stresses. I specify, among other things, that all iron shall have an elastic limit of 25 000 pounds per square inch; that standard specimens cut from plates, bars or shapes of one square inch section shall show an ultimate strength of 50 000 pounds and a stretch of 15 per cent. in 8 inches, and for each additional square inch in the bar, the specimen cut from it may show a

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reduction of 150 pounds per square inch in ultimate, and of two-tenths of one per cent. in the stretch. Full size plates, bars or shapes of one square inch section shall show 50 000 pounds ultimate, and 12 per cent. stretch in 10 feet, and for each additional square inch of sectional area they may show a reduction of 200 pounds per inch in ultimate and of four-tenths of 1 per cent. in stretch. Steel for rivets must show an ultimate between 53 000 and 57 000, an elastic limit of 28 000, all in pounds per square inch of full size rivet bar; a stretch of 25 per cent. in 8 inches and reduction of 50 per cent. Standard specimens cut from full size plates, bars or shapes intended for riveted work, and having a section of 1 square inch or less, shall show an ultimate between 55 000 and 62 000, an elasticity of 30 000, all in pounds per square inch, a stretch of 22 per cent. in 8 inches and reduction of 44 per cent.; for each additional square inch in section of plate, bar or shape the specimen cut from it may show a reduction of 100 pounds per inch in ultimate and of one-tenth of 1 per cent. in stretch, and this steel may be punched without reaming and be sheared without planing.

Specimens cut from steel for eye-bars, rods, pins or rollers having a sectional area of 1 square inch or less, shall show an ultimate between 60 000 and 68 000, an elasticity of 33 000, all in pounds per square inch, a stretch of 20 per cent. in 8 inches and a reduction of 40 per cent.; and for each additional square inch of original bar the specimen cut from it may show a reduction of 100 pounds per square inch in ultimate and of one-tenth of 1 per cent. in stretch. Full size eye-bars of this steel of a sectional area of 1 square inch shall show an ultimate between 60 000 and 68 000, an elasticity of 33 000, all in pounds per square inch; a stretch of 16 per cent. in 10 feet, and a reduction of 35 per cent.; and for each additional square inch of section they may show a reduction in ultimate of 150 pounds per square inch, of two-tenths of 1 per cent. in the stretch, and of four-tenths of 1 per cent. in reduction of area. Annealing, reaming and planing are prescribed for this steel.

It will be noted that there is no call for a specimen to be rolled down and tested for quality of steel in each melt, and I prefer that specimens for testing (except for rivet steel) shall be cut out of a full size bar, as this gives a test of the actual mill work as well as of the quality of the steel. If we assume that steel at a white heat is a mass of spherical molecules in a plastic condition, their cohesion is due to the limited contact of spherical surfaces; but if we roll or compress this very hot steel, the spherical molecules become polyhedral, presenting facets (or flat instead of spherical surfaces), for cohesive contact, and if we do not continue this rolling or other compression until the steel gets red cool enough to "hold its work," the hot molecules will resume more or less their spherical shape when the pressure is removed. Hence, I prefer to cut specimens out of full size plates, bars or shapes, after the rolling is done, and the intention of the above specifications

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for materials, is to legalize for the benefit of our inspectors about what we actually get out of the shops in general practice.

I am aware that in the foregoing percentage of increase or decrease in working and test strains, there is no bumper provided against sliding up or down into infinity; but the bridge engineers of this country are reasonable men and are very apt to use reasonable sections; and reflecting on this fact is what encourages me to hope that Mr. Waddell will be reasonable about admitting such a lengthy and discursive contribution to his "asymptotic curve."

H. H. QUIMBY, M. Am. Soc. C. E.—If the details of bridge connections were thoroughly scientific, and secondary strains properly cared for if not eliminated; the intensities of stress as prescribed now and for some time past in railway work, would make the possibility of the failure of a bridge of modern design, under any conceivable load, so remote that laborious computations to find a possible slightly greater stress might reasonably be regarded as unnecessary refinements. But with specifications and practice as crude in the matter of details as they often are, it is only wise and prudent to provide for the greatest concentrations of the rolling loads expected to be carried, even if they are only two per cent. greater than the approximations.

Specifications which are minute in respect to loads and unit stresses, in many cases ignore important and even vital considerations. The disastrous collapse of a railway bridge in Europe a few months ago was, by some engineers, attributed to the faulty connection of the lateral bracing to the trusses, and similar reasons have been given for the fall of bridges in this country. The trusses and lateral systems may be proportioned for their given loads without allowance being made for the fact that one system can operate to complicate and interfere with the action of the other. The bending effect of both end and intermediate sway bracing on the posts of through spans is often serious. The engineer who prescribes a given wind load to be carried down a certain post, may, without perceiving the anomaly, permit the builder to proportion the post for compression only and to treat it as being fixed at the point of attachment of the sway bracing, thereby reducing its ratio of length to radius of gyration. We hear objections to the detail of riveting floor beams to posts because the deflection of the beams springs the posts out of a true line, although the fastening is ample to keep the post in the control of the floor beam, and make its lower end absolutely fixed. The danger from poorly designed or improperly adjusted transverse bracing is vastly greater, but is often overlooked.

This subject, together with Mr. Waddell's declaration of hostility to adjustable bridge members, recalls a case wherein the intermediate sway rods of a through span—which were attached to the posts a short distance below the top struts—had been screwed by the erectors so tight that they had sprung the top ends of one or more of the posts

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out of line enough to excite the surprise of the inspecting engineer that the bridge had not collapsed. A bridge should be designed so that its safety cannot be imperiled by a lack of judgment on the part of erectors. This incident teaches also that it is of the highest importance that careful attention be given to proportioning the jaws of posts, particularly such as have their flanges cut away to facilitate the chord packing. A safe formula for the jaws, deduced from experiments on full size pieces, is as important as one for the section of the member, and as the work of clipping, punching and riveting generally tends to curve these jaws, their inspection is equally important.

JNO. A. FULTON, M. Am. Soc. C. E.—*A.—Live Loads.*—It is a good practice to plot the engine diagram and train load to a scale of say ten feet to an inch, then on a separate strip of paper lay off the proposed panel lengths, and place the one under the other in such position that the leading panel point shall come immediately under the second or the third driving wheel (it will not make much difference which) and distribute all the loads to the several panel points by the law of leverages. We then have a series of live loads equal in amount and very nearly equivalent in effect to the actual live load of engine and train, the leading load in this series being considerably greater than any of those following. Then making no account of the counter effect of the small live load which always exists at the panel point next in advance of the engine drivers, slide this series of live loads across the strain diagram from panel point to panel point, assuming that the web shear under the leading load is the maximum on members leading from that point. In this way determine the shear on all web members, including the end posts. This method of using a fixed series of loads for a given panel length, regardless of the number of panels in the bridge, may not always be strictly correct, and the position of the engine wheel base with reference to the panel point under it may not always be exact; but the method is easy of application and the results as close as we can proportion the sizes of material.

Having thus determined the vertical shear on the end post, find the bottom chord strain for the first one or two panels and add increments for succeeding panels to the center, precisely as for a uniform load which would produce that shear on the end post. At first sight this would seem to give excessive chord strains for all but the one or two panels at the end; but if it be borne in mind that it is not at all infrequent for the rear end of one train to be close against the front end of a following train when standing on a bridge or elsewhere, or that a dead engine is being hauled about the middle of a train, or, worse still, immediately behind the hauling engine, then it follows that for maximum chord strains at the center of the span the engine should be placed at the middle of the train instead of at the end. With the engine at the center of the bridge and the typical train immediately in front and behind it,

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the chord strains will be correct, as determined so easily by the method of increments above described.

For the maximum live load on a floor beam it is a simple and short operation to slide the engine diagram back and forth over a panel point until the position for maximum effect is determined, and then distribute the loads to the floor beam by leverages as before described for panel loads.

In the case of deck plate girder bridges, the engine diagram, so placed on the strain sheet that either of the two central wheels shall be at the center of span, readily gives the maximum bending moment at the center, and the same diagram slipped along until either of these two wheels is at the quarter point of the span, gives in the same way the maximum bending moment at that point. A very quickly obtained, and perhaps sufficiently close approximation to the value of the bending moment at the quarter point, is reached by calling it 77 per cent. of that at the center of the span. Having, then, the bending moment at the center, at the two quarter points, and zero at the ends, and laying off these as ordinates from a straight line, and sketching a parabolic curve through them, we readily determine the bending moment at any point.

Placing the engine diagram so that the point driven is directly over (or rather just inside) the center of the base plate and distributing as before described, gives the maximum live load shear on the web. In the case of through plate girders with stringers and floor beams, the load should be distributed to the panel points as in a truss bridge. In any plate girder in which the web is spliced, great care should be exercised to see that the splice has rivets enough to carry the shear at that point.

Concerning live loads generally, I am decidedly in favor of using every legitimate means for obtaining heavy bridges, since to add, say, 10 per cent. to the capacity of a proposed bridge does not add 10 per cent. to the cost of it. I would use for a typical live load, not an engine followed by a train of cars weighing from 10 to 20 per cent. less per foot than the engine, but a solid train of engines, and would figure the engines heavy enough to cover a good many contingencies not altogether foreseen just now. No one can safely predict what the engine of the future will be or that the tendency of the last few years to increase engine weights has reached its limit. Had the bridge engineer of twenty years ago made a fair allowance for the future loads on his bridges, there would not now be the necessity for replacing so many of them.

If all parties interested could be convinced that the typical live load should be a train of engines, then I should be in favor of using a uniform live load for web and chord members; but for floor beams, stringers and short span girders I would use actual concentrations as before described, or else such a uniform load as would produce strains equal in all cases to those produced by actual concentrations. The admirable series of typical engines proposed by Mr. Waddell would seem to leave

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nothing unprovided for in the way of live loads if he would leave off the train load and substitute engines instead.

The policy of the average railroad company, which unhesitatingly builds its embankments 16 feet wide and excavations 22 feet wide, either of which might be made less in the first place and increased at any time afterward if found too narrow, and yet shaves down the cost of an iron bridge to the lowest mark, is economically unsound. To strengthen it after completion will be expensive and difficult, if not altogether impracticable, and the company thus secures weak spots in its roadway at points where, of all others, it should have excessive strength, since failure at such points is sure to prove most disastrous. The saving is, perhaps, $10 000 on a $100 000 contract, where the rest of the roadbed costs millions. The typical bridge should not be a source of dread either to the public who do not understand it, or to the employees who do.

B.—For wind pressure I would suggest 200 pounds per linear foot on the unloaded chord, and 500 pounds on the loaded chord (300 of this to be moving load) for all spans up to 150 feet. For spans 500 feet long I would suggest 350 pounds on the unloaded chord and 650 on the loaded chord, with some portion of this as above for moving load, and proportional amounts for intermediate spans. For the effects of wind load I would assume that all such loads on the top chord would pass through the top laterals and end posts to the abutments without relief from the vertical sway bracing at panel points; and would then make each interior set of sway bracing sufficient for a single panel, with the usual provision as to the minimum size of material to be used, and would assume the indirect effect on the bottom chord to be uniform from end to end of span.

C.—I would use plate girders up to 80 feet, riveted Warren through girders from 80 to 125 feet, Pratt trusses from 125 to 225 or 250 feet, and subdivided Pratt trusses from that up; but see no serious objection to a lattice deck bridge of two systems of triangles, provided they are not run together into one system just before reaching the abutments.

As to spacing of stringers and webs of plate girders, 6 feet 6 inches has to recommend it a reasonably small bending effect on the ties under ordinary conditions, and a reasonable safety for the outside wheel of a derailed truck. To use outside stringers that can be called into action only in case of derailment is clearly to waste material. Oak bridge ties 8 x 8 inches x 11 feet, notched one-half inch over stringers and spaced 12 or 13 inches center to center, with oak guard rails 8 x 8 inches at the outer ends of ties, either with or without inside guard rails; make a stiff floor, and are not cut to pieces by derailed wheel flanges, as would be the case with white pine ties.

D.—Intensities of Working Stresses.—In test specimens we may re-

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quire an elongation of 20 per cent. and ability to stand a sharp bend without sign of fracture, but in bridge wrecks we do not find any such elongation nor any such bending. Doubtless, the difference in treatment is the cause of the difference in results. Attempt to cut ordinary molasses "taffy" with a knife or to twist it or pry it apart and the task is quite troublesome, but strike it with a light hammer and it is found to be about as brittle as glass. The former experiment shows how we test material, the latter shows how we use it. If some one would devise a scheme for testing bridge members which would treat them about as they are treated in a bridge, the results of such tests for elastic limit and for ultimate strength would be exceedingly valuable to the bridge designer, but they might differ somewhat from the values generally assumed.

To illustrate an important point, assume a Pratt truss of ten panels with a uniform live load advancing over it. Leaving out of account the effect of dead load, the vertical shear on a theoretical counter in the second panel will be one-tenth of a panel load; that in the third panel will be three-tenths of a panel load, but the increment is only two-tenths, the other one-tenth being already in that member. Proceeding onward to the end post at the further end of the span, we find the vertical shear on that member is forty-five-tenths of a panel load, of which only nine-tenths of a panel load or 20 per cent. of the entire load is the increment, the remaining 80 per cent. being already in the member from preceding loads; and whatever the number of panels, it is evident that the increment will never quite equal one panel weight. Now it seems clear that when the end post gets its maximum live load, this maximum load will consist of nearly all of the last panel load, plus the impact for that amount of panel load, plus the proportional amount of all preceding panel loads without impact; since the effect of these preceding loads has reached the end post before the strain on it had become a maximum, and has reached it not suddenly, but gradually.

It should also be borne in mind that the shear from loads near the rear end of the span reaches the forward end post through a series of interior posts and diagonals equivalent in effect to one very long, and therefore elastic, member. If this view be a correct one, then the effect on the end post of any bridge from each ton of live load is but very slightly in excess of that for each ton of dead load, or, in other words, the impact due to high speed counts for a relatively very small amount on the end posts, but increases toward and becomes a maximum at the first (theoretical) counter. It would therefore seem that in proportioning the web members of a truss this variable effect of impact, a maximum at the beginning of the span and a minimum at the end of it, should be the measure of intensities of working strains on web members.

I would suggest that the web strains be determined by any satis-

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factory method, making due allowance for the negative effect of dead load on counters, and that the strains thus obtained be then increased by percentages determined as follows: Let n = the number of panels in any truss, and $n - 2$ = the number of panels requiring an additional percentage. Then $\frac{100}{n-2}$ = the increment to be added at successive panel points.

Take, for instance, an eight-panel bridge. Then $n - 2 = 6$; $\frac{100}{6} = 16\frac{2}{3}$; hence, the percentage to be added to the strains on the six interior panels will be respectively $16\frac{2}{3}$, $33\frac{1}{3}$, 50, $66\frac{2}{3}$, $83\frac{1}{3}$, 100; or approximately the strains on the main web members will have their amounts increased from 0 at the end to 50 per cent. at the center, and on counters from 50 per cent. at the center to 100 per cent. at the end. With strains thus increased it would be proper to use a constant intensity of say 9 000 pounds on main members, and for additional security 8 000 pounds on counters.

Concerning formulas which involve both the maximum and the minimum stress, if a given truss rod is to have a maximum strain of 50 000 pounds and a minimum of 20 000 pounds, while another rod is to have a maximum of 50 000 pounds and a minimum of 1 000 pounds, it is not quite clear why there should be any difference in the size of the two rods, unless we are ready to abandon the theory that iron not overstrained is permanent in strength.

Another idea occasionally appearing in bridge literature, with seemingly not sufficient cause, is that because a certain excessive strain is likely to occur only at long intervals, it is therefore not objectionable, while, if it were to occur at short intervals, it must be provided for by using additional sectional area. If we are proportioning our bridges for certain train loads with the idea that these bridges are sufficient for the purpose, but would not be sufficient for a continuous series of such trains hour after hour and day after day for an indefinite period without any deterioration whatever except for the wear of the elements; then, in the interest of those who have to pay for these bridges, it would seem that our unit strains must be too high and ought to be reduced. An organic body existing by the daily destruction and renewal of animal tissue may become "fatigued" by work which is far within its safe capacity and may require rest, and is certain to enter into a long rest within a very limited period; but it would seem that no such doctrine as this could be applicable to iron and steel, which have nothing within them to require renewal.

E.—As to combined stresses, where wind stresses and load stresses both occur in the same member and are of the same kind, I would determine the sectional area required for each and add the results. If it be good designing to allow chord members to be overstrained 20 or 25 per cent. during wind storms, why not reduce the sections of web mem-

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bers to correspond? It would seem that a bridge might as well be weak or might as well fail in one place as in another. As to bending of end posts on account of wind strains, if end floor beams be used and well connected to the lower ends of end posts by means of gusset plates, it would seem fair to consider the lower end of the post as fixed, and thereby reduce the lever arm of the bending moment 50 per cent.

F.—In plate girder proportioning I would advise the use of nothing less than three-eighths inch for thickness of web plate. Whatever its thickness, it should have sufficient net area to resist the vertical shear at any point with a low unit strain and should have sufficient bearing area for the rivets in any portion of the flange. In long shallow girders this will require a web much thicker than three-eighths inch. One point often overlooked in plate girder designing, in fact it is perhaps generally overlooked, is the thickness of wall plates and base plates at the ends of a girder. Many girders having a bottom flange consisting of two angles and one or more cover plates, have the first cover plates say three-eighths inch thick made the full length of the girder, and dispense with a base plate altogether; thus using material where it does no good and omitting to use it or not using enough at the place where it is needed. On account of the deflection of a girder under a load, the inner edge of a base plate is certain to get more load than the outer edge; it is desirable, therefore, that the end bearing of a girder be made as short as practicable lengthwise of the girder, and spread out laterally for the necessary bearing area. It seems entirely possible that the plate girder of the future will be built on a rocker bearing, to secure uniformity of pressure on the masonry as in a pin connected truss. The projecting part of a wall plate or base plate should be figured as a solid shallow beam with a uniform load pressing upward, and the base plate should be well riveted to the bottom of the flange angles or it will lack much of its supposed stiffness. In order to have a base plate well supported it is necessary to use web stiffeners of ample projection, and that these stiffeners be so placed that they will distribute the load over the base plate to the best advantage.

In the designing of floor beams, it would seem difficult to devise a worse arrangement than that of destroying the floor beam web in order to run the bottom chord through it instead of over or under it, and then patching up the weak spot by the queer devices sometimes used. There would seem to be no difficulty in building floor beams with flanges parallel between trusses, and then riveting them between the posts above the bottom chords where practicable, or below the chords if desirable, extending the posts for that purpose to the bottom of the floor beam, but riveting the connection in all cases. If the longitudinal strain from lateral rods would produce too much bending strain on the posts, the difficulty could be easily remedied by using light tension members, connecting the foot of the post with the bottom chord pins at adjacent panel points after the manner of the Fink truss.

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BENJAMIN DOUGLAS, M. Am. Soc. C. E.—It is not quite as simple a matter as Mr. Waddell thinks to find a uniform load which will cause the same stresses in all the members of any span as will a system of concentrated loads. That a uniform load can be found which will cause nearly equal maximum shears and bending moments is true, as has been shown, and in a Pratt truss with parallel chords the maximum stresses in the members will also be nearly equal for the two systems of loading. In compound trusses, however, when the entire shear or bending moment is not resisted by a single member, the relative distribution of the loads may be important and the uniform load cause maximum stresses differing much more from those caused by the concentrations. This is the case with the Whipple truss, for example.

In the following table are given the vertical components of the maximum stresses in the diagonal tension members of a Whipple truss having 10 panels of 25 feet each, computed for Mr. Cooper's "Class A" diagram, and for a uniform load of 3 112 pounds per lineal foot, which is given by Mr. Waddell as equivalent to the loading of the diagram.

MEMBER.	VERTICAL COMPONENT OF STRESS IN POUNDS.		PERCENTAGE OF ERROR.
	By Diagram.	By Uniform Load.	
1st Main Diagonal	177 500	155 600	12.3 Danger.
2d "	143 700	124 500	13.4 "
3d "	109 900	93 400	15.0 "
4th "	83 600	70 000	16.5 "
1st Counter	57 200	46 700	18.4 "
2d "	38 300	31 100	18.8 "
3d "	19 900	15 600	21.6 "

These percentages of error are certainly too great to be neglected, and while they are perhaps larger than would be found in most other forms of truss, they show, I think, that Mr. Waddell is mistaken in thinking that his assumption that the trusses are of the simple Pratt type with parallel chords cannot in any way vitiate the results of his investigations, and that we should not be too sure that a uniform load which is equivalent to engine concentrations for one form of truss will be so for any other we may be considering.

It would save some labor to computers if every railroad company would adopt one of a series of standard live loads in designing its bridges, and I see little to criticise in those proposed by Mr. Waddell, except that the weights on the tenders are too small, and there should be an alternative load corresponding to Mr. Cooper's 100 000 pounds on two axles. It is true that this is not needed for long panels, but short ones are necessarily used occasionally, and the specified loads should be such as to provide for them.

What is said about wind pressure is in general in accordance with

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my views, but the overturning moment of the wind pressure on the train should be considered as well as that on the top lateral system. I am not yet prepared to adopt riveted laterals throughout, although they have strong points in their favor. When used between compression members, they must either be made stiff enough to act in compression, or they will be slack when most needed. If they are made stiff, they will be very heavy compared with rods for short spans. I do not agree with Mr. Waddell in thinking that no railway specification begins to cover the ground of proportioning the inclined end posts of through spans, to transfer the wind pressure on the upper lateral system to, the masonry. Many of them say that the combined unit stresses shall not exceed a certain limit for this or any other members of the bridge, and I think that covers the case. It does not seem necessary for the railroad company to tell the contractor how to compute the stresses. The latter is supposed to know that.

I, too, firmly believe that the proper way to proportion the section of a plate girder is to count in the web as aiding in resisting bending moments, but do not think that the rule given for finding the total moment of resistance is sufficiently accurate. The moment of resistance of the web is substantially as given (one-sixth its area multiplied by the depth of the girder), but the actual moment of resistance of the flange angles is less than their area multiplied by the distance between the centers of gravity of the flanges, and in shallow girders the error is considerable.

Let $2\,d$ = depth of girder from back to back of angles,

$2\,h$ = distance between centers of gravity of flanges, and

$2\,y$ = depth of girder over all; then the true moment of resistance of the flange angles is $\frac{\text{area} \times h^2}{y}$ if we neglect the moment of inertia of the angles about their center of gravity. The moment of resistance by Mr. Waddell's method is area $\times\ h$, which is greater than the actual moment. Assume a girder with a web plate 24 inches x $\frac{3}{8}$ inch, and flange angles 6 x 4 inches, weighing 20 pounds per foot. Then $2\,d = 24$ inches, $2\,h = 21.8$ inches, $2\,y = 24$ inches, area of one angle $= 6.0$ inches, and its moment of inertia about the axis through its center of gravity is 7.8. The true moment of resistance is $\frac{4\,(6.0 \times 10.9^2 + 7.8)}{12} + 24 + \frac{3}{8} + \frac{1}{6} + 24 = 276$, and the moment of resistance given by Mr. Waddell's rule is $4 \times 6.0 \times 10.9 + 24 + \frac{3}{8} \times \frac{1}{6} + 21.8 = 294$, which is too large by $6\frac{5}{11}$ per cent. If we neglect the web, the moment of resistance found by multiplying the area of the angles by the distance between their centers of gravity is 262, which is not as much less than the real moment of the whole section as the other is greater. For deeper girders the error is less, but for shallower ones it is greater, and I have found cases where the true moment of resist-

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ance was less than the area of one flange multiplied by the distance between the centers of gravity of the flanges. These shallow girders, it should be remembered, are usually short, and should have an excess rather than a deficiency of strength.

SAMUEL TOBIAS WAGNER, M. Am. Soc. C. E.—Under Group "C," "Styles and Proportions of Bridges," the writer would prefer the following limits for the lengths of spans used in plate and lattice construction, namely: Plate girders up to 80 feet spans, lattice girders from 80 to 125 feet, at which latter point pin connected trusses should begin to be used. The most serious objection to the use of plate girders from 80 to 100 feet, is the necessarily great waste of metal caused by having a continuous web. As true engineering practice aims to produce the best construction for the least money, and as it is perfectly practicable to design a very rigid and unquestionably perfect lattice span of such lengths, with a saving of from 30 to 40 per cent. in weight, over the same length of plate girder span, and with much better and more pleasing general effect; we do not seem justified in making the customer pay the difference unless he has special reasons for doing so. Owing to the uncertainty of stress distribution in riveted connections, riveted spans should not be used except for lengths where pin spans are lacking in rigidity; and as pin spans can be, and are, designed with floor systems rigidly connected to the trusses and thoroughly braced with stiff laterals for spans of 125 feet, the writer would, for spans over this length, pass at once to pin connected Pratt trusses, with panels as long as can be rigidly braced. He thoroughly advocates Mr. Waddell's claim for the single cancellation principle for all trusses as being the only really scientific construction, and much simpler and therefore better in practice.

Regarding adjustable members in bridges, the writer again believes Mr. Waddell to be on the right track. If properly designed for erection purposes, and carefully manufactured, adjustable counters can be made a thing of the past. There can be no question about the lower laterals being rigid, as no one who has ever carefully watched the action of poorly braced floor systems under heavy and sudden loading could long remain in doubt on this point. For long spans, at any rate, the top laterals should be of the same construction.

The writer is very glad to see that Mr. Waddell has advanced his opinion regarding the unnecessary reaming of rivet holes of mild and medium steel which will stand the enlarging of the rivet hole 25 per cent. by means of the drift test successfully; and hopes that more of the members will put themselves on record in the same way. It has always seemed an insult to good metal to require reaming when the quality of the metal is such that no practical benefit is derived from it. The line, however, where the reaming becomes necessary should be carefully observed, and probably Mr. Waddell's requirements meet it as well, if not better, than any other way.

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Now that eye-bars are manufactured of widths up to and including 10 inches, it is advisable to use wider bars for long panels than was formerly the custom, and at the present time this is being largely done. By using a wider bar it is possible also to reduce the number of bars in a panel, the thickness of each individual bar, the bending moment on the pin and consequently its diameter and length. In making bars of the largest sections, special care should be paid to the tests, as special arrangements must be made by the manufacturer to cast special ingots, or use larger piles to insure the requisite amount of work in the rolls upon the metals. Steel bars of the larger sections specially are much more desirable in every way than iron, both in reliability and finish.

WILLIAM CAIN, M. Am. Soc. C. E.—Whilst agreeing with Mr. Waddell in most of the points brought forward in his paper, I must take exception to the co-efficient $\frac{1}{2}$ in his formula for unit stress,

$$\text{Intensity} = \text{constant}\left(1 + \tfrac{1}{2}\frac{\text{minimum stress}}{\text{maximum stress}}\right),$$

though I admit that, in the present state of our experimental knowledge, this co-efficient must be, to a certain extent, a matter of opinion. Still, as it has been assumed all the way from $\frac{1}{2}$ to $\frac{3}{4}$ by certain bridge engineers, it is well that all interested should give the basis, if any, of their assumptions, when possibly the range of permissible values will be restricted within much narrower and more manageable limits than at present. If we call the ratio of minimum stress to maximum stres θ, we can write the above formula of Mr. Waddell's,

$$\text{Unit stress} = \text{constant}\ (1 + \tfrac{1}{2}\theta),$$

which is Launhardt's formula (as given by Weyrauch) for, say, forty million repetitions of stress. For a less number of repetitions, it is found by working over the values given in the original experiments that the co-efficient $\frac{1}{2}$ is decreased, and becomes quite small for a few hundred thousand repetitions, which is nearer the number to be provided for in our bridges than the former. The iron, in these experiments on direct tension, broke under a single application of a load gradually applied ($\theta = 1$) at 45 000 pounds; for millions of applications of a stress of 30 000 pounds per square inch (the stress varying from 0 to 30 000), rapidly repeated, the bar again broke. The above formula becomes for this case, unit stress $= 30\,000\ (1 + \frac{1}{2}\theta)$. For $\theta = 0$ we have 30 000, for $\theta = 1$ we have 45 000, and for intermediate values of θ the formula is found to agree with the results of experiments.

This iron was not such as we would use in bridges, and it is believed for iron whose breaking strength is 50 to 55 000 pounds, that this co-efficient of θ would be smaller for the same number of repetitions. The loads were very rapidly repeated, and the conditions could only be paralleled, in a railroad bridge, by trains gliding rapidly, without appreciable friction or impact, in rapid succession across the structure. It

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is readily granted that these conditions do not obtain in practice, and yet it would seem that we have gained something valuable from the Wöhler experiments, that we should not throw away. We certainly should feel ourselves on safer ground by asserting (for a railroad bridge, the only kind I shall consider), with Wöhler, that repetitions of a load will cause destruction sometimes when the load constantly applied will not, and I think this law holds true even where the loads do not succeed each other rapidly.

Now, what are the conditions for a railroad bridge, when a train headed by a locomotive rolls over it? Professor S. W. Robinson (in *Transactions* of the American Society of Civil Engineers for February, 1887), tells us that as the locomotive enters upon the bridge, and before it has reached the center, the deflection at the panel point nearest the center becomes a maximum, and that a rapid vertical and horizontal oscillation is set up, which continues until the train leaves the bridge. He experimented on spans of 128 to 189 feet, 13 open web bridges being examined, the records of horizontal and vertical deflections at each instant of the train's passage being graphically recorded by an automatic apparatus. It is possible that the increased deflection due to the train in motion over that for the train at rest, was due in part to the unsteady motion, not in a straight line, but deviating laterally and vertically from a straight line every instant; mainly, however, to the centrifugal force of the counter weights on the drivers, which hammer away at the rails at each revolution of the wheels. Professor Robinson found that the increased live load deflection for freight and passenger trains at usual speeds due to the motion, varied all the way from almost nothing to 28 per cent. Out of a hundred and ninety-three train transits observed, twenty-five gave unusually large superadded deflections, averaging 18.4 per cent.

The graphic records show plainly, I think, that the bridge has time to recover from the first "sudden application" of the load as the train advances, so that the superadded deflections due to the train in motion over those for the train at rest, when at the maximum, are due not to the "sudden application," but to impact and vibration from unbalanced weights and rough track. Now, if we call the extra stresses induced by the extra vertical deflection the amount due to impact, we have for these bridges to add to the stress caused by the live load at rest on a chord member, say, 18.4 per cent. to get that due to the impact of the live load in motion, assuming that the stresses in the chords are directly proportional to the deflection. This much is very simple, and if similar experiments were performed, for all spans, from 0 up to 500 +, we could quickly rid ourselves of part of the indeterminates of the problem that confronts us.

Having, however, got this far, shall we assert that there are no more stresses caused in the bridge members than those corresponding to this

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vertical deflection? Surely not, for the horizontal deflection (which was not insignificant) must cause additional stresses in certain members, and besides this, many members may be largely strained from the tremors caused by the bounding and bouncing of the train, whilst other members may be relieved at the same time, so that the result is not shown in the deflection to an appreciable extent. A vibration in a tie rod at right angles to its length (which we know occurs from the rattling often observed as a bridge is crossed), will cause additional stresses. These could only be ascertained by a direct observation of the elongations of a portion of the tie bar as the train passes.

Granting, then, that there are additional stresses to those corresponding to the vertical deflections, and that the repetition of stresses due to the passage of many trains a day will lower the working strength, what shall we allow for these influences? Here is where we can all honestly differ. Not knowing anything better than the Launhardt formula to express these influences, especially as its form commends itself for its simplicity, I shall write, for the safe stress in pounds per square inch, for wrought iron eye-bars in tension—

$$b = 10\,000\,(1 + \tfrac{1}{2}\,\theta) \ldots\ldots\ldots\ldots\ldots\ldots (1)$$

This allows for repetition and stresses caused by tremors not giving any vertical deflection, and assumes that these two influences combined in a bridge member are as hurtful as millions of repetitions rapidly succeeding each other, as in Wöhler's experiments. To use this formula, we must add a certain per cent. of the live load to itself to allow for the superadded deflection due to motion. If we call A = stress in a lower chord member due to the weight of bridge, B = ditto due to live load at rest, and Bp = superadded stress corresponding to the extra deflection due to the train's motion, then the cross-section of the member is given by the formula,

$$\frac{A + B + Bp}{b}.$$

But the same cross-section can be found by dividing the sum of stress due to dead and live loads both at rest, by—

$$a^1 = 8\,000\,(1 + \tfrac{7}{8}\,\theta) \ldots\ldots\ldots\ldots\ldots\ldots (2)$$

$$\therefore \frac{A + B\,(1 + p)}{10\,000\,(1 + \frac{1}{2}\,\theta)} = \frac{A + B}{8\,000\,(1 + \frac{7}{8}\,\theta)}.$$

Thus, for the bridges mentioned above, $Bp = .184\,B$ = average stress due to extra deflections, as shown by the average of twenty-five experiments giving the largest deflections. As θ for these spans is about $\frac{1}{3}$, or $B = 2A$, on substituting these values we see that the equality above nearly exactly holds; in fact, the co-efficient of θ in (2) was formed from this equation very nearly. Formula (2) gives a range of stress for $\theta = 0$ to $\theta = 1$, from 8 000 to 15 000 pounds; but

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as this differs so little from the extremes 7 500 and 15 000, I prefer to write, for simplicity, in place of (2)—

$$a = 7\,500\,(1 + \theta) \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (3)$$

whence,

$$\frac{A + B\,(1 + p)}{10\,000\,(1 + \frac{1}{2}\theta)} = \frac{A + B}{7\,500\,(1 + \theta)} \quad \ldots\ldots\ldots\ldots \quad (4)$$

Subtracting (3) from (1), we have for the decrease in unit stress, due to impact, which causes extra deflection, over the live load at rest,

$$2\,500\,(1 - \theta) \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (5)$$

For all dead load ($\theta = 1$) this becomes 0, and for all live load $\theta = 0$, it reaches its maximum, as should be the case. For intermediate values of θ, this term varies directly with θ, which is certainly the most simple supposition to make. As it is interesting to see what values of p we obtain from (4), for chord members for various spans, I have given them in the table below, for assumed values of θ:

Span in Feet.	θ	p.
20	.10	.30
70	.20	.28
150	.33+	.25
300	.50	.22
450	.66+	.20

This allows a little more for the 150-foot spans than the $.184 = p$ first taken, which is not to be regretted, as on some of the bridges the deflections due to motion were over the average taken. The formula (3) above was deduced for a lower chord member. A similar method applies to upper chord and batter braces, with the modification due to the use of column formulas. For counters and middle ties $\theta = 0$, giving $p = .334$, or slightly over the .25 above for a 150-foot span, which is not to be regretted, as these members should have a slight increase in their section due to their small size and greater lateral vibration. For intermediate ties and posts the unit stress increases pretty regularly from the center to the ends, as it undoubtedly should. The exceptional decrease in unit stresses for counters and main ties is thus a strong point in favor of the formula in place of telling against it.

Nothing has been said so far about the mild steel now used in bridges, but the same reasoning would lead to a similar formula to (3) only with the constant 7 500, and possibly the constant θ slightly increased, if we are to consider at all Weyrauch's formulas corresponding to (1), where for millions of repetitions of stress, the co-efficient of θ for steel, having a breaking strength of 100 000 pounds, was found to be six-fifths in place of the one-half used for iron. As before stated for

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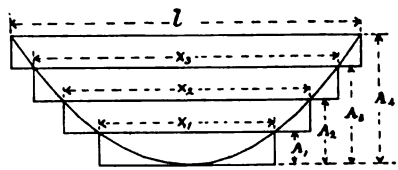
iron, the conditions of the experiments are not those of members in a bridge, but there is of course repetition of stress in a bridge member, and these experiments give us some insight into its effect, and should help us in coming to a somewhat safer conclusion. Finally I should say that if (3) is modified, it should be in the direction indicated by Mr. Waddell, by decreasing somewhat the co-efficient of θ, for reasons apparent from what has preceded.

FRANK C. OSBORN, M. Am. Soc. C. E.—A full discussion of the subject brought up by Mr. Waddell must be productive of much good to designers, builders and users of metal bridges. The railway companies in particular will be the gainers in any movement resulting in a greater uniformity in specifications, and consequent diminished cost of estimating, for while the expense of estimating and designing is borne by the bridge companies directly, it certainly enters as an item of cost and is paid eventually by the railway companies.

The method given in the paper for finding the concentrated load at a floor beam by the use of the equivalent uniform load for a span of two panel lengths can hardly be called new, as it was in use by Mr. Edwin Thacher at least as far back as 1885. Regarding the determination of the lengths of flange plates for plate girders, Mr. Waddell remarks: "The moment parabola referred to affords a very expeditious method of ascertaining the proper lengths of cover plates." From this it seems that Mr. Waddell has not seen the very neat and expeditious method given in Mr. Thacher's pamphlet on the slide rule. This method is based on the equation of the parabola, and is as follows:

Let x_1, x_2 and x_3 represent the lengths of plates required. Let A_1 represent the area of the outer plate, A_2 the area of the outer two plates, and A_3 the area of the three plates. Let l = the length of span, then



$$x_1 = \text{length of outer plate} = l\sqrt{\frac{A_1}{A_4}}$$

$$x_2 = \text{“ “ 2d “} = l\sqrt{\frac{A_2}{A_4}}$$

$$x_3 = \text{“ “ 3d “} = l\sqrt{\frac{A_3}{A_4}}$$

Now, for any given case, A_4 and l are constant, and the values of x_1, x_2, etc., may be obtained by means of the slide rule directly and with a single setting as follows: Set A_4 on slide to l on scale of roots, then opposite A_1, A_2, etc., on slide read x_1, x_2, etc., on scale of roots. These lengths, x_1, x_2, etc., are of course the theoretical lengths for a true parabolic curve. The actual lengths should, as usually specified, be at least 1 foot more than those called for by the above formula.

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The method of obtaining end shear in plate girders is simple and easy of application, and the results certainly close enough for all practical purposes. The comparison of results obtained by means of diagram and by the uniform load is certainly interesting, and a strong argument in favor of the uniform load. The idea of a half dozen or more standard typical engines is an excellent one ; those proposed by Mr. Waddell have the advantage of simplicity and uniformity in spacing, and should be approved by the engineers of the railway companies to the extent of adopting the one nearest their present requirements.

In regard to wind, the assumed pressure, for ordinary spans, of 450 pounds per foot for the loaded chord and 150 pounds per foot for the unloaded one, seems amply sufficient. For long spans, perhaps, this loading should be increased, but why not do it by means of a simple formula which will give the load per foot to be provided for in terms of the length of span ? It takes no small amount of time to figure up the vertical projection of a long span truss, and probably not once in a lifetime will the wind strike the truss in the exact manner assumed. If an equivalent uniform load is admissible as a substitute for an engine diagram in the calculation of the principal members of a truss, and I firmly believe that it is, then a uniform load should certainly be good enough for such an erratic loading as wind pressure. The effects of the assumed loading should, however, be fully provided for.

The proposition to increase the clear width of through bridges to 16 feet for single track and 29 feet for double track structures is an excellent one, and the only objection to such a move, namely, the increase in cost, would be much more than offset by the additional safety secured. I agree with Mr. Waddell in a preference for a rigid lateral system.

The question of proper depths for eye-bars of different panel lengths is an interesting one, and the following table shows the calculated fiber strains due to direct bending for the limiting sizes as proposed by Mr. Waddell:

Unsupported horizontal length of bar.	Minimum depth of bar.	Strain per inch, extreme fiber.
15 feet.	2 inches.	3 400 pounds.
17 "	3 "	2 900 "
20 "	4 "	3 000 "
24 "	5 "	3 450 "
27 "	6 "	3 650 "
30 "	7 "	3 860 "
33 "	8 "	4 100 "
40 "	10 "	4 800 "

When it is considered that these strains should be added to those due to the direct tension, and that the above table represents fairly the present practice, it appears that the question is a serious as well as

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an interesting one. In regard to spacing of stringers, I wish to express myself as decidedly opposed to stringers placed directly under the rail. In the first place, the impact of passing loads is given directly to the stringers and their connections. In the second place, the weight of floor beams is increased on account of the extreme leverage; and thirdly, agreeing with Mr. Waddell, "outer stringers should be used to support the ends of ties in case of derailment," which adds still further to the weight, and therefore to the cost of the structure. The stringers should be at least 8 feet centers, and I think 9 is even better. With cross-ties 8 x 10 inches there is ample strength, and with the 1½ or 2 feet leverage, there is spring enough to the tie to take up a large proportion of the impact which otherwise would all go to the stringer. The short leverage gives a light floor beam, and the wide spacing obviates the necessity of outer stringers, so that on the whole the 8 or 9 feet spacing gives a better as well as a cheaper floor than one with stringers directly under the rails. I agree with the author in the preference for pine instead of oak for floor timber.

The question of just what unit stresses are proper for tension members in metal bridges can hardly be satisfactorily decided until exhaustive practical experiments have been made in this direction. The German experiments with repeated impacts are very interesting and valuable, but they hardly cover the case of bridge members subjected to a constant and sometimes quite large strain per square inch from dead load, and then subjected to an additional live load strain. We should have a number of experiments in the way of delicate measurements on stringers, beams and eye-bars of bridges under engine loads at various velocities and passing at various intervals; and also a series of tests on specimens subjected to a steady strain, together with suddenly applied and released loads; the proportion of steady and sudden load to vary and also the time between the impact load.

For unit strains in compression members my preference is still for the general form of Gordon's formula, and I am inclined to think that the discarding of this formula by some of our most prominent bridge engineers was due to a too hasty conclusion in regard to practical experiments. Mr. Thacher's "straight line" formula does, of course, agree very well with the experiments; from its construction it should. The theory of Gordon's formula, however, assumes a condition of the metal corresponding to strains within the elastic limit, and we know that when a column has been destroyed in a testing machine, the elastic limit of the metal has long been passed. Should we then say that Gordon's formula is wrong because a column fails under a condition of affairs entirely foreign to the basis of the formula? I should not think so, but would rather think it better to make careful and delicate measurements during column tests, before the metal reaches its elastic limit, instead of basing a formula on ultimate failure.

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In regard to plate girder proportioning, I think it would be much better to keep the compression flanges within twenty diameters rather than let them reach thirty. The latter is I think too much. I will take this opportunity to protest against the practice of making the top and bottom flanges of plate girders of different areas, instead of proportioning the tension flange, and then making the compression flange of the same gross area. As plate girders are usually figured, the bending moment is divided by the depth center to center of gravity of flanges, in order to get the flange strain; this flange strain is then divided by the unit strain specified in order to get the desired area. Now, suppose the given specifications would require a larger area for the top flange than for the bottom : the center of gravity of the girder as a whole is thereby raised; and if we then calculate our flange strains by means of the moment of inertia of the section, which is quite proper, we find a larger strain in the bottom flange and a smaller one in the top than required by our given specification.

If different areas in the flanges are desired, then they should be obtained by using the moment of inertia of the section instead of by dividing by the depth of girder, as is ordinarily done. When the web is one continuous sheet, I think it perfectly legitimate to count one-sixth of it as flange area ; when very deep and relatively thin and with frequent splices, I do not think it is advisable to count the web, except for shear.

W. H. Breithaupt, M. Am. Soc. C. E.—The interesting paper under discussion advocates some sound principles of construction which it is to be hoped will be more generally followed than they have been. Whether a uniformity in specifications and design among bridge engineers, much closer than it is, will yet be arrived at, even should close uniformity be desirable, is questionable. Since the beginning of iron bridge building there has been an approximation to uniformity in design, at least among American bridge engineers. Any advance that has sufficient merit is soon generally adopted. So much is this the case that the design of a bridge will generally tell approximately the date of its construction. No department of engineering has experienced greater development during the past fifteen years than this, and with the constant improvement in material and the advance in the governing conditions, it is futile to think that we have arrived at anything like a finality. Individual differences of opinion, too, will no doubt continue to exist even in so exact an art as bridge building has become.

Mr. Waddell makes extended argument in favor of the equivalent uniform load method of calculating stresses. The determination of stresses is a small part of the complete designing of a structure. With a slide rule and a table of squares the stresses are generally soon figured by any of the various methods individually preferred. But for ordinary fixed spans the method of moments some time ago de-

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scribed before the Society ("Cooper on American Railroad Bridges," July, 1889) is not only more exact, as is admitted, but requires considerably less work; I think I am safe in saying that it is the one most generally used now. The making of the single diagram used in the moment method can be done in, say, one-quarter of the time required for getting up the several tables of equivalent uniform loads that would have to be used.

Fig. 1 is a typical moment diagram, with quantities as affecting one truss, made on a scale to be conveniently applied to the skeleton of a truss. The upper two lines are essential, the others are added for convenience. The successive moments on the diagram are quickly obtained by adding to the moment up to any point the product of the total loads up to that point by the distance to the next one. Unequal panel lengths do not affect the facility of application of this method, as separate panel concentrations are not used. Long spans make practically no difference. When the chords are not parallel some extra work is required for obtaining web stresses in locating the intersections for origins of moments. For the case of some of the leading loads being off the span the proper moments are obtained, again from the diagram, by two simple

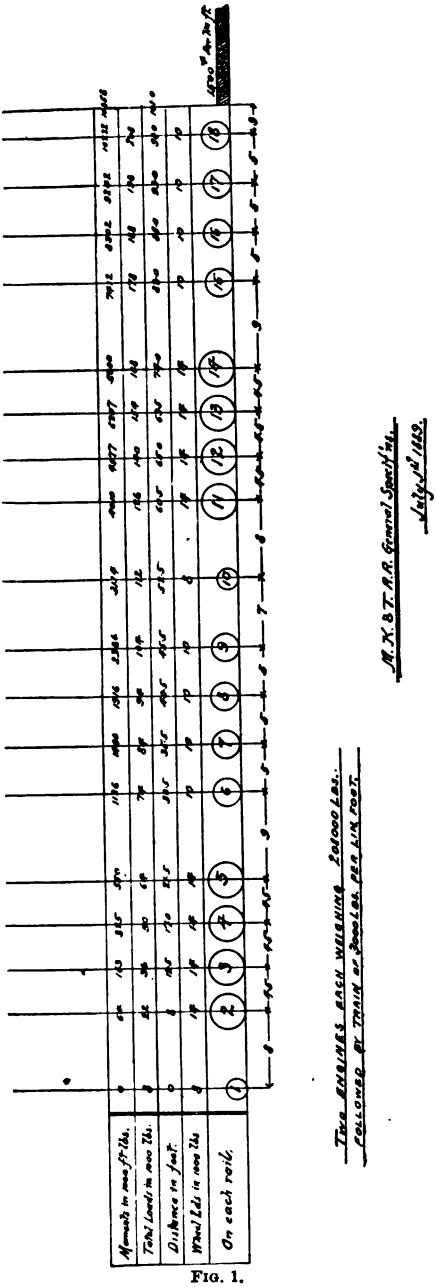

FIG. 1.

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subtractions. The moving load railways can carry is not now so much a question of bridges as of track, which latter seems to be about taxed to its limit by the present heaviest moving loads, until radical changes are made. Regulation of loads must be considered as outside of the province of bridge engineers. What they can do, as has been stated, is to design for proper proportion, which may be assumed to remain fairly uniform, of excess of concentrated load to general load, so that bridges may remain uniformly strong throughout. As to wind pressure, the assumption of fixed loads per running foot, as often specified, should not be applied to spans of over 200 feet in length, taking for longer spans a pressure per square foot of surface, and considering effective surfaces by the method as given in the paper, which is a good one. The more rational way of considering the overturning effect is that it travels partly by top chords through the portal and partly directly to the bottom chords, and provide for this overturning effect in the bottom chords by means of a large unit strain.

Riveted pony trusses have their proper place, where deep floors are admissible, for spans of from 100 to 125 feet. It is true that with shallow floor beams with chord brace to bracket extensions on beams, the deflection of the beams may injuriously vibrate top chords; this condition should rule out this class of bridge. When it can properly be used, a pony truss with its heavier sections and greater compactness will be more rigid and as economical as a high overhead braced span of the same length would be. Reduction in cost of material and consequent proportionally greater saving if shop work is reduced, as well as considerations of efficiency, have strongly tended to simplification of design. One manifestation of this is in the use of the present long panels. The use of a few bars of large section instead of more of smaller section is for several reasons desirable; but for prevention of sagging in the bottom chord bars, increase of section is not required and is not as effective against either sagging or vibration as are mid-panel brackets now used for supporting chord bars. Such brackets are an extension of transverse brace frames between the stringers.

The author gives a bridge floor which is in many respects a good one. Oak is more generally used for ties and guard rail than pine, but good pine is to be preferred. For guard rails pine is particularly preferable, as it is almost impossible to keep an oak guard rail straight. Oak and pine should not be used together, as the acids from the oak will destroy the pine. Preserved woods are strongly to be recommended for bridge floors. For the best floor the stringers should be directly, or almost so, under the rails, as otherwise the ties will deflect. Then there should be outer safety stringers under or directly outside of guard rails. The inner guard rail should be a railway rail, which may be of lighter section than the regular rail used. The dapping of the guard rail is depended on for preventing

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bunching of ties; a bolt to every third tie will properly hold down the guard rail. Some form of nut lock is essential for all bolts in the floor or elsewhere about a bridge, as without it nuts will invariably work loose. This is something that is frequently neglected.

Under intensities of working stresses the author gives the well known Launhardt formula. The formula is not much used now in this shape; but its principle is, in a more readily applicable way, by using greater unit strains for live load than for dead load.

To use end floor beams is the proper thing to do. Besides the advantages in their favor mentioned by the author, there is this, that they obviate injurious hammering of the masonry, which takes place when end stringers rest directly on it. It often occurs that floor beams have to be reduced in depth at their ends to fit them to the posts or to clear the bottom chords. Efficient reinforcement to effect this, when reinforcement is required, can generally be accomplished much more economically than by going to the extreme of running an intermediate flange the entire length of the beam.

The author's remarks on eccentricity of connections and on induced bending moments by certain groupings of rivets are strongly to the point. It is to be regretted that he has not said something on the disputed question of impact. The effect of impact depends on the speed of the moving load and on change in direction of surface from the ground to a bridge and on a bridge; but how much should properly be allowed for this effect is as yet a matter of conjecture only. It is a minimum when the floor system is stiff and rigid and deflects very little under moving loads; with a surface which changes direction only very gradually if at all, and is, as near as can be, a straight continuation of the ground surface. In painting, another subject not mentioned in the paper, there is to be said that the use of oxide of iron paints, now so general, is much to be deprecated. The pigment in these paints is simply rust, which on iron or steel tends to induce more rust. Lead oxides, or carbonates, such as red lead and white lead, and linseed oil, should be used.

FRANK W. SKINNER, M. Am. Soc. C. E.—Without entering upon the mathematical deductions of this very comprehensive and valuable paper, I wish to discuss some principles and practical considerations.

Live Loads.—I believe in the use of engine excesses and real train values for the computation of live load strains; these should be assumed for present or future conditions of traffic, and those data selected which will give the maximum; so that different loads as well as different positions of the same loads may be required to give maximum results for different parts of the structure, each of which should be proportioned for its maximum. This leaves no uncertainty; the definitely stated problem is accurately solved. By the equivalent distribution system only one of many possible conditions is assumed, and an approximate equivalent is substituted for that.

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A prime function of any constructing engineer is to make the most careful and accurate assumptions, founded on skill and experience, and then prepare exact computations therefrom. Exact computations are complained of as laborious—so is most valuable engineering work; but the mathematician is only a small part of the engineer; and the bridge concerns that compute and build four-fifths of the railway bridges, have special computers who are continually employed solely on strain sheets. Economy should be practiced in restricting the useless multiplication of designs and estimates by unsuccessful bidding on the same structure, by inviting bids from only a limited number of select, responsible parties.

Wind Pressure.—This is usually considered as dead and live load; should it not also be figured for the impact that it undeniably exerts through sudden gusts?

Proportions of Bridges.—Under certain conditions the inclination from the vertical, of the planes of the main trusses of long spans, so that their cross-section is somewhat like a truncated inverted V, is worthy of consideration as affording economy of upper lateral and floor systems and increasing the rigidity. Mr. Waddell's plea for properly designed floor systems, protection and re-railing apparatus, cannot be too strongly endorsed. I believe the tendency is toward the use of too heavy eye-bar bars. When a bar exceeds 10 inches x 2 inches x 40 feet, it is very doubtful if the steel of which it is composed receives sufficient work in its reduction from the comparatively small ingots generally used; and, so far as I know, cross-sections of that size never receive any edge rolling after leaving the blooming rolls, as I believe they certainly need.

Intensities of Working Strains.—I think that the author has overlooked the results, occasionally reported, of tests upon bridge members after having been a long time in use, and I should be glad if railroad bridge engineers could furnish more of them as their bridges are renewed. Such tests would be of especial practical value if they could be accompanied by the length of service and the original tests of the same material when manufactured. I would like to know what provision is allowed, and what, if any, experiments or observations have been made on the effect of impact strains added to existing heavy static strains. It does not accord with our theories of steel to suppose that tension members built of plates and shapes, are as strong per net square inch of cross-section as are eye-bars; because shapes and wide plates are not expected or generally required to give as high unit strength in small test pieces as flats and rods do. They cannot have their strength increased by reworking, and if injured at all by punching, shearing, etc., they cannot be restored, as are eye-bars, by annealing.

Combined Stresses.—I should like to see the proof that the addition referred to of 17 square inches of reinforcement to the section of a bridge post could increase the working strains so enormously as 18 per cent.

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Plate Girder Proportioning.—I endorse the limitation of the number of flange plates allowed, and think that part of the chord section may often be advantageously disposed in web plates.

It is doubtful if it is desirable to have the best machine driven rivets 6 or 8 inches long between heads, and it is certain that a hand driven rivet of that length is very imperfect. It is also evident that rivet holes which would match pretty well for two or three plates, would require much more reaming, and considerably weaken the section when carried through a high pile of plates. Inspectors will sometimes condemn machine driven rivets for slight excentricities of head, etc., when they can be replaced only by hand driven rivets that are incomparably worse; to say nothing of the injury done to the plates (especially steel ones), in forcing out a large, long, well upset rivet.

I shall be glad to see the detail that Mr. Waddell describes as having practically and satisfactorily accomplished the focusing of all lines of strain, at an absolute point at the center of a main truss lower chord connection. I do not wish to advocate the general use of suspended floor beams, but I think there may be conditions where a well built suspended floor beam is preferable to one with a poorly riveted and poorly arranged rigid post connection. A prominent bridge company with which I was once connected imported and used soft steel almost entirely at that time. Much of the steel was Belgian shapes, and endured wonderfully severe cold bending tests. Angles and channels could be bent backward and almost tied up without cracking. Experiments were made with the use of these channels (heavy webbed 6-inch bars, as I remember) for floor beam suspenders. The channel bar was bent at the middle completely around a pin (slightly smaller than the required one). The ends were then brought down parallel, back to back, about three-eighths inch apart, so that their webs would rivet vertically across the end of the web of the floor beam, and the loop above, properly reamed out, would receive the lower chord pin. I do not know what the result of the experiments was, for I did not see them concluded, but I believe that the channels endured the treatment and made pretty stiff suspenders.

A. J. SWIFT, M. Am. Soc. C. E.—Mr. Waddell's paper is certainly an excellent one in its scope, covering as it does many points upon which opinions differ among bridge designers and which are often discussed; but never before, it is thought, assembled in such form as to give the professional public a chance to express its views upon so many debated points all together, and in a form sure to meet the eye of the profession at large.

As one who has experienced the troubles noted by Mr. Waddell in using typical train and engine loads with exact wheel concentrations, I can heartily endorse what is urged by him as to the need for the use of a simpler form of load. I would suggest, however,

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as more consistent, in being nearer the facts of the case than a uniform load several times greater than the heaviest train in use, the often adopted expedient of a single concentration of the engine excess sufficient to equal a panel load (floor beam shear), arising from an ideal engine of sufficient weight to discount a considerable increase in gross weight of the heaviest type of engine to be used. This single concentration to be located for strains in web members, at the head of a uniform load, equivalent to a train similarly in excess of the heaviest train actually in use, and located, for chord strains, at any point in such a uniform train load. Also a single concentrated weight still greater than that above noted for calculation of strains in stringers; this latter weight to vary with the lengths of panels, and its amount to be based also upon the heaviest existing engine, with due allowance for future increase. For floor beams, it is thought that the well known proportions between stringer shear and floor beam shear would suffice to insure sufficiently exact results. This, it is submitted, would seem to meet the difficulties as to complexity of calculation without going too far backward toward the practice of former days of using uniform loads only, and the consequent loss of view of the true form of the problem in hand. It would leave the general problem in its true form, without introducing annoying intricacies in calculation.

To any one who can recall the changes in opinion as to curved or other than parallel chords, floor beam connections, compression formulas and curves, the effects of live loads as compared with dead loads, etc., which have been noted within the past twenty years, and the diversity of view which now exists upon such points, Mr. Waddell's claim that there is such a thing as unnecessary refinement in concentration certainly recommends itself; and the fact that such points as the effect of wind in throwing excessive load from a train upon leeward stringers and floor beam connections has been neglected, while the effects arising from the same cause in towers and lateral systems has been carefully provided for, may also be noticed as warranting, for the sake of consistency, a less minute amount of care as to points like this, of concentration of rolling loads.

It is thought that no one will contest the truth of the ground taken by Mr. Waddell as to the advantage of using plate girders up to 90 feet span, although it seems apparently a step backward toward the days of tubular bridges; but it is suggested that some distinction should be made as to the different allowable spans of plate girders for double and single tracks, such as he has made, in regard to the length of lattice trusses. He has drawn the line in this latter connection, it is thought, at exactly the right point, but has not stated his reasons for so doing. I may be permitted to say, that a good reason for endorsing his views is, that the result of experience gained in keeping in repair a large number of riveted bridges for many years, has been

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to show that the thickness of the iron used and riveted is really the controlling feature. This experience proves that well and heavily designed bridges of the lattice type and of recent date, in which connections of diagonals with chords through several inches of metal have been made by means of 1-inch rivets, have required greater care and more frequent renewal than those in which the rivets have passed through thinner metal; although the rivets in the latter have been under much greater shearing and bearing strain for many years. It would seem that the vibration of a web member in tension composed of heavy angle bars, has seemed to fairly pry off rivet heads and loosen them, and that this really constitutes the limiting feature in such designs.

This being the case, it would seem that 70 feet for a double track plate girder span of either two or three girders is about the useful limit, and 90 feet for single track girders, with riveted lattice trusses (rather than the limits noted by Mr. Waddell), and pin connections for longer spans. The views expressed in this paper as to avoiding all pony trusses certainly seem sound, but an exception can be made, it is thought, in the case where floors can be made so deep as to give to the top chord the support of a stiff connection with the floor beams, whose top flanges are not more than 2 or 3 feet below them. This limit of the length of riveted lattice spans on account of the failure of riveting through thick iron at the connection of diagonals with cross chords, seems to conflict with and render invalid Mr. Waddell's strictures upon lattice work of more than one system; since it often occurs that, in the design of lattice spans even of the length endorsed by him, two or more systems (up to four) have to be introduced, in order to keep down this objectionable thickness at these points of connection. Furthermore, it is submitted that practically no trouble was ever found to result from the use of several systems, while certainly cases are on record, as witnessed by numerous photographs, where serious accidents have been unscientifically prevented, by the aid given by one system to another disabled by a derailed train. This feature would seem to be the one really giving the lattice bridges the superiority noted by Mr. Waddell as existing over pin bridges in case of derailment. This increased safety is obtainable for pin bridges by enlarging the clear width between trusses so as to remove them beyond the reach of impact from a derailed train; it would therefore seem a pity to deprive the riveted bridges of this peculiar and great element of value.

The riveted lattice truss was the first step toward concentrating upon known lines the strains of unknown location and direction existing in webs of plate girders, and it does not seem clear that it is the true drift of bridge engineering now to try to force it (the lattice truss) to comply with the form found desirable in its offspring with pin connections. Furthermore, secondary strains at connections are lessened

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by keeping the members within reasonable limits of size, rather than increasing them as would be necessary if the form of truss were limited to the Warren girder. Rigidity also is of great value in preserving tie rivets in the chord connections of riveted work, much more so it is submitted, than with pin connections, and therefore shorter panels and consequently more systems seem justifiable. Again, one serious theoretical defect in lattice bridges, is that the shear in passing from one diagonal to the next is necessarily carried by the chord as transverse strain, and this objectionable feature is certainly diminished by lessening the amount of this shear so carried at each section of the chord at panel points, this result being reached by the use of several, rather than one system. It is submitted that such views as those expressed by Mr. Waddell, and adopted in many lattice trusses, have resulted in structures which cannot be classified as "fish, flesh or fowl," the result being unfair criticism upon the excellent class of work of which these structures are not fair types; and that their manifest shortcomings almost warrant belief that, with few exceptions, designers accustomed to one form of bridge cannot properly design the other.

Exception, it is thought, may also be taken to Mr. Waddell's statement that plate girders should be riveted in full at the shops and not shipped in parts to facilitate erection. The difficulty in erecting whole a girder of 90-feet span, even for a single track, is very considerable, and it has not been found that field riveted work properly done is so inferior to that done at shops as to be inadvisable, if a sufficient excess in number of field rivets is allowed; while the danger of accident to large completed girders during transportation and erection is serious, resulting as it does from lifting them by improper points of support during transhipment and also at their destination.

As regards spacing track stringers or girders of short span, it is suggested that the points to be gained are a uniform support to a derailed train inside of the tie bearings on stringers, without the use of ties of unnecessary dimensions and consequently unnecessary expense; and that a distance of 7 feet between centers of stringers with ties 8 x 8 inches x 10 feet, accomplishes this result, and that, too, without going further than necessary in the direction of a return to timber floors resting upon stringers hung in the line of the chords, such as were more or less in vogue in the past. The additional expense for one renewal of ties upon bridges under my charge which would result from adopting Mr. Waddell's standards, would be $33 000, a serious item if not really essential to safety.

In answer to Mr. Waddell's inquiry, I would say that a number of cases of split angles in top flanges of stringers and girders not properly supported by fitted stiffening angles, have been noted in work under my charge, and that on this account a proper provision of this kind seems very advisable at points throughout their length as

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well as at their ends ; but it is suggested that their service in preventing buckling of webs over bearings and elsewhere would be as efficiently performed without this bearing upon flange angles, since the load which this fitting enables them to take is only that from a local application over them, while the instant before the wheel reaches this point, the web has to carry almost the same shear, and to resist almost the same buckling tendency. It may be allowable to add, that during an experience covering a number of years, every case of failure noticed in iron bridges has been in the tension flanges of track stringers; which have been found from errors in workmanship or in original design to have been strained to 15 000 pounds and more per square inch of net section at the point of fracture; while truss members have been found in the older class of bridges which have been strained not less than 22 000 pounds per square inch hourly for years, and still have shown no sign of failure or deterioration when removed. This illustrates in a rough way, it is thought, the injurious effect (now so well recognized) of direct application of strain as compared with its more gradual application.

Numerous instances have been found, upon the other hand, when before reinforcement, top chords of old deck lattice spans of sections shown have supported ties directly, with a span between panel points of from 10 to 12 feet, and have shown no signs of failure after twenty years' service, although they were practically stringers with no bottom flanges whatever, and also strained as truss members.

This fact is noticed merely as an illustration of the extremity which bad designing has reached in the past, and, it is feared, sometimes reaches even now; and to emphasize the fact noted above, that theories seem to fail in view of the known results of such cases of overstrain, and that therefore it scarcely seems necessary to carry the refinement of concentration of rolling loads and their proper location upon bridges to obtain maxima strains, to the extent which has been customary and required in recent years.

Mr. Waddell's views as to rigid lateral systems and the advantages of unadjustable truss members have, of course, always been held by designers of riveted lattice work; and it has often been urged with justice, that a considerable economy of material results from these rigid lateral systems, since both Warren girders of a double lateral system of this kind can be assumed to be under strain by wind load in one direction—one of each pair of intersecting members acting in tension and the other in compression.

His views as to spacing ties and the use of guard timbers have certainly the support of innumerable cases of safely carried derailed trains; but from the point of view of one wishing to incur no expense

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not demonstrated to be really necessary, I would suggest that a clear space of 6 inches between ties and a guard rail 6 x 8 inches, gained 1 inch and bolted to every third tie, seems from actual experience to provide amply against "bunching" under derailed wheels. It may be added that any spreading of guards at the ends of a bridge greater than the 8 or 9 feet of length of track ties is useless, since a truck off of the track and beyond the ends of the track ties would certainly sink in the ballast and overturn its car or break the couplings, so that no benefit would result from a further spread of the ends of the guards. Experience in maintaining iron bridges under heavy traffic fully justifies, it is thought, the use of end floor beams, since the direct impact of wheels upon the ends of stringers frequently shatters stone bearings or causes the bed plates of stringers to cut into them, resulting in bad surface of track, and necessitating shimming under ties at the ends of bridges, which is certainly a most objectionable feature in their maintenance.

O. F. Nichols, M. Am. Soc. C. E.—Undoubtedly much more can be accomplished toward uniformity in bridge designing by concerted action among engineers, more, I believe, through the report of a committee of the Society than through a general discussion like the one proposed by Mr. Waddell, which is bound to treat the subject in an unsystematic and necessarily rambling way. I do not mean that the report of such a committee should be made fast and binding, a mandate of the Society for all to follow; it would merely cover in the end the recommendations of the ablest of our engineers, members of the committee, and have such weight as their reputations and investigations would warrant, the Society acting as the agent for whom such investigations were made and by whom such recommendations were developed and announced.

It would certainly be well if some uniform system of loading could be agreed upon, simple enough to involve less loss of time in estimating on bridge work, a loss which is absolute to the unsuccessful bidder. The employment of equivalent uniform loads will probably help the matter somewhat; there are so many problematical contingencies in our railroad loadings and the chances are so greatly in favor of increased loadings, that it is well not to indulge in over-refinement in calculations, but to assume some gross figure sure to provide amply for the present and somewhat for the future, the error, if any, being on the side of safety; if the cost is somewhat greater it should be considered a wise investment. The extent to which material is forced out of some of our structures by refinements in calculations, leaves them often without that mere mass of material which we now, more than ever, realize is necessary to the staying qualities required to resist impact, continuous pounding, alternation of stresses and numbers of incidentals to actual use which are difficult to estimate accurately.

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I think it unwise to use metal less than three-eighths inch thick anywhere, on account of oxidation for one thing; our paints and painting are not improving as time goes on, and decay from corrosion is certain. When it comes to using five-sixteenths or one-fourth inch web and cover plates for girders, I believe the characterization "tin structure" is not too strong; thin cover plates are frequently distorted by oxidation alone. I think cover plates should be avoided wherever possible, and limited to one or two thicknesses of considerable dimensions wherever practicable; their action with the flange angles or channels is uncertain, largely from differences in elongation under stress due to manufacture, and they are by no means the protection against corrosion often supposed.

We, of course, know really very little of the actual behavior of web plates in practice, the most of the rules we have been using depend largely on Gordon's formula for columns and give results largely in excess of what we know to be required. It is unfortunate that extended and skillful experiments on full sized pieces cannot be made. Some girders of good size were recently broken abroad to determine relative values of different materials, and no special attention seems to have been made of the action of the webs under stress. The translator says of one of the steel girders, "the web was bent out of its vertical plane," but how did it bend, did it buckle, and how much? In all probability it bent sideways with the failure of the girder, when the web came to sustain all the stresses in the girder. We do know that web plates will endure greater stresses than we have heretofore supposed. Mr. Cooper has recently sanctioned some quite bold practice in omitting stiffeners altogether from plate girders up to 4½ feet deep, and Mr. Thatcher in the "Buffalo Trace" viaduct used girders 60 feet long and 5 feet deep without stiffeners, the web plates being one-half inch thick. The counter stresses of tension and compression, at right angles to each other in the web, operate more forcibly than we have supposed to keep the plate in vertical plane. The common allowance of 4 000 pounds per square inch for shear, and indeed all our allowances for shear, seem quite conservative as compared with allowances for other stresses.

The common rules for the use of stiffeners seem chaotic indeed; it is quite clear that they should be less than the depth of the girders apart at the ends of girders, otherwise the web, virtually unstiffened, sustains practically all the shear for a length equal to the depth of the girder at these points, and if it will do it for this length it will do it for a greater length; a reasoning which logically continued omits them altogether. If stiffeners are used I think they should be treated as posts and the web be considered as a tension diagonal only. In plate girder design we confuse the condition in which the flange needs the stiffeners, as in supporting a continuous wall of masonry with that of the railroad bridge where the tie is supported on an edge equal to the thickness of the web and the vertical legs of the flange angles.

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An extravagant use of stiffeners has been introduced by some of the iron manufacturers; starting at the ends of girders with backs of angles facing from the ends, when the middle is reached instead of omitting them entirely as theory would dictate they double the stiffeners at this point, bringing the angles back to back and riveting them together, a keen sense of symmetry not permitting an otherwise possible saving of material.

I can see no good reason why we should hesitate in these days of perfect plates to increase the thickness of web to a reasonable extent. With flange angles one-half to three-fourths inch thick, why not have the webs of the same thickness if necessary, when (considering labor and material) we do not thereby materially increase the cost of the girders? The result would be stiffer and more permanent girders. Thin plates are bound to corrode to a dangerous extent long before the flange angles are materially weakened, unless they also are absurdly thin.

The stringer should be riveted in between the floor beams, wherever practicable, thus holding the latter vertical and avoiding a twisting in the floor beams, sometimes induced when steps near the bottom of the floor beams are used without such riveting. Provision for expansion can be easily introduced. I am glad to know that Mr. Waddell has abandoned views he once held as to placing stringers directly under the rail. Ties will not last as long under the combined decay of spike holes and support as girders at the same point. Suitable lateral bracing, which I prefer to place at intervals less than his minimum, and to which the floor system lends considerable aid, will prevent departure from a vertical plane in the stringers. The elasticity of timber should be utilized by wider spacing of stringers, to lessen the effect of impact and to avoid rigidity of track.

The floor system proposed seems a good one for bridges; the bolting errs, if at all, on the side of safety. I wish the spacing of the guard rail had been given, as this seems to be a point on which there is considerable difference of opinion. There should be room for the wheels to run between the steel rail and the wooden guard rail; and if inside and outside rails are both used, I think they should be spaced at different distances from the steel rail, the inside guard rail being nearest to the steel rail. I hope these points and the sizes of guard rails will be thoroughly treated in this discussion.

HENRY B. SEAMAN, M. Am. Soc. C. E.—At the Seabright Convention, 1891, a resolution was offered, proposing the appointment of a committee on bridge specifications, but it was withdrawn on account of the strong opposition manifested. This appeared to arise from a misconception of the duties of such a committee, and of the purpose for which it would be appointed. The work proposed was not merely that of formulating a specification, and making our *Transactions* "books of ready reference," but rather that of investigation, and

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accomplishing for this branch of the profession, what had already be done abroad by organizations similar to our own. In outlining t present paper, its author was, no doubt, inspired by the same motiv of progress; and while we contribute what we may toward the gener discussion, we can but remember that any success here attained is b an indication of the more complete results to be attained by an autho ized committee. The preliminary report of such a committee wou take the subject up where the present paper must lay it down. T final report would be of incalculable value to the entire profession.

The subject of engine concentrations, as mentioned in the pap has long been a vexatious one to calculators. The methods and resu of exact calculations are attractive to those who have time and incli tion to follow them through; but to others, who look upon this subj from a purely business standpoint rather than from a professional one shorter method is much to be desired. If railroads were able to adop fixed type of engine, and if it were possible for them to restrict the selves to the use of that type for all future time, the adoption of ex methods of computation, in new construction, would be justifi but when we consider that all successful railroads are progressive that increased business requires increased weight of rolling stock, a that each new type of engine brings with it a new distribution of co centrations—the attempt to attain refined results for any particu type is a misconception of judicious design. If, then, we can attain equivalent uniform load, which shall give approximately the desir results, its adoption s to be commended.

The illusion of the apparent refinement of engine distribution new structures, may be illustrated by the engines provided for in so general specifications. In these, the wheel spacing is given to inch and the concentrations, in some instances, to 100 pounds. This refi ment of weight will not hold true of different engines of the same ty and is worse than useless, as applied to various roads with various ty of engines. When in addition to this, the weights of tender are given as often furnished by the manufacturer—with water, but with no co it shows that such refinement is more apparent than real.

A tender, when fully loaded with water and fuel, will have appro mately equal weights upon each axle; but if the coal is neglected, bei placed mostly over the forward wheels, these concentrations will dif by several thousand pounds. This is the condition, however, und which the weights are often furnished from the shops, and in seve instances they have been introduced, without change, into bridge spe fications. A tender, with water, designed for heavy consolidati engines, weighs about 18 000 pounds on each of the forward axl and about 20 000 pounds on each of the rear axles. Add to th 16 000 pounds of coal with which these tenders may be loaded boarding up the sides, as is often done, and we have 23 000 pounds

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each of the four axles. The tenders of the proposed typical engines would therefore appear considerably too light, particularly for the heavier classes. The weights on the pony wheels of the engines, on the other hand, appear excessively heavy, and would probably not exceed 16 000 pounds for the heaviest type.

In assigning the maximum concentrations to which bridges may be subjected in the future, we must consider the present conditions which are apt to govern them. The maximum concentration in use to-day is 40 000 pounds. This has been the case for a number of years on heavy passenger engines, and the roadbeds on several of the most important lines have already been modified to meet it. It also appears to be the heaviest concentration in use abroad, and it is not liable to be much exceeded with the present gauge. This, then, is the natural limit for the concentrations of consolidation engines, and it would seem better in outlining a set of typical engines, to have this assumed as a maximum, and deduce the others by uniform decrements if desired.

The practicability of outlining a set of typical engines, as proposed, and formulating tables of equivalent uniform loads, varying with the different lengths of span, can only be proven by experience. It is doubtful whether they will prove attractive to maintaining engineers, for insertion in their specifications. These specifications are already growing very cumbersome, and to insert tables would only make them appear more so. It would probably be more practicable to insert one of these types, leaving the tables for use if desired. The most practical substitute for engine concentrations, for insertion in specifications, is the uniform load, with single concentration as proposed by Mr. Geo. H. Pegram, M. Am. Soc. C. E., several years ago. The reason that this has not already been widely adopted, is because of the difficulty in ascertaining the equivalent uniform load and concentration, to correspond to a given type of engine.

The wheel spacing of the several engines, proposed by Mr. Waddell, appears to be well chosen; though the spacing between the drivers is usually 6 inches less than here shown. In advocating the adoption of equivalent uniform loads for bridge construction, it is not necessary, nor is it advisable, to abandon the system of engine concentrations in maintenance. Having a generally well proportioned bridge to maintain, it is the province of the engineer to ascertain as nearly as possible the actual strains to which it is subjected by the engines in use; and although our ignorance of the effect of impact prevents an exact knowledge of the results, it is always well to define the unknown factors as closely as possible, in order that experiment may gradually reduce these to a minimum.

The assumption that "in the near future, the material for the metal portions of railway bridges will be exclusively steel," seems to me a little premature. The objections to its use have not yet been removed;

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and although the development of its manufacture has enabled its production with as much uniformity and economy as that of wrought iron, it still has the same objectionable structure. The objection to the use of steel in railway bridges is, that it resembles a refined, ductile, homogeneous cast iron, rather than a fibrous wrought iron; and although recent practice has gradually reduced the carbon until its composition approaches more nearly that of wrought iron, it has never yet been given a fibrous structure. It is to this fact that the present objection to its use is due.

Steel is objectionable because the least scratch, so fine as to be imperceptible to the closest inspection, may in time, under vibratory strains, lead to the destruction of the member. No inspection can be close enough to avoid this danger, and the reduction of the hardening elements does not eliminate it. This characteristic is due to its homogeneous structure rather than to its chemical composition.

Tests of steel under constant strain are of no value whatever as a criterion for its use in railway bridges. The development of a scratch under a single application of load would not be noticed. The chief care in the maintenance of iron bridges, and the one to which the closest inspection is directed, is the detection of flaws in details. A member rarely fails because of overstrained material. There are members in bridges which have been in use for years, with strains far exceeding those usually allowed, and which are safe; but there are details which, to all outward appearance, have been perfectly sound, but have suddenly developed flaws which, had they not been detected before the passage of a train, would have resulted in a bridge failure. It is for this reason that engineers hesitate to adopt a material the character of which favors the development of such flaws.

The general use of steel would seem objectionable for the reasons mentioned, but to cite the drifting test as an authority for its use without reaming, would seem worse than to depend upon the single application of a load to show the development of slight flaws. "The standard drifting test" is of value only in discovering hard spots in the immediate vicinity of the hole, but cannot detect cracks or scratches caused by punching, the presence of which render reaming necessary. Enlarging the holes by drifting, forces the metal back upon itself, and tends to close rather than to open these defects. It may indicate soft material, as would also a tension test, but as a demonstration that soft steel does not require reaming, the drifting test is a delusion.

The use of steel rails is sometimes cited as an argument for the adoption of this material in railroad bridges. When bridges receive the same incessant inspection in all their details, or when bridge members can be replaced as quickly and with as little expense as can a broken rail or splice bar, and when the failure of a bridge is attended with nothing more serious than a derailment—then may the general

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use of steel be advocated for railway bridges; but until this is true it is well to proceed with caution.

That the specification of 150 pounds per linear foot of chord for wind pressure, irrespective of the length of span or surface exposed, should prove unsatisfactory, is not surprising. The specification is not scientific, and like all empirical clauses is applicable only between limits. It should never be used in a general specification. The early specifications, provided for a wind pressure of 30 pounds per square foot for truss bridges, and 50 pounds per square foot for unloaded trestles. More recently the latter pressure has also been applied to unloaded truss spans. It should not be expected that a review of older bridges would show them proportioned for the heavier pressure. The fact that the exposed surfaces of truss bridges of moderate span approximate to 10 square feet per linear foot of bridge, renders the empirical wind specification convenient of application, but it should not be mistaken for a general clause.

The adoption of the right line formula for compression members, instead of the Gordon formula, as modified by Rankine, has become quite an engineering fad. It is no more accurate, and with the use of tables, now universally adopted, is no easier of application; but the profession has apparently become restless for want of a better formula, and, for a change, adopts an empirical one. The chief labor in proportioning columns, is in the calculation of the moments of inertia, (unless these, too, are tabulated), and from this r^2 as used by Rankine is more directly deduced, than is the value of r used in the right line formula; and while the latter formula is purely empirical, that of Rankine is of thoroughly rational origin. The plea that the constants of the Rankine formula are liable to be varied, and thus render the use of tables difficult, hardly justifies the adoption of a formula wherein even a greater variety of constants had been used.

The Rankine formulas, which are most recent, and now in very general use, are :

$$\text{Fixed ends} \ldots\ldots\ldots P = \frac{a}{1 + \frac{l^2}{36\,000\, r^2}}$$

$$\text{One end fixed} \ldots\ldots P = \frac{a}{1 + \frac{l^2}{24\,000\, r^2}}$$

$$\text{Hinged ends} \ldots\ldots P = \frac{a}{1 + \frac{l^2}{18\,000\, r^2}}$$

and for those who have no tables of their own, very convenient ones can be found on page 147 of "Carnegie's Pocket Book," edition of 1889, or later.

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It is noticed that Mr. Waddell suggests the use of the Launhardt formula for truss members, but for girder spans and for details, proposes constant allowable strains. If the Launhardt formula—and the principle of the fatigue of metals on which it is based—is true of truss members, it is even more true of the floor system and of details, and its omission in the one case would justify its omission altogether. The chief objection to the Launhardt formula is the amount of extra labor involved in its application, and it is probably for this reason that the proposition is made to restrict its use to the general parts of the structure. Another objection is, that it implies a definite knowledge of fatigue which we do not possess. In spite of these objections, however, the adoption of this formula was a marked improvement in the scientific practice of bridge construction, and it has caused modifications in most of the earlier methods of proportioning. It will probably remain in use until some equally rational, though less laborious method is devised. An examination of Wöhler's experiments indicates that it is quite as consistent with their results to consider the effect of live load as about twice as injurious, under continuously repeated strains, as that of dead load. Under these circumstances the dimensioning would be much simplified by the use of different unit strains for live and for dead loads, with proper provision for impact. To this end, Mr. C. C. Schneider, M. Am. Soc. C. E., has already framed a specification, in which he provides for the fatigue under live load strains by a constant in his formula for impact. In this respect his specification is probably the most advanced of any yet in general use.

J. C. Bland, M. Am. Soc. C. E.—I heartily agree with Mr. Waddell as to the desirability of replacing the wheel load concentrations now so universally used in bridge specifications by another method. One, in my judgment, equally correct in results, is the use of a uniform load per linear foot of track, plus a concentrated weight. Such a method is, I think, easier of application than an equivalent uniformly distributed load as recommended by Mr. Waddell. The method I name is by no means new, for as long ago as 1875 I calculated a number of spans to the following specifications. "The strains on the trusses will be computed for the following loads. There will be assumed a moving load of 3 200 pounds per foot of track, on spans from 50 to 100 feet; 3 000 pounds on spans from 100 to 150 feet, and 2 800 pounds on spans from 150 to 200 feet. In addition to the weight of the trusses, floor and track, and to the strains thus deduced, must be added the effect due to a concentrated weight of 30 000 pounds on each track, moved across the bridge. The strains upon the entire floor system will be computed for a moving load of 6 000 pounds per foot of track." Also, Mr. George H. Pegram, M. Am. Soc. C. E., in the *Transactions* for June, 1886, called attention to such a method, and in his paper gave some interesting comparisons covering spans as high as 400 feet.

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To verify Mr. Waddell's Table of Equivalent Uniformly Distributed Live Loads, given on page 8 of his paper, I submit herewith Table No. 1, which I computed in February, 1884, and which is for a consolidation engine and tender weighing 88 tons in 54 feet. The loading and spacing for this engine are given on Plate XXII. This is very nearly like Mr. Cooper's Class A engine, shown on page 6 of Mr. Waddell's paper,

TABLE No. 1.

TABLE OF ACTUAL MAXIMA MOMENTS, End Shears and Cross-girder Loads for 88-Ton Consolidation Engine, and uniformly distributed loads equivalent thereto.

Eighty-eight-Ton Consolidation Engine and Tender, 54 feet long, out to out.

	MAX. MOMENTS.		END SHEARS.		CROSS-GIRDER LOADS.	
Span in feet.	Actual Max. Moment. Foot-tons.	Equivalent load per linear foot of track. Pounds.	Actual End Shear. Tons.	Equivalent load per linear foot of track. Pounds.	Actual Max. load on Cross-girder. Tons.	Equivalent load per linear foot of track. Pounds.
1.....	3.0 } Case I.	48 000	12.0 } Case I.	48 000	12.0 } Case I.	24 000
2.....	6.0	24 000	12.0	24 000	12.0	12 000
3.....	9.0	16 000	12.0	16 000	12.0	8 000
4.....	12.0	12 000	12.0	12 000	12.0	6 000
5.....	15.0 } Case II.	9 600	13.2 } Case II.	10 560	14.4 } Case III.	5 777
6.....	18.0	8 000	15.0	10 000	18.0	6 000
7.....	21.0	6 857	16.3	9 308	20.6	5 877
8.....	24.8	6 199	17.3	8 650	22.5	5 625
9.....	30.4 } Case III.	6 000	18.0 } Case III.	8 000	24.0 } Case IV.	5 333
10.....	36.0	5 766	19.8	7 920	26.4	5 280
11.....	45.0	5 950	21.3	7 734	29.4	5 156
12.....	54.0	6 000	22.5	7 500	30.0	5 000
13.....	63.0	5 964	23.5	7 242	31.7	4 875
14.....	72.0 } Case IV.	5 877	24.9 } Case IV.	7 104	33.4 } Case V.	4 776
15.....	81.0	5 760	26.4	7 040	34.9	4 659
16.....	90.0	5 625	27.7	6 939	36.2	4 530
17.....	99.6	5 512	28.9	6 810	37.4	4 412
18.....	111.4	5 500	30.0	6 667	38.4	4 271
19.....	123.2	5 460	30.9	6 516	39.4	4 144
20.....	135.0	5 400	31.8	6 360	40.8 } Case VII. VIII.	4 080
21.....	146.9	5 330	32.6	6 204	42.3	4 027
22.....	158.8	5 248	33.5	6 084	43.6	3 967
23.....	170.6	5 162	34.4	5 990	44.9	3 902
24.....	182.5	5 070	35.3	5 888	46.0	3 833
25.....	194.4	4 977	36.2	5 786	47.4	3 789
26.....	206.3 } Case V.	4 884	36.9 } Case V.	5 680	48.9	3 763
27.....	220.0	4 828	37.6	5 574	50.4	3 731
28.....	234.0	4 776	38.3	5 470	51.7	3 694
29.....	248.0	4 716	38.9	5 366		
30.....	262.0	4 658	39.7 } Case VI.	5 298		
31.....	276.0	4 592	40.5	5 228		
32.....	290.0	4 532	41.2	5 156		
33.....	304.0 } Case VI.	4 466	41.9	5 084		
34.....	318.0	4 400	42.6	5 010		
35.....	332.0	4 356	43.4	4 963		
36.....	346.0	4 272	44.2 } Case VII.	4 913		
37.....	362.7	4 238	45.0	4 862		
38.....	376.6	4 172	45.7	4 809		
39.....	392.6	4 130	46.4	4 755		
40.....	406.5	4 085	47.2	4 720		

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In Table No. 2 I also give similar data for a heavy passenger to such span lengths, beyond which the results would be l those given for the 88-ton consolidation engine in Table No. 1.

TABLE No. 2.

TABLE OF ACTUAL MAXIMUM MOMENTS, End Shears and Cro Loads for 100-ton Fast Passenger Engine, and uniform tributed loads equivalent thereto.

One hundred-ton Fast Passenger Engine and Tender, 54 fe out to out.

Span in Feet.	MAX. MOMENTS.		END SHEARS.		CROSS-GIRDER	
	Actual Max. Moments. Foot-tons.	Equivalent load per linear foot of track. Pounds.	Actual End Shears. Tons.	Equivalent load per linear foot of track. Pounds.	Actual Max. Load on Cross-girders.	Eq l l o
1	5.0	80 000	20.0	80 000	20.0	
2	10.0	40 000	20.0	40 000	20.0	
3	15.0	26 667	20.0	26 667	20.0	
4	20.0	20 000	20.0	20 000	20.0	
5	25.0	16 000	20.0	16 000	20.0	
6	30.0	13 333	20.0	13 333	20.0	
7	35.0	11 430	20.0	11 430	.....	
8	40.0	10 000	20.0	10 000	.....	
9	45.0	8 888	22.2	9 876	.....	
10	50.0	8 000	24.0	9 600	.....	
11	55.0	7 272	25.5	9 258	.....	
12	60.0	6 666	26.7	8 890	.....	
13	65.0	6 154	27.7	8 520	.....	
14	.....	.....	28.6	8 168	.....	
15	.....	.....	29.3	7 822	.....	
16	.....	.....	30.0	7 500	.....	
17	.....	.....	31.2	7 336	.....	
18	.....	.....	32.2	7 160	.....	
19	.....	.....	33.1	6 972	.....	
20	.....	.....	34.0	6 800	.....	
21	.....	.....	34.8	6 620	.....	

Case I. Case II. Case III.

This heavy passenger engine weighs, with its tender, 100 ton feet, as follows:

Two pairs of truck wheels at 10 tons.....	=	20 tons.
Two pairs of drivers at 20 tons.....	=	40 "
Four pairs of tender wheels at 10 tons.....	=	40 "
Total engine and tender.....	=	100 "

The spacing of the wheel loads is:

Pilot to first pair of truck wheels.....	=	5′ 0″
Truck wheel base.....	=	7′ 6″
Rear truck wheel to leading driver.....	=	8′ 0″
Driving wheel base.....	=	8′ 0″
Rear driver to first pair of tender wheels.....	=	8′ 0″
Tender wheel base. 3 at 5′ 0″.....	=	15′ 0″
Rear tender wheel to end.....	=	2′ 6″
Total length of engine and tender.....	=	54′ 0″

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Combining Tables Nos. 1 and 2, in Table No. 3 is given the maximum bending moments for spans up to 40 feet, due either to a 100-ton passenger engine or 88-ton consolidation engine, whichever is the greater.

TABLE No. 3.

TABLE OF MAXIMA BENDING MOMENTS, due to 88-ton Consolidation or 100-ton Passenger Engine, whichever the greater, and equivalent uniformly distributed load per linear foot, corresponding to 2 800 pounds per foot + 28 000 pounds.

Span in feet.	Case Number.	Critical Load.	Distance from center of span to the critical load. Feet.	Actual Maximum Moments per track. Foot-tons.	Equivalent uniformly distributed load per linear foot of track. Pounds.	Equivalent uniformly distributed load corresponding to 2 800 pounds per foot, + 28 000 pounds.
1.....	H. P. L.	P_3 or P_4	0	5.0	80 000	58 800
2.....	"	"	0	10.0	40 000	30 800
3.....	"	"	0	15.0	26 667	21 467
4.....	"	"	0	20.0	20 000	16 800
5.....	"	"	0	25.0	16 000	14 000
6.....	"	"	0	30.0	13 333	12 133
7.....	"	"	0	35.0	11 430	10 800
8.....	"	"	0	40.0	10 000	9 800
9.....	"	"	0	45.0	8 888	9 022
10.....	"	"	0	50.0	8 000	8 400
11.....	"	"	0	55.0	7 272	7 891
12.....	"	"	0	60.0	6 666	7 467
13.....	"	"	0	65.0	6 154	7 107
14.....	Cons. III.	"	0	72.0	5 877	6 800
15.....	"	"	0	81.0	5 760	6 533
16.....	"	"	0	90.0	5 625	6 300
17.....	Cons. IV.	P_3	1.125 left.	99.6	5 512	6 094
18.....	"	"	"	111.4	5 500	5 911
19.....	"	"	"	123.2	5 460	5 747
20.....	"	"	"	135.0	5 400	5 600
21.....	"	"	"	146.9	5 330	5 466
22.....	"	"	"	158.8	5 248	5 345
23.....	"	"	"	170.6	5 160	5 235
24.....	"	"	"	182.5	5 070	5 133
25.....	"	"	"	194.4	4 977	5 040
26.....	"	"	"	206.3	4 884	4 954
27.....	Cons. V.	"	0.071 left.	220.0	4 828	4 874
28.....	"	"	"	234.0	4 776	4 800
29.....	"	"	"	248.0	4 716	4 731
30.....	"	"	"	262.0	4 658	4 667
31.....	"	"	"	276.0	4 592	4 606
32.....	"	"	"	290.0	4 532	4 550
33.....	"	"	"	304.0	4 466	4 497
34.....	"	"	"	318.0	4 400	4 447
35.....	"	"	"	332.0	4 356	4 400
36.....	"	"	"	346.0	4 272	4 355
37.....	Cons. VI.	"	1.25 left.	362.7	4 238	4 313
38.....	"	"	"	376.6	4 170	4 274
39.....	"	"	"	392.6	4 130	4 236
40.....	"	"	"	408.5	4 085	4 200

On Plate XXII is represented the variation in the maxima effects due

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to the 88-ton consolidation engine, and likewise is shown how the results of using a loading of 2 800 pounds per linear foot, plus a concentrated weight of 28 000 pounds, compare with those from the actual engine. Mr. George P. Bland, M. Am. Soc. C. E., to whom I showed the foregoing tables in the spring of 1885, suggested the use of a uniform load in connection with a concentrated weight, to be practically equivalent in all respects to the maxima effects got by considering the wheel loading. He plotted the results from the tables on a diagram, and decided (I think) on 3 000 pounds per linear foot, plus 24 000 pounds, as well fitting the actual curves. I have frequently tested this suggested loading by comparison with the results from wheel concentrations, and in a wide range of spans have always found it gave exceedingly close approximations.

I may add that the Tables Nos. 1, 2 and 3 were computed from the expression—

$$M_o = \frac{\Sigma . P}{4}\left(l + \left[\frac{\Sigma^1 . Pd}{\Sigma . P}\right] . \frac{1}{l} - 2 . \frac{\Sigma . Pd}{\Sigma . P}\right)$$

where M_o = max. bending moment.

$\Sigma . P$ = sum of loads on the span l.

$\Sigma^1 . Pd$ = summation of the moments of the loads on the span around the critical load as origin, regard being had to sign.

$\Sigma . Pd$ = summation of the moments of the loads on the span around the critical load as origin, no regard being had to sign.

d = spacing of the wheel loads.

The critical load is the one under which the maximum bending moment occurs. For a further explanation of the above expression, see my paper, "A Method of Computing the Absolute Maximum Bending Moment on Stringers, etc.," published in *The Engineering News* for February 2d, 1884.

In Table No. 4 is given the maxima bending moments due to two Pennsylvania Railroad consolidation engines shown in its specification of 1887, and for comparison also is given the maxima center moments from the loadings of 3 000 pounds per linear foot plus 30 000 pounds. and from 3 200 pounds per linear foot plus 32 000 pounds. On Plate XXIII there are plotted these results; also the maxima moments from the Pennsylvania Railroad passenger engine of specification of 1887. An examination of the following table and diagram will show the degree of approximation which is given by the two assumed loadings.

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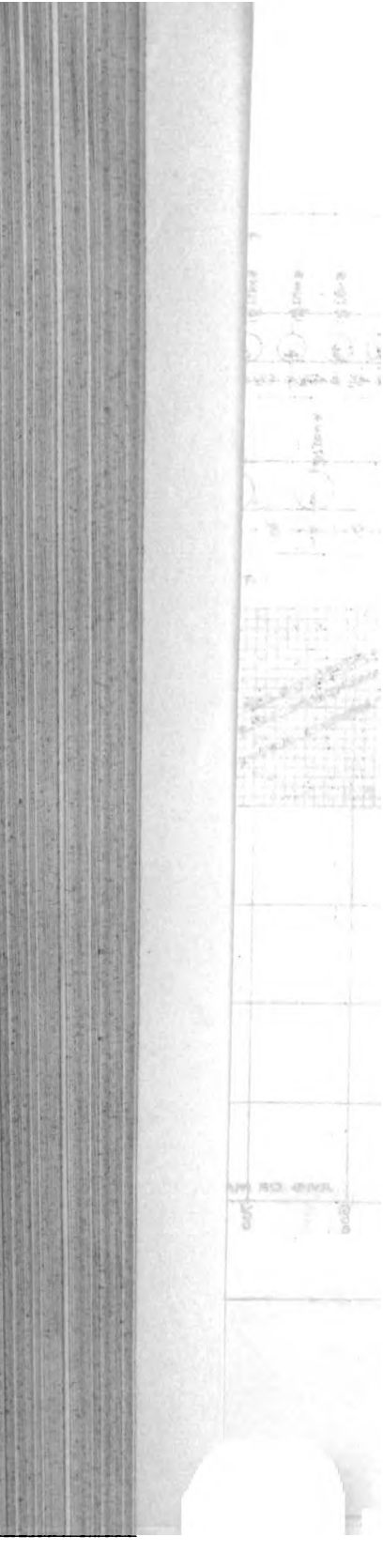

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TABLE No. 4.

TABLE OF ACTUAL MAXIMA MOMENTS due to two typical Consolidation Engines—Pennsylvania Railroad Specifications of 1887. Also the Maxima Center Moments from 3 000 pounds per linear foot + 30 000 pounds, and from 3 200 pounds per linear foot + 32 000 pounds.

Span in Feet.	Case Number.	Critical Load.	Distance from center of span to the critical load, Feet.	Actual Maxima Moments in foot-tons per track.	Max. Center Moments due to 3 000 pounds per linear foot + 30 000 pounds. Foot-tons.	Max. Center Moments due to 3 200 pounds per linear foot + 32 000 pounds. Foot-tons.
1.....	I.	Any driver.	0	3.75	3.94	4.2
2.....	"	"	0	7.50	8.25	8.8
3.....	"	"	0	11.25	12.94	13.8
4.....	"	"	0	15.00	18.00	19.2
5.....	"	"	0	18.75	23.44	25.0
6.....	"	"	0	22.50	29.25	31.2
7.....	"	"	0	26.25	35.44	37.8
8.....	II.	P_8	1.125 left.	31.00	42.00	44.8
9.....	"	"	"	37.97	48.94	52.2
10.....	"	"	"	45.05	56.25	60.0
11.....	III.	"	0	56.25	63.94	68.2
12.....	"	"	0	67.50	72.00	76.8
13.....	"	"	0	78.50	80.54	85.8
14.....	"	"	0	90.00	89.25	95.2
15.....	"	"	0	101.25	98.44	105.0
16.....	"	"	0	112.50	108.00	115.2
17.....	IV.	"	1.125 left.	124.47	117.94	125.8
18.....	"	"	"	139.22	128.25	136.8
19.....	"	"	"	154.00	138.94	148.2
20.....	"	"	"	168.80	150.00	160.0
21.....	"	"	"	183.62	161.44	172.2
22.....	"	"	"	198.45	173.25	184.8
23.....	"	"	"	213.30	185.44	197.8
24.....	"	"	"	228.16	198.00	211.2
25.....	"	"	"	243.04	210.94	225.0
26.....	"	"	"	257.92	224.25	239.2
27.....	V.	"	0.26 left.	274.17	237.94	253.8
28.....	"	"	"	291.16	252.00	268.8
29.....	"	"	"	308.16	267.44	284.2
30.....	"	"	"	325.16	281.25	300.0
31.....	"	"	"	342.15	296.44	316.2
32.....	"	"	"	359.15	312.00	332.8
33.....	"	"	"	376.14	327 94	349.8
34.....	"	"	"	393.14	344.25	367.2
35.....	"	"	"	410.14	360.94	385.0
36.....	"	"	"	427.13	378.00	403.2
37.....	"	"	"	444.13	.....	421.8
38	"	"	"	461.12	413.15	440.8
39.....	"	"	"	478.12	.....	460.2
40.....	VI.	P_{12}	1.31 left.	496.26	450.00	480.0
41.....	"	"	"	515.19	.....	500.2
42.....	"	"	"	534.11	488.25	520.8
43.....	VII.	"	0.21 left.	554.11	.....	541.8
44.....	"	"	"	575.10	528.00	563.2
45.....	"	"	"	596.10	.....	585.0
46.....	"	"	"	617.10	568.25	607.2
47.....	"	"	"	638.10	.....	629.8
48.....	"	"	"	659.10	612.00	652.8
49.....	VIII.	"	1.30 left.	679.19	.....	676.2
50.....	"	"	"	702.12	656.25	700.0
51.....	"	"	"	725.06	.....	724.2
52.....	"	"	"	748.00	702.00	748.8
53.....	"	"	"	770.94	.....	773.8

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TABLE No. 4—(*Continued*).

Span in Feet.	Case Number.	Critical Load.	Distance from center of span to the critical load. Feet.	Actual Maxima Moments in foot-tons per track.	Max. Center Moments due to 3 000 pounds per linear foot + 30 000 pounds. Foot-tons.	Max. Center Moments due to 3 200 pounds per linear foot + 32 000 pounds. Foot-tons.
54.....	IX.	P_{12}	0.18 left.	797.06	749.25	799.2
55.....	"	"	"	822.06	.....	825.0
56 ...	"	"	"	847.05	798.00	851.2
57.....	"	"	"	872.05	.....	877.8
58.....	X.	"	1.31 left.	920.94	847.25	904.8
59.....	"	"	"	946.94	.....	932.2
60.....	"	"	"	973.89	900.0	960.0
61.....	"	"	"	1 000.38	.....	988.2
62.....	"	"	"	1 026.89	953.25	1 016.8
63.....	"	"	"	1 053.41	.....	1 045.8
64.....	"	"	"	1 071.00	1 008.0	1 075.2
65.....	XI.	P_{12}	0.21 left.	1 062.07	.....	1 105.0
66.....	"	"	"	1 111.07	1 064.25	1 135.2
67.....	"	"	"	1 140.07	.....	1 165.8
68.....	"	"	"	1 169.06	1 122.0	1 196.8
69.....	"	"	"	1 198.06	.....	1 228.2
70.....	XII. (a)	P_{12}	1.30 left.	1 226.13	1 181.25	1 260.0
71.....	"	"	"	1 259.06	.....	1 292.2
72.....	"	"	"	1 290.04	1 242.0	1 324.8
73.....	"	"	"	1 321.00	.....	1 357.8
74.....	"	"	"	1 351.96	1 304.25	1 391.2
75.....	XII. (b)	P_{12}	0.95 right.	1 381.60	.....	1 425.0
76.....	XIII.	"	0.99 left.	1 414.48	1 368.0	1 459.2
77.....	"	"	"	1 448.33	.....	1 493.8
78.....	"	"	"	1 483.18	1 433.25	1 528.8
79.....	"	"	"	1 517.52	.....	1 564.2
80.....	"	"	"	1 551.85	1 500.0	1 600.0

TABLE No. 5.

TABLE OF ACTUAL MAXIMA MOMENTS due to two Consolidation Engines and Tenders, each 110 tons, in 54 feet of total length. Showing also Maxima Center Moments due to 3 400 pounds per linear foot of track + 34 000 pounds concentrated.

Span in Feet.	Case Number.	Critical Load.	Distance from Center of Span to the Critical Load. Feet.	Actual Maxima Moments in foot-tons per track.	Maxima Center Moments due to 3 400 pounds per linear foot + 34 000 pounds. Foot-tons.
8	II.	P_2	1.125 left	31.0	47.6
14	III.	P_2	0	90.0	100.9
22	IV.	P_2	1.125 left	198.4	196.3
30	V.	P_2	0.143 left	327.6	318.7
38	VI.	P_2	1.25	470.8	468.4
46	VII.	P_4	0.388 left	645.1	645.1
52	IX.	P_{12}	0.09 right	792.6	795.6
62	X.	P_{12}	0.135 left	1 085.1	1 080.3
70	XI.	P_{12}	1.175 left	1 341.9	1 338.6
78	XIII.	P_{12}	0.541 left	1 649.7	1 624.3

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In Table No. 5 is given the maxima bending moments resulting from two typical consolidation engines somewhat heavier than the engines of the Pennsylvania Railroad specification of 1887. These engines (and tenders) each weigh 110 tons in 54 feet. The spacing is as shown on Plate XXII, and the loading as follows:

One pair of pony wheels at 10 tons.....	=	10 tons.
Four pairs of drivers at 15 tons.....	=	60 "
Four pairs of tender wheels at 10 tons.....	=	40 "
Total engine and tender.....	=	110 "

The Table No. 5 shows how closely the maxima center moments from 3 400 pounds per linear foot, plus 34 000 pounds, coincide with the actual maxima moments. An examination of the tables and diagrams already mentioned will, I think, show the validity of Mr. Waddell's claims, and also indicate that a uniform load per linear foot plus a concentrated weight covers the case as well. This latter is also applicable to the computation of stresses in framed structures, giving a slight excess above the actual stresses in the counters and central panels of web members, a very excellent place, by the way, to have a surplus of metal.

Taking the loading to be $p + Q$, where p = uniform load per linear foot, Q = concentrated weight. Then for girder spans, where p can act continuously:

In flanges.—The maximum moment at any point x from the right abutment, occurs where the span is fully covered with p per linear foot, and Q acting at the point x.

$$\text{Thus } M_x = \frac{px}{2}(l - x) + \frac{Q \cdot x}{l}(l - x).$$

When $x = \frac{l}{2}$, that is, when Q is at center of span, the greatest maximum movement occurs, and is—

$$M_o = \frac{pl^2}{8} + \frac{Ql}{4}.$$

Web.—The maximum shear at any point x from the right abutment occurs when p extends from o to x, and Q is at x.

$$\text{Thus } F_x = \frac{px^2}{2l} + \frac{Qx}{l}.$$

When $x = l$, the greatest maximum shear occurs, and is—

$$F_o = \frac{pl}{2} + Q.$$

Cross Girders.—The maximum load on a cross girder is given by $R = pl + Q$.

For girders where p acts discontinuously, or for truss bridges, the same principles hold good, using, however, the panel load $P = \frac{pl}{n}$ (where n is the number of panels) in connection with Q. The maxi-

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mum at any panel point for the chords, is when the span is fully covered with p at every panel point, and Q at the panel point in question; and the greatest maximum moment occurs when Q is at the center of the span. The maximum shear in any panel x from the right abutment, is when each panel point to the right of x is loaded with p, and there is a load Q at the first panel point to right of x. It will be seen that the loading $p + Q$ conforms fairly closely to the results derived by using wheel concentrations, and where there is divergence it is due to some singularity in the wheel spacing or to peculiar relation between it and the panel length; or, again, to ratio between tender wheel loads and those on drivers, which ratio is usually two-thirds. The uniformly distributed load equivalent to the moment from the engine, M_o is

$$p' = \frac{8\,M_o}{l^2},$$

in which the assumption is made that the engine maximum is at the center of the span. As a matter of fact, it is not always so, but still it never deviates much therefrom. (See Table No. 4.) The uniformly distributed load equivalent to the loading $p + Q$ is—

$$p' = p + \frac{2\,Q}{l} = \text{also } \frac{8\,M_o}{l^2}.$$

Neither the maxima center moments $\frac{p'l^2}{8}$ nor $\frac{pl^2}{8} + \frac{Ql}{4}$ vary much from the engine moment M_o, and any ordinary variation is inconsiderable in view of the fact that the assumed static wheel loads, particularly the driver loads, are so subject to local changes:—such changes as those caused by the centrifugal action of the driver counterweights, varying according to speed and to kind of engine, by the oscillations due to non-balance of engine on its springs, and by the variations in static load on drivers from a rough track or low joint.

Calculating stresses from wheel loadings tends to make the computer feel more than a warrantable confidence in his results, as being the true stresses in the several members. They are, indeed, true if the loads would remain static, which they do not. If allowance for impact, etc., were on the same basis of certainty as the static stresses from wheel loading, there would be no great fault to find; but methods for making such allowances are crude and seem insoluble in exactness. Nor would there be any objection to the use of wheel loading, despite the time wasted in preliminary designs such as Mr. Waddell speaks of, if in the end, in the results attained, closer accordance with the real and true stresses in the structure were obtained. It seems to me, that the alternatives suggested, so closely in accordance with the wheel load results when statically considered, are (their simplicity and ease of application being fully weighed,) fitting substitutes for the method now in vogue, and especially when no two specifications are alike as regards

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their typical engines, the differences frequently being just enough to make them unlike.

I think the use of a uniform load per linear foot, plus a concentrated weight, easier of application than that of a uniformly distributed load equivalent to the engine maxima moments. In the former the calculations are exceedingly simple and direct; in the latter, one has to have his memorandum book or his diagram of moments always at hand, so as to get the necessary equivalent uniformly distributed load, varying, as it does, for every span for even the same engine.

Regarding another point which Mr. Waddell makes—the examination of stress sheets tendered railroad companies in competitive bidding,—if the scheme of $p + Q$ be used, every sheet could be tested by the one standard; whereas, now, "every man has his doctrine," though there is an absolutely mathematical, correct way of getting the static stresses due to wheel loading, which is neither troublesome nor tedious. I may say on such score, I have no very great fault to find with the present wheel concentration requirements. However, in the former method, the time is shortened alike to the designer and to the railroad company's engineer in comparing the several designs.

I would welcome the adoption of a specification in which, like Mr. Bouscaren's specification of 1887, the live load requirements were simply stated by giving the values of p and Q, which corresponded broadly to the rolling stock which governed, or would in time govern, the bridgework on the line. I admit the great value which I think the wheel load requirements have been to us. The consideration of the actual thing which crosses our bridges has turned attention to just what that thing is, and we have realized in a way never before attained, to what local variations of loading an engine is subject, and some insight into the causes of such variations; and though no exact means is at hand to determine the true dynamic stresses resulting from these variations in wheel loading, this very uncertainty paves the way for other and quicker methods of getting the static stresses, to which we can (as we do now) make allowances for impact, etc., either by percentages to the apparent (static) stresses, or by a diminution of the working stresses, and attain results equally worthy of confidence.

As Mr. Waddell remarks. "All assumed loads for computing stresses in bridges are merely typical," and it is the provoking differences in these wheel concentrations for practically the same class of bridge, which makes it especially hard on the engineer of a bridge company, whose daily duties are so increased by having to furnish with his bid a stress sheet complete—any minor inaccuracies in which throw doubt on his company's integrity or capacity to undertake the work, and makes it easier to give the work to the bridge company, which, as the author states, may have the "inside track."

Mr. Waddell also objects to the "added insult" of the computa-

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tions being required to cover two different types of engines, and in some cases three. Such a demand is necessary when wheel loading is used, for the heavier concentrations on the drivers of an eight-wheel engine, even though farther apart, affect small spans. An example of this can be seen by inspecting Tables No. 2 and 3, where the heavy passenger engine governs the flange stresses in spans up to 13 feet, the end shears up to 20 feet, and cross-girder loads to 6 feet. This last is perhaps infrequent, and the increase in end shears beyond 16 feet is inconsiderable. It may be remarked, however, that the Pennsylvania Railroad use 8 feet panels in their through plate girder spans. The use of the loading $p + Q$ covers well all these divergences.

As to "The Proper Live Load for Modern Bridge Specifications." Increase in engine weight and car capacity has been steadily going on since Mr. Cooper wrote his first specification, and the limit is not yet. Car capacity has steadily grown from the 40 000 pounds of a few years ago to the very ordinary 60 000 pounds of to-day. Engine weight, which ten to twelve years ago was for heavy consolidation engines, 95 700 pounds, 82 700 pounds of which was on the drivers, has increased to 114 600 pounds, 100 600 pounds of which is on the drivers, an increase of 21½ per cent. on drivers, or 20 per cent. increase in weight of the whole engine.

Class C passenger engine of Pennsylvania Railroad weighed 81 800 pounds; Class K, of the same line, weighs 96 700 pounds, and Class P weighs 100 600 pounds; roughly an increase of weight in the later types of 20 per cent. Excluding such peculiar types as the El Gubernador of Central Pacific, having 121 600 pounds on ten drivers, and the Baldwin Decapod of 128 000 pounds, on ten drivers; and, later, the engines working the St. Clair Tunnel, Grand Trunk Railway, having 180 000 pounds on ten drivers, in a space of 18½ feet, all of which were designed for special work in comparatively limited localities; we are justified, I think, in saying that the increased requirements of railway traffic of the past ten years have demanded about 25 per cent. increase in the weight of the engines. In other words, the driver loads in engines of the consolidation type have increased from 20 000 to 25 000 pounds per pair.

As regards the cars, while the capacity has increased 50 per cent., the weight has increased but about 20 per cent., or the total loading but 40 per cent. The present stock weighs loaded about 2 500 pounds per linear foot of track, the length being about 34 feet, and the wheel base about 27 feet; and allowing for overloading, these cars would weigh probably close to 2 700 pounds per foot of track.

Roughly speaking, then, the past ten years has witnessed an increase in engine weight of 25 per cent. and an increase in car weight of 50 per cent., the former affecting materially all floor systems and spans say 100 feet and under, and the latter affecting all other spans to at

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least 25 per cent., since the train load on lines having this kind of traffic was formerly rarely assumed less than 1 gross ton per foot. On some lines the increased weight of train load would cause no excess, since even ten years ago the rolling load was assumed at 3 000 pounds per linear foot.

Tables Nos. 6, 7 and 8 give data for rolling stock in actual use at present in Colorado and Utah, and in Tables Nos. 7 and 8 the Colorado engines are compared with Pennsylvania Railroad engines and with English engines doing same class of work. Inspecting Table No. 7, and bearing in mind that Class R is, I think, the heaviest consolidation type now in use on that line, it is seen that the Denver and Rio Grande Class 112 is but 2 100 pounds lighter, or engine and tender together 5 per cent. heavier than Class R; that the Colorado Midland Consolidation engine has 13 per cent. more weight on the drivers, the engine and tender together being 20 per cent. heavier than Class R; however, it carries 33⅓ per cent. more water and 100 per cent. more coal, operating as it does on a 3 per cent. line.

In Table No. 8 comparison is made of the eight-wheel Class P of the Pennsylvania Railroad with the ten-wheel engines in Colorado and Utah, these latter being used for all passenger service as well as for light freight trains. It will be noticed that the Colorado Midland ten-wheeler has 16 500 pounds on each driver.

TABLE No. 6.

STANDARD FREIGHT STOCK IN ACTUAL USE ON DENVER AND RIO GRANDE RAILROAD.

	Gondola Coal Car.	Hopper Bottom Coal Car.	Low Flat Car—Continuous Draw Bar.	Box Car.	Stock Car.
Length out to out of bull-nose	34′ 6″	30′ 6″	34′ 8½″	34′ 6″	34′ 6″
Length over end sills	32′ 0″	28′ 0″	32′ 0″	32′ 0″	32′ 0″
Max. width of car, out to out.	9′ 8″	10′ 0″	9′ 2′	9′ 3″	9′ 3″
Number of trucks	2	2	2	2	2
Kind of truck	4-wheel	4-wheel	4-wheel	4-wheel	4-wheel
Wheel base of truck	4′ 8″	4′ 8″	4′ 8″	4′ 8″	4′ 8′
Wheel base of car	27′ 3″	23′ 0″	27′ 3″	27′ 3″	27′ 3′
Weight of car	24 000	25 000	21 000	26 000	26 000
Capacity of car	60 000	60 000	60 000	60 000	60 000
Total weight of car and load	84 000	85 000	81 000	86 000	86 000
Weight per linear foot of track	2 435	2 785	2 335	2 495	2 495
Ratio $\frac{\text{weight}}{\text{capacity}}$	0.40	0.42	0.35	0.43	0.43
Ratio $\frac{\text{capacity}}{\text{total weight}}$	0.28	0.29	0.26	0.30	0.30
Relative capacity to weight	1 to 2⅕	1 to $2\frac{4}{10}$	1 to $2\frac{2}{10}$	1 to $2\frac{4}{10}$	1 to $2\frac{4}{10}$

MEMORANDUM.—Allowing for overloading, 2 700 pounds per linear foot will cover the weight of train.

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TABLE No. 7.

Freight Engines in Actual Use in 1891.

	Class R. Engine consolidation, Penna. Railroad, 1886.	Class 112. Consolidation Engine, Denver and Rio Grande Railroad, 1888.	Colorado Midland, Consolidation Engine, 1888.	Class 133½. Consolidation Engine; Rio Grande Western Railway, 1891.	Freight Engine, Great Northern Railway, England.
Weight on pony truck	14 000	16 700	12 000	16 700	none. six.
Weight on eight drivers	100 600	95 800	114 000	98 300	90 160
Total weight of engine	114 600	112 500	126 000	115 000	90 160
Weight of tender loaded	57 800	68 000	82 000	68 000	58 800
Total weight of engine and tender	172 400	180 500	208 000	183 000	148 960
Rigid wheel base	13' 10"	13' 6"	14' 6"	13' 6"	17' 7"
Engine wheel base	21' 9"	21' 4"	22' 0"	21' 6"	17' 7"
Tender wheel base	16' 4"	16' 0"	15' 6"	15' 10"	20' 7"
Total wheel base of engine and tender	48' 9"	48' 4"	48' 0"	48' 4"	48' 7"
Total length of engine and tender, out to out	57' 8"	about 58' 6"	58' 0"	60' 0"	.....
Water capacity of tank	3 000 gals.	.....	4 000 gals.	3 200 gals.	.....
Coal capacity of tender	8 000 lbs.	.....	16 800 lbs.	15 680 lbs.	.....
Diameter of drivers	50"	45"	51"	45"	.....
Cylinders	20" x 24"	20" x 24"	20" x 26" generally	20" x 24"	19" x 28"
Maximum grade	.....	3%	3%	3.8%	
General grade	.....	1 and 1½%	3%	1%	
Maximum curve	.....	12°	16°	about 10°	

TABLE No. 8.

Passenger Engines in Actual Use in 1891.

	Class P, Passenger Engine, Penna. Railroad.	Class 106, Passenger and Freight. Denver and Rio Grande Railroad, 1888.	Colorado Midland Railway Passenger Engine, 1888.	Rio Grande Western Railway Passenger Engine, 1891.	English Express Engine.
Kind of engine	8-wheel.	10-wheel.	10-wheel.	10-wheel.	6-wheel.
Number of drivers	4	6	6	6	2
Weight on trucks	40 850	26 300	21 850	.....	.....
Weight on drivers	65 150	84 400	98 950	Practically the same as Denver and Rio Grande. Class 106.	34 000
Total weight of engine	106 000	106 700	120 800		85 000
Weight of tender, loaded	60 000	68 000	82 000		.....
Total weight of engine and tender	166 000	174 700	202 800		.....
Rigid wheel base	7' 9"	11' 9"	12' 0"		.....
Engine wheel base	22' 7½"	23' 0½"	22' 6"		15' 6"
Tender wheel base	15' 4"	16' 0"	15' 0"		12' 0'
Total wheel base of engine and tender	47' 8"	47' 3½"	48' 9"		.....
Total length of engine and tender, out to out	56' 2"	58' 0"	56' 9"		.....
Water capacity of tank	3 000 gals.	.....	4 000 gals.		2 400 gals.
Coal capacity of tender	12 000 lbs.	.....	16 800 lbs.		.....
Diameter of drivers	68"	54"	57"		84", 17" dia.
Cylinders	18½" x 24"	18" x 24"	19" x26"		

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Bridges of different capacities will, of course, be required, according to the varied positions of the lines, the kind and volume of the traffic, actual or probable and the gradients and alignments, the last two governing the class of locomotives to be used. Mr. Cooper's four diagrams of consolidation engine loading, seem to me to cover broadly the several classes of railways operating in the United States. I agree, however, with Mr. Waddell in his plea for more uniformity and greater simplicity in the spacing of the loads. For consolidation engines all the spacing could, I think, be multiples of 5 feet. I would exclude the mogul engine and its 2 000-pound train load, and assume the train loads for the heavy grade, extra Class A, Class A and Class B to be respectively 4 000, 3 400, 2 800 and 2 500 pounds per linear foot.

Retaining Mr. Cooper's notation, on the system of calculation of $p + Q$, the scheme could be written as follows, and which I think would give practically the same class of bridge.

Heavy Grade..... $p + Q = 4\,000$ pounds $+ 40\,000$ pounds.
Extra Class A..... $p + Q = 3\,400$ " $+ 34\,000$ "
Class A..... $p + Q = 2\,800$ " $+ 28\,000$ "
Class B..... $p + Q = 2\,500$ " $+ 25\,000$ "

These would correspond pretty well with the following engines:

First.—Heavy Grade Consolidation Engines and 4 000-pound train.

On each truck and tender axle..... 20 000 pounds.
On each driver axle..... 40 000 "
Total weight engine and tender.....260 000 "

Second.—Extra Class A, Consolidation Engines and 3 400-pound train.

On each truck and tender axle..... 20 000 pounds.
On each driver axle..... 30 000 "
Total weight engine and tender.....220 000 "

Third.—Class A, Consolidation Engines and 2 800-pound train.

On each truck and tender axle..... 16 000 pounds.
On each driver axle..... 24 000 "
Total weight engine and tender.....176 000 "

Fourth.—Class B, Consolidation Engines and 2 500-pound train.

On each truck and tender axle..... 14 000 pounds.
On each driver axle..... 22 000 "
Total weight engine and tender.....158 000 "

All the foregoing engines to have the following spacing:

Pilot to truck..... 5′ 0″
Truck to leading driver.....10′ 0″
Three driver spaces at 5′ 0″.....15′ 0″
Rear driver to first tender wheel..... 10′ 0″
Three tender spaces at 5′ 0″.....15′ 0″
Rear tender wheel to end..... 5′ 0″

Length of engine and tender out to out = 60′ 0″

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Table No. 9 gives the typical consolidation engines in use on the Denver and Rio Grande Railroad as compared with those of the Pennsylvania Railroad's specification of 1887.

TABLE No. 9.

TYPICAL FREIGHT ENGINES used in Bridge Specifications on the Denver and Rio Grande Railroad and on the Pennsylvania Railroad.

	Typical Consolidation Engine of Pennsylvania Railroad. Specifications of 1887.	Typical Consolidation Engine. Denver and Rio Grande Railroad. Load Diagram No. 5 1888.
Weight on pony trucks	16 000	20 000
Weight on each pair of drivers	30 000	30 000
Weight on four pairs of drivers	120 000	120 000
Total weight of engines	136 000	140 000
Weight on each tender axle	16 000	20 000
Total weight of tender	64 000	80 000
Total weight of engine and tender	200 000	220 000
Rigid wheel base	13' 6"	13' 6"
Engine wheel base	21' 6"	21' 6"
Tender wheel base	15' 6"	15' 0"
Total wheel base of engine and tender	48' 6"	46' 6"
Total length of engine and tender, out to out	56' 6"	54' 0"

While the assumed driver loads in each are the same, the tender loads of the former are necessarily greater, having to carry more coal and water. The Denver and Rio Grande engine and tender is just 10 per cent. heavier than the present requirements of the Pennsylvania Railroad. That this excess is justified is seen by reference to Table No. 7 of actual data, and moreover I think the chances for further increase in the weight of engines are greater on such lines as the Denver and Rio Grande than on the Pennsylvania Railroad, though in the matter of train load the reverse would be the case—the one operating a comparatively low grade line, the other a line having a maximum gradient of 3 per cent., and curves of 10 and 12 degrees. It should be mentioned too that the Colorado Midland consolidation engines (and tender) weighing only 8 per cent. less than the typical consolidation engines of the Denver and Rio Grande, for several months of this year ran over 75 miles of the Denver and Rio Grande tracks.

Considering the Tables Nos. 6, 7, 8 and 9, in which it is shown that the car stock of the Denver and Rio Grande Railroad is abreast of modern requirements, the actual consolidation engines in use slightly heavier than those of the Pennsylvania Railroad, and the typical consolidation engine necessarily 10 per cent. heavier, for reasons already given, than that of the latter line; the question arises: if the assumed

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engine loads are practically alike, should the bridges be designed with the same margin of strength ? that is, should the same working stresses be used ? My answer would be, bridges on the former line should be made fully up to the very best requirements of present practice, while those on the Pennsylvania Railroad and like lines should have a wider margin of strength than now obtains.

Excluding shifting engines at Denver, there are all told about seventy trains per twenty-four hours belonging to the Denver and Rio Grande tracks, whilst in the Philadelphia terminal of the Pennsylvania Railroad there are, perhaps, close to six hundred trains every twenty-four hours. Out well on the line of the Denver and Rio Grande there are possibly no more than twelve or fifteen trains per day, whilst on the Pennsylvania Railroad Division there are at least one hundred and fifty. To carry the comparisons farther away, at Clapham Junction, London, there is a train passing at an average of every fifty-four seconds from 7 A.M. to 10 P.M.; and at Cannon Street Bridge, London, thirty-five trains have been counted in as many minutes.

Our specifications, worthy the name, make differences in bridges, depending only on differences of assumed loading, the unit stresses used being the same; that is, all the bridges are to be first-class as regards dimensioning. The same assumed loading will give the same bridge, no matter where its location or on what kind of line. We make bridges presumably to last an unlimited time under an assumed loading, by using a factor of about three on a quantity beyond which an unlimited number of repetitions will not cause rupture, or else we use working stresses of about one-third the elastic limit, lowered in certain instances an arbitrary quantity to allow for impact, etc., and yet an increase in assumed loading of about 25 per cent. has seemed to cause signs of weakness to appear in bridges presumably so made. How this can be is hard to understand, unless our marginal factor of three involves in reality less margin than is supposed.

The Wöhler-Launhardt scheme was based on experiments on prepared test pieces, and on loads supposed to be gradually applied at intervals of something like fifteen seconds. It becomes a question to what extent suddenly applied loads would affect the ordinary statement of these experiments. Kirkaldy found a reduction of about 20 per cent. in ultimate capacity, by suddenly applying a load, "without jerk"; but it is in some such way as "with jerk" that our live loads are applied. From some points of view it would seem that a rapidly passing train exerts less damaging effect on a bridge than were the speed less, especially if the track be good. Clearly our bridges will not last an unlimited time, and equally clear is it, if such be the case, that the amount of actual or probable traffic demands a place in bridge designing. On the line cited, a most exaggerated estimate of prospective traffic would not, in fifty years, reach one-fourth of that now

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on the Pennsylvania Railroad; and I think it likely, considering the grades, as now their engines are as heavy as the latter's, we can look for increase in weight sooner than on the Pennsylvania Railroad ; still under the same loading one bridge is doing nearly ten times the work of the other.

I have examined a very fair number of bridges, and, on the basis of first-class working stresses, in some instances found them overstrained 12 to 20 per cent. when passed by the engines in use, a fact which should cause no uneasiness, considering the infrequency of the loading, and there being no evidences of weakness. Had these bridges been crossed by from one hundred to four hundred trains per day, the case would, in my judgment, be very different, though the unit stresses caused by every passage would, in each case, have been the same. In design, too, we figure for double headers, though their use is in small proportion to trains headed by one engine, except, of course, on certain heavy grades, where double headers are the rule.

A short time ago, in a Philadelphia newspaper, it was stated that a bridge over the Schuylkill River was being strengthened because it had grown "tired" under the passage of about 500 trains per day. As the cost of the repairs was put down at $25 000, and as being 12½ per cent. of the original cost of the bridge, I fancied the statement to be substantially true, though I write under correction. Here is a bridge designed by an eminent engineer, built by a most reputable bridge works, presumably proportioned for the heaviest engines of ten years ago, and on working stresses, if anything, lower than in general use, and at least no greater than 5 tons per square inch on lower chord and main diagonal bars.

The bridge is used principally for passenger and express traffic, for such are the trains which form the greater proportion of the five hundred stated. An increase in weight of 25 per cent. in engine and train over that for which it was designed, would only raise the unit stress in tension to 6¼ tons per square inch, which is at least still under one-half the elastic limit, and yet report says the bridge is "fatigued." The same bridge under the same loads but of less frequent application of them would, I dare say, show no signs of weakness nor necessity of repair. As it is, does it not show the necessity of making bridges which bear such frequent passages of the load with a wider margin of strength than those, which, though under the same loading, are so situated as now to get but few applications of it, and to be never likely to get as many as five hundred daily, year in and year out? It shows too the best designed bridges do not have an unlimited life, as is the supposition in fixing the working unit.

The New York Elevated structures have required strengthening during the past few years; their engines, it seems, have been increased some 26 per cent. in weight, and the number of trains daily, multi-

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plied enormously. Originally, no doubt, the bridges were well proportioned, with the design and workmanship at least up to the average standard of their day. If, under the originally assumed loads, the working stress was low enough to fit the supposition of unlimited life, would an increase of 26 per cent. in the load itself, raise the unit stresses to a limit where weakness was exhibited ?

It seems highly probable that very frequent applications of such loading do cause dynamic stresses quite beyond what is supposed, and bridges at terminals and like places, passing hundreds of trains daily, some as often as one a minute for at least twelve hours per day, require a much greater margin of safety than is now given even under the usual assumption of unlimited life.

Regarding the structure mentioned in the discussions on "Inspectors and Bridge Work," *Transactions* for December, 1887, page 313, Mr. Cooper says: "There have been, however, no defects developed in these structures which cannot be clearly traced to faulty design, hasty workmanship, or want of appreciation of traffic demands; and not a bit of evidence can be found to show that the rapidity or number of trains have developed defects that would not in time have been produced under other conditions of the same loads. I do not believe that any number of applications of the loading will produce any more injury than a single loading upon a structure so proportioned that the actual strains shall never exceed the elastic capacity of that material."

Mr. Sloan (page 310) said, "The structures were designed by careful engineers, and built and erected by responsible bridge building firms"; he also says that "due to an increase in weight of engines from 17 to 19 and 21½ tons, and, to keep the factor of safety within the limit prescribed by the Rapid Transit Commissioners, we have strengthened the floor beams on the Sixth Avenue pin-connected structures, and strengthened the Third Avenue girders by a double system of triangulation."

In view of Mr. Sloan's statement, the question suggests itself, was the design, which, Mr. Cooper states, was faulty in respect of the judgment of to-day, considered so at the time of building ? If the former, though there has been great progress in design during the past ten years, is there any finality about our present knowledge and practice ? And as respects Mr. Cooper's statement regarding the action of stresses within the elastic limit, such is true, but the question arises: may not the dynamic stresses (repeated about one thousand times a day), induced on the structure, coupled with shock, etc., have been such that, in connection with 26 per cent. increase in weight of engines, the elastic limit has been nearly approached if not exceeded ?

By repeated stress beyond the original elastic capacity, the limit may be "exalted," and experiment shows very much so; but it certainly renders the bar or structure less able to resist the frequently

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repeated applications of stresses, or, in other words, the capacity to endure work is very much lessened. Of course, if the engines were increased 26 per cent. in weight or thereabouts, and at the same time certain commissioners lowered the allowable working stresses, the required strengthening offers no subject for either comparison or criticism, nor any grounds for argument as to the necessity of a greater margin of strength in structures subjected to maxima loads as often as every minute or two.

Though the question may receive no answer from the Elevated Lines of New York, in the case of the Philadelphia bridge no State Commissioners have had any jurisdiction, the required strengthening being undoubtedly evident to the railroad officials and so ordered. It is rather curious to note that, in both instances, the structures are almost entirely under passenger traffic, though, without doubt, the Philadelphia bridge was designed for consolidation engines followed by a 3 000-pound train. In this connection it may not be amiss to mention "Cooper's Counter-rod," quoted by Prof. Thurston. It had tenacity, 44 000 pounds per square inch; elastic limit, 36 000 pounds per square inch; fractured area = original area; elongation = none; fracture, crystalline; facets, large, and as ductility may be deduced from elongation and contraction of area, the above is suggestive. It would likewise be interesting to consider, for example, what Prof. Thurston says on the "law of fatigue and the refreshment of metals."

Intensities of Working Stresses.—Regarding Mr. Waddell's assumption (page 21 of his paper) "that in the near future the material for the metal portions of railway bridges will be exclusively steel," it may be remarked that the most important railroad in the country still refuses to use steel in its structures, even, I understand, to the exclusion of steel eye-bars; and that a large line west of Pittsburgh, by implication, from reading its specification, contemplates the use of iron structures to the exclusion of steel. Until such railroads can be shown the "errors of their ways," I fear Mr. Waddell's "near future" will be somewhat remote.

As to the relation between statically applied loads and the same loads applied with different velocities, this, as Mr. Waddell says, is still practically unsolved, and though one of the most important points in the design of a bridge, there seems little unanimity of opinion amongst our best engineers regarding even the points of view from which to approach the subject. The Society has had the benefit of two different papers which, I think, are worthy of close attention, viz., Professor Robinson, M. Am. Soc. C. E., in *Transactions* for June, 1886, pp. 432 to 437, and the same on "Vibration in Bridges," in *Transactions* for February, 1887; and Mr. William H. Booth, M. Am. Soc. C. E., on the "Stresses in Bridges," in *Transactions* for April, 1889. These papers, though independent, overlap to some extent in their conclusions,

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especially those relating to the avoidance of a panel length, which is a multiple of the driver wheel circumference. This question, considering the variety of driver diameters on any one line, it seems almost impossible to consider in bridge design, or to allow a defined place in bridge specifications. On one important railroad, for example, the engines in use have driver diameters of 44, 50, 56, 62, 68 and 78 inches.

It will be readily granted, I suppose, that live loads produce effects intermediate between those due to static stresses and those due to shock from a body falling freely by gravity. Mr. Booth, in his paper referred to, apparently following this view, assesses the percentage for impact in any span by the ratio of the times occupied respectively by the body in falling freely by gravity through a height equal to the deflection, and by the time occupied in reaching the center of the span. For the deflection he assumes $\frac{1}{1200}$ of the span, which, he states, Professor Robinson's observations show; this deflection is the static deflection due to live load, whereas, I think, it is the dynamic deflection due to live load which should enter the ratio, and this may be assumed as twice the former, or, say, $\frac{1}{1200}$ of the span. Using this in the equation for impact percentages, we get, $p = \frac{50}{35} \cdot \frac{v}{\sqrt{l}}$, which may be written approximately, $p = \frac{3}{2} \cdot \frac{v}{\sqrt{l}}$. , where v = velocity of train in feet per second; l = the span in feet; p = impact percentage. The argument and reasoning leading to the foregoing equation are clearly given in Mr. Booth's interesting paper, to which reference is here made. In order to exhibit the variation in impact percentages for sundry spans and speeds, Table No. 10 is given, calculated from $p = \frac{3}{2} \cdot \frac{v}{\sqrt{l}}$. , for velocities of 30, 40, 50, 60, 70 and 90 feet per second, corresponding to speeds of 20½, 27, 34, 41, 48 and 61¼ miles per hour.

TABLE No. 10.

IMPACT PERCENTAGES FROM $p \frac{3}{2} \cdot \frac{v}{\sqrt{l}}$

Span in Feet.	$v = 30$ $V = 20\frac{1}{2}$	$v = 40$ $V = 27$	$v = 50$ $V = 34$	$v = 60$ $V = 41$	$v = 70$ $V = 48$	$v = 90$ $V = 61\frac{1}{2}$
5	20.1	26.8	33.5	40.3	47.0	60.3
10	14.1	18.8	23.5	28.2	32.9	42.3
15	11.7	15.6	19.5	23.4	27.3	35.1
20	9.9	13.2	16.5	19.8	23.1	29.7
30	8.1	10.8	13.5	16.2	18.9	24.3
40	6.9	9.2	11.5	13.8	16.1	20.7
50	6.3	8.4	10.5	12.6	14.7	18.9
60	5.7	7.6	9.5	11.4	13.3	17.1
70	5.4	7.2	9.0	10.8	12.6	16.2
80	5.1	6.8	8.5	10.2	11.9	15.3
90	4.8	6.4	8.0	9.6	11.2	14.4
100	4.5	6.0	7.5	9.0	10.5	13.5
150	3.6	4.8	6.0	7.2	8.4	10.8
200	3.3	4.4	5.5	6.6	7.7	9.9
250	2.7	3.6	4.5	5.4	6.3	8.1

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There is also to be considered the centrifugal force due to vertical non-balance of the counterweights on the drivers. I find that an average of 200 pounds per wheel acting at the crank pin center, represents fairly closely the quantity to be considered. When the counterweight is "down," the dynamic augment to the wheel load is given by

$$P = \frac{W \cdot v^2}{g \cdot r} = .0031\ v^2 .$$

where P = centrifugal force in tons due to the counterweight.
W = the above 200 pounds = $\frac{1}{10}$ ton.
v = velocity of crank pin in feet per second.
g = gravitation unit = 32.
r = radius of circle described by center of crank pin = generally 1 foot.

Assuming consolidation engines having 12 000 pounds on each driver of 44 inches diameter; ten-wheelers having 15 000 pounds on each driver of 54 inches diameter; and eight-wheel passenger engines having 16 000 pounds on each driver of 64 inches diameter; and further assuming two crank pin velocities in each case, viz., 20 and 30 feet per second, Table No. 11 is constructed, which shows the dynamic augment when the counterweight is "down" and its percentage of excess above the static load on the driver wheel. It is, of course, to be remembered that when the counterweights are "down" on one side of the engine they are horizontal on the other side, and on that side exert no vertical excess; also that when counterweights are "up," the static wheel load is decreased by the same amount as it was before augmented when counterweights were "down"; so the range or variation in the dynamic load of a wheel is double the dynamic augment. Further, the crank pin velocity is greater the less the diameter of the driver, so that small drivers at high speeds are the most damaging.

TABLE No. 11.

DYNAMIC AUGMENT to Driver Loads due to action of Counterweights, calculated from $P = .0031\ v^2$.

Kind of Engine.	Speed in Miles per Hour.	Velocity of Crank Pin in feet per second.	Dynamic Augment on each driver when counterweight is down. Tons	Load on each driver. Tons.	Per cent. of excess above the static driver load.
Consolidation	25	20	1.24	6.0	20⅔
"	37½	30	2.79	6.0	46½
Ten-wheeler	30⅔	20	1.24	7.5	16½
"	46	30	2.79	7.5	37½
Eight-wheel pass...	36½	20	1.24	8.0	15½
" ..	54⅔	30	2.79	8.0	35

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If any one doubts the weight which considerations such as the foregoing should have in bridge design, let him ride on an engine for a few days. He will then appreciate in a practical way how the load is applied to a railway bridge,—what blows the floor-system has to stand,—the meaning of "plunging" and the engine "working hard,"—how the weight carried by the engine springs is thrown now on this side, now on that—and what shocks are caused by a bad track or low joints; and if he recalls the fact that action and reaction are equal and opposite, his views of moving load may change. From considering a rapidly moving body of invariable distribution of loading gliding over an imaginary track without joints, he will remember all the above; and last, but not least, he will realize that the condition of the track leading to the bridge determines how the engine will be acting when it crosses, and perhaps conclude that next in importance to a stiff floor system, is a good track leading to and over it.

Considering the form cited by Mr. Waddell (page 23):

$$\text{Intensity} = \text{constant}\left(1 + \frac{\text{min. stress}}{\text{max. stress}}\right).$$

In this expression there is allowance both for the supposed effects of fatigue and for impact. Excluding impact, and allowing only for the effect of an unlimited number of repetitions of stress, the expression is commonly written:

$$\text{Intensity} = \text{constant}\left(1 + \tfrac{1}{2} \cdot \frac{\text{min. stress}}{\text{max. stress}}\right).$$

Are not the above mere empirical modifications of Launhardt's formula, of which the general form he gave was

$$a = w\left(1 + \frac{t - w}{w} \cdot \phi\right).$$

where

a = the ultimate strength of the member.

w = the stress which by an unlimited number of repetitions failed to break the bar, the bar after each repetition returning to an unstrained condition.

t = the ultimate capacity of the piece under one steady slow stress, viz., the static breaking weight.

Now, the value, $\frac{t-w}{w} = \frac{1}{2}$, gives $w = \frac{2}{3}t$; and the value, $\frac{t-w}{w} = 1$, gives $w = \frac{1}{2}t$. Thus the first expression considering ultimate strength is, $a = \frac{t}{2}(1 + \phi)$; and the second is, $a = \frac{2}{3}\ t\ (1 + \frac{1}{2}\ \phi.)$ For an all dead load both become $a = t$; and for an all live load the first becomes $a = \frac{t}{2}$, and the second becomes $a = \frac{2}{3}t$; whence the form,

$\text{Intensity} = \text{constant}\left(1 + \frac{\text{min. stress}}{\text{max. stress}}\right)$ agrees with the assumption that a member under an all live load repeatedly applied, is capable only

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of standing one-half of what it would were the load all dead and once applied, and therefore presumably includes impact. And the form,

$$\text{Intensity} = \text{constant}\left(1 + \tfrac{1}{2}\frac{\text{min. stress}}{\text{max. stress}}\right)$$

agrees with the assumption that a member under an all live load when indefinitely applied is capable of standing but two-thirds of what it would were the load all dead and once applied, and therefore excludes impact, agreeing as it does with Launhardt's original expression.

The values given to t and w will, of course, vary as the member is a rolled bar, or of plates or shapes. If the constant be considered to represent w divided by a marginal factor, the result gives the working stress, and this marginal factor could change with due regard to the place and office of the member in the structure. The use, however, of Launhardt's formula or modifications of it to cover impact, has already received wide discussion, unnecessary to repeat. Paper No. 1882 of the Institute of Civil Engineers, by Weyrauch, goes well into the matter from various points of view. Also the subject was pretty well threshed out in the *Transactions* for June, 1886, in discussion of Mr. J. M. Wilson's specifications. The opposite views then expressed had able supporters, and it seemed impossible to reconcile them; nor since that time have there been, to my knowledge, any further experimental data to throw light on the question.

I have made extended use of both the forms:

$$f = \frac{w}{n}(1 + \phi)$$

$$f = \frac{w}{n}(1 + \tfrac{1}{2}\phi)$$

the former in the case of railway bridges, since it, to at least some extent, makes allowance for impact; the latter for roadway bridges, wherein, no matter what view is taken, there is a difference, both in kind and degree, in the manner of application of live load from that occurring in railway bridges. As the foregoing formulas are essentially those based on ultimate strength, it seems consistent that a specification employing them should likewise use post formulas based on ultimate resistance. If one regards the matter from the point of view of primitive elastic capacity, this view should be carried out to embrace compression members as well.

In Mr. Cooper's specification, by using one-half the working stress for live load that is allowed for dead load, inferentially he adopts the view that a live load is twice as destructive as a dead load, a view, in my judgment, well to take, especially considering the dynamic action of live load coupled with shock. Mr. Cooper assesses the necessary section in a member as depending on the absolute amounts of dead and live stress acting thereon, the ratio of minimum to the maximum stress not being involved. As I understand the Wöhler experiments,

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the range of stress variation was what was primarily considered, the ratio of minimum to maximum stress not necessarily entering into the expressions covering the results. However this may be, and however admirably the Launhardt formula fits the case for railroad bridges, in roadway bridges the manner of application of the live load does not in any way conform to the way in which the experiments which gave rise to the formula were conducted.

Referring again to the form

$$f = \frac{w}{n}(1 + \phi),$$

which includes an allowance for impact, the broad assumption was made that the effect of impact diminishes with the increased weight of the member, and as the weight of the web members increases with tolerable uniformity from the center to the ends of the span, equally so the ratio ϕ. On this assumption the formula, $a = \frac{2}{3}l\,(1 + \frac{1}{2}\,\phi)$ was changed to read, $a = \frac{l}{2}\,(1 + \phi)$.

There was then a reduction in the value of w of 25 per cent., which vanishes when $\phi = 1$, since then the load being all dead, $a = w = l$. This formula if applied to web members and floor systems works well, but it is not clear why it should apply to chords with the same value of $\frac{w}{n}$; for though in chords and end panel diagonals the values of ϕ are about alike, the manner of application of the stresses is by no means similar. The live load acts directly on the stringers, less directly on the cross-girders, is thence conveyed to the web members, and so to the chords, and the values of ϕ through this series do not, I think, cover the history of impact effect; for in any diagonal bar, for example, part of the live load stress gets there by direct load, and part by the cumulative effect from the other web members nearer the loaded abutment—with relatively little impact from cumulative stress, and considerable from direct load applied at the foot of the bar. It would seem more in accordance with the actual state of affairs to deduce the working stress in a member by the formula $f = \frac{w}{n}\,(1 + \frac{1}{2}\,\phi)$ where $w = \frac{2}{3}l$, and then reduce f by an amount dependent alone upon the amount of direct live stress, the manner of application and liability to shock, etc.

The only recognition I have seen of something like this point of view appears in the specification for the Pennsylvania lines west of Pittsburgh, wherein the impact is covered as follows: "Twenty-five per cent. shall be added to the above load (the engine loading) in calculating the floor system. Twenty-five per cent. of the *maximum panel load* from the above engine shall be added to the stress on all vertical members of the web system, and its oblique equivalent to all diagonals, including end braces." (The italics are mine.) And for riveted bridges

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on same line, "twenty-five per cent. shall be added to the above loads in calculating the floor system. The girders shall be calculated for an addition of 25 per cent. for spans of 20 feet or less, and diminish uniformly to 10 per cent. for spans of 100 feet."

In applying the method of calculation of $p + Q$, 25 per cent. would be added to the stress in web members due to one panel load, $\frac{pl}{n} + Q$, $\frac{l}{n}$ being the panel length; and 25 per cent. to the stresses due to $p + Q$ for floor systems and short spans, diminishing to 10 per cent. for 100-foot spans.

Calculating by engine concentrations, the panel load spoken of might be considered the maximum cross-girder load, since in any absolutely correct method of calculating stresses from engine loading, panel loads are not involved. The idea advanced on page 52 could be stated thus: That part of the live stress which gets to a member by cumulative effect to be allowed for, in connection with the dead stress, by $f = \frac{w}{n}\left(1 + \frac{1}{2} \cdot \phi\right)$, repetition of loading being the governing principle;—the remaining live stress—the amount which acts on the member directly—that is, its static amount—to be allowed for at no greater unit than 6 000 pounds per square inch; then each member will, in my judgment, be proportioned closely to accord with the real action of the loading. It will be noticed that this will give a varying percentage of total live load stress for impact, to each member.

The specifications just referred to—those of the Pennsylvania lines west of Pittsburgh—are in my opinion among the best in existence, certainly the best emanating from a railway company. And they contain in germ, at least, what would be my reply to many points raised by Mr. Waddell in his paper, notably in regard to the following: Spacing of stringers and deck plate girders; use of end floor beams; stiffening the two end panels of the lower chord; "squaring" the ends of stringers on skew bridges; camber in pin connected and riveted bridges; minima sections and thicknesses; and generally the requirements regarding plate girder bridges.

Ward Baldwin, M. Am. Soc. C. E.—In the consideration of the question as to whether a concentrated or uniform loading should be used, there is one class of problems that seems to be often overlooked; but the frequency with which they occur warrants a careful consideration of them in devising a scheme of loading for general use. The railroad engineer is not only concerned with the loading for his bridge specifications, but he is frequently called upon to determine whether some special engine or other concentrated load can go over his road without damage to the existing bridges; and it is not unusual for him to decide whether a certain class of engines may or may not be used on the road.

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To solve problems of this kind readily it is necessary to deal with the actual concentrations, or to have some general method of arriving at close approximations. The method proposed by the author requires the determination of the empirical co-efficients in the formula $W = A + LB$ for each loading, and so would be of no assistance in the solution of such problems as noted above, unless the tables covered a very wide range. The method of using an equivalent uniform loading, determined from the maximum shear in the end panel, first proposed by Mr. C. L. Strobel, M. Am. Soc. C. E., has proved the most generally useful approximation yet published. The author has used "Class A" in finding percentages of error involved in the approximations he uses. It is to be noticed that in the case of this loading, the weight of the engines per linear foot exceeds the uniform load per foot by only 10 per cent. It is not surprising that the approximate solution proposed does not in this case give a large percentage of error; but the limit of error may not be so small for a loading like Class U, where the engine load per lineal foot is 23 per cent. heavier than the uniform load per linear foot.

The author quotes the usually allowed unit stress per shear on the webs of girders, viz., 4 000 pounds for iron, and 5 000 pounds for steel. There seems to be no good reason why, at least in the case of steel, the working stress for shear should be less than 80 per cent. of the working tensile stress.

A. C. Stites, Assoc. Am. Soc. C. E., and Lee Treadwell, Jun. Am. Soc. C. E.—Having been associated with Mr. Waddell in the production of his paper, there is little in the way of original discussion for us to prepare, but we would like to submit some of the valuable results acquired during the various investigations incident to work upon the paper, as an additional contribution to the general good. While the results were arrived at by precise mathematical methods, they were given expression, in the curves (Plates XXIV–XXV) appended to this discussion and the formula herewith submitted, more with a view to practical utility than to scientific refinement. In no case, however, will results obtained from the curves or formula depart from actual conditions enough to warrant their use in any connection being questioned. The formula gives its most exact results for spans from 20 to 35 feet (and was constructed with this end in view, as between these limits lie the panel lengths most largely used), and is as follows:

Maximum end shear $= w + \frac{1}{2}$ (panel length $\times$ equivalent uniformly distributed load), where $w = A + BL$, in which L is the span length and A and B constants to be found in the table given below.

Class.

	Z	*Y*	*X*	*W*	*V*	*U*
A.....	8 500	9 500	10 500	12 000	13 000	14 000
B.....	100	105	110	115	120	125

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G. H. BLAKELEY, Jun. Am. Soc. C. E.—The correct proportioning of railway bridges, in all their parts, to properly resist the varying stresses produced by service, began with the introduction of the specification of concentrated live loads. How often is the engineer reluctantly compelled to condemn bridges built under the old requirements for a uniform load, the same for all spans; which, though highly strained in general, are dangerously so in some members only, such as counters and floor system, due to the inadequate provision for the proportionally higher stresses on these members resulting from actual loads. If the method of engine concentrations has entailed somewhat laborious calculations, the labor has not been expended wholly in vain, for it has been the investigation of the static stresses imposed by actual conditions, which makes the substitution of a sliding scale of equivalent uniform loads possible.

It cannot be successfully contended that the process of calculation for concentrations of loads is as easy or as rapid as for uniform loads, though the labor required in the former is overstated by Mr. Waddell. Those who are almost daily obliged to make such calculations for the design of railway bridges in competition, have found some "short cut" by means of which the labor is greatly abridged, and results obtained, if not of precision, yet of sufficient accuracy for constructive purposes. The graphical method by moment polygons furnishes an easy and rapid mode of deriving results, and by following the method outlined in Sections 72 and 74 of DuBois' "Graphical Statics" the stresses in an ordinary truss, when once the polygons are made, are obtained with less labor and in but little more time than would be required to calculate them for a uniform load.

While no one contends that the actual loads on bridges are identical with the assumed typical concentrations, or that the actual stresses are the same as those calculated therefrom; yet the growth of the increasing weights of motive power on railways has been and continues to be in the direction of the well designed typical loads, and the actual static stresses are proportional to the quantities on our strain sheets. The typical concentrated loads, approaching actual loads as they nearly do, furnish a standard of the capacity of a bridge which can be comprehended by the ordinary railway manager, more readily than any combination or schedule of uniform loads. That this is appreciated by many railway superintendents, who are not engineers, is evident from the fact that they act upon it by ordering their bridges to be built to carry a definite type of locomotive, most frequently the heaviest then in use on their line.

The greatest burden connected with the system of concentrations is not so much its use, as is the unnecessary diversity in the amount and arrangement of the loads made by the specifications of the different railways. The practice among the smaller railways is to specify an

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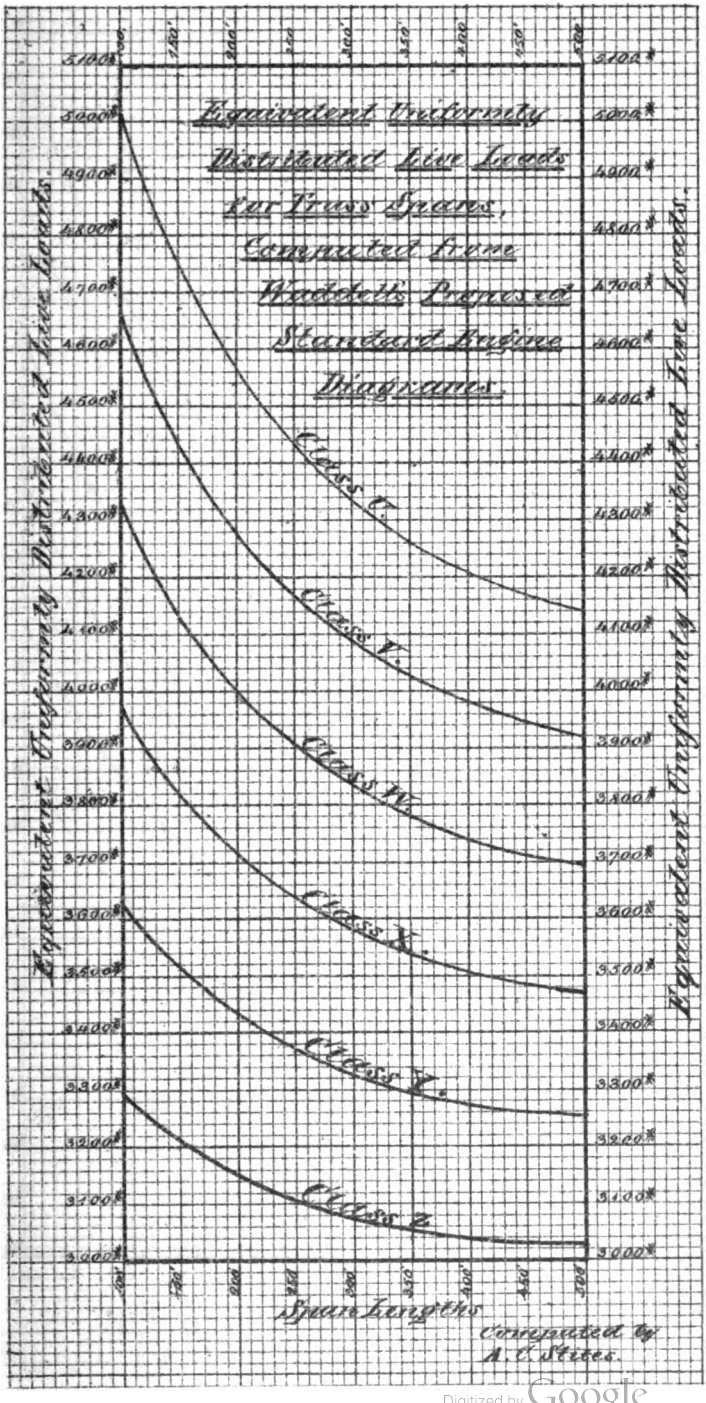
PLATE XXIV.
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XXVI. Nº 522.
STITES & TREADWELL ON
R. R. BRIDGE DESIGNING.



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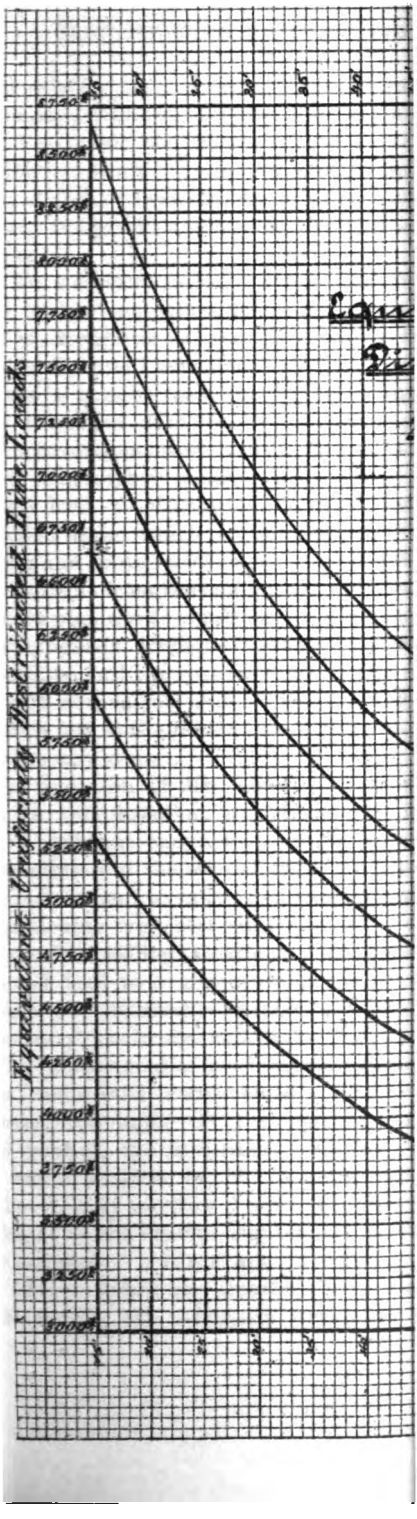

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actual existing train load, and too frequently before the new bridge is upon its pedestals, new and heavier motive power is ordered or in use. Nearly all the railways have their own standards of loading, differing but slightly from one another in their final effect. It is due to this almost endless variety of loads, that the computer of the bridge company is forced to adhere to the method of calculation by concentrations.

When a certain railroad intends to build a bridge and invites a dozen bridge companies to tender proposals, instead of making the labor of designing as easy as possible by adopting a live load the same as the well considered types of any of its near neighbors, it specifies a concentrated loading of unique arrangement. The computer intrusted with the preparation of the design has not seen such an arrangement of loads before, and for aught he knows will not have to deal with it again, hence instead of endeavoring to discover a uniform load producing the same strains, and the conditions and limitation under which it would have to be applied, he finds it easier, quicker and more satisfactory to proceed at once and determine the actual strains from the concentrations themselves. It would seem to be the wiser plan not to abolish the typical standards of loads; but to have fewer standards, arranged in gradation similar to those proposed by Mr. Waddell, to suit the requirements of the service on different railways. Tables of equivalent uniform loads being prepared for these types, it would be left to the preference of the computer what method of calculation he would use.

The adoption of Mr. Theodore Cooper's specifications has done much in this direction, and it is to be hoped that Mr. Waddell will receive sufficient encouragement to prepare tables of uniform loads, representative of the several types of loads accompanying his paper, as an inducement to railway engineers toward the adoption by them of the particular type best suited to their requirements. If this paper shall result in calling attention to the needless diversity in load specifications and lead to a general adoption of standard types, it will have served a most admirable purpose, and lifted from the shoulders of the engineers of the bridge companies "a burden grievous to be borne."

The employment of an integral number of feet for wheel spacings is an undoubted convenience. The weights of the tenders are, however, too light. If the weights on each axle were increased 2 000 pounds, the resulting weights would not be in excess of the actual weights of the tenders used on the leading railways in the vicinity of New York. The neglect to specify the loading produced by the heavier eight-wheel passenger engines is an indefensible omission. It is not always possible to have long panels. Ten and 12-foot panels are unavoidable sometimes, and, upon stringers of this length, the maximum strains are produced by the drivers of

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the passenger locomotive. To cover this effect it is not necessary to provide two distinct sets of typical loads, but only to specify as an additional loading, "80 000 or 100 000 pounds on two axles, 8 feet apart," and it will not be necessary to make two calculations of strains, as the effect of the alternative loading is felt only on short spans, say under 12 feet.

The adoption of the proper live load is a question worthy of consideration. The constant aim should be to construct for the future and not to meet present requirements only. On the other hand, if the live load is taken too high the bridges become too costly, and, with limited expenditure, it is not always possible on existing lines to make all the renewals as fast as required, or on new lines to make the cost of construction come within reasonable bounds. The live loads are taken as low as possible, and the bridges are not long erected before the increase in the weights of motive power and rolling stock strains them too highly. The slight difference in the first cost of bridges built for heavier loads is not properly appreciated. A single track bridge of 150 feet span, built to carry the live loads of Class X, weighs only 4 per cent. more than a bridge of the same span designed for carrying Class Y live loads. The difference in weight does not represent the difference of cost of the two structures, as the labor in the shop and in the erection of both bridges would be the same.

It is safe to say that the difference in cost of bridges built to carry any class of the live loads proposed in Mr. Waddell's paper would not be more than 3½ per cent. greater than if built for the next lighter class. Class Z loading seems to be too light, as there are few railroads, in these times of heavy motive power, that have not in use or contemplation locomotives of the Class Y type, and it does not seem to be a wise policy to build bridges for lighter loads than the latter. At the other limit, it scarcely appears to be a judicious outlay to provide for heavier loads than the Class V type. Those who have seen the enormous consolidation locomotives with 140 000 pounds on the drivers, built for the Union Pacific and Northern Pacific railways for use on their mountain grades, can appreciate this limit. The general adoption of such motive power would require the almost entire reconstruction of the road. As it is, such special locomotives are now and then built for special purposes, and used over small portions of a road, on which the bridges can be built to suit the conditions.

The testing of mild steel under many conditions and its subjection to all kinds of rough treatment, has convinced the writer that it is safe to use such steel with the same processes of manufacture as are in vogue for wrought iron. Unless the steel is of very high tenacity, the precaution of reaming the rivet holes is not needed much more than it is for iron, and indeed the milder grades of steel show less injury by punching than does wrought iron. In either metal the reaming is beneficial.

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The notion that mild structural steel is brittle and like the material used for tools, is being gradually displaced by a better knowledge; and there is a more general recognition of the fact that the structural steel of to-day, produced by the open hearth process, is not properly steel at all, but more or less a homogeneous wrought iron in which the absence of slag and cinder prevents the development of fiber in the processes of rolling.

A thin narrow strip of soft steel with a hole punched in it, can be bent over until the faces are nearly in contact, without cracking through the line of bend which passes through the punched hole. This cannot be done with iron except in extremely rare instances. The time was not long since when it was scarcely thought possible to produce steel with a less percentage of carbon than 0.30 per cent. Now it is easily produced with only 0.12 per cent. or less. The experiments on the injurious effects of punching have been principally made upon this higher steel and very few upon the milder steel of more modern production. More extended acquaintance has brought increased confidence in the capacity of mild steel for structural purposes, and there is now as much steel used in construction with the same process of manipulation as wrought iron, as there is reamed and planed.

It is, however, not advisable to use steel of high ultimate strength without reaming the punched holes. The limit above which reaming should be required should be placed at about 70 000 pounds. Steel of this quality will stand the drifting test of an enlargement of 50 per cent. at least, and the writer knows of steel of 80 000 pounds ultimate strength which has withstood an enlargement of 100 per cent. The drifting test is a valuable adjunct in the way of testing, but it should not be made the final test.

The formulas suggested by Mr. Waddell permit too high an intensity of working stresses in short spans. There seems to be a general agreement in the results obtained from the best specifications to allow about 10 000 pounds per square inch on the lower chord iron eye-bars of spans of from 125 to 150 feet. Tests of full size steel eye-bars do not warrant the assumption of more than 25 per cent. excess of strength over iron eye-bars. This would make the permissible stress in the bottom chord bars of a 150-foot span, if of steel, 12 500 pounds per square inch. Mr. Waddell's formula would permit 14 000 pounds per square inch. Mr. Cooper's specification would permit but 11 500 pounds per square inch on the same bars, and this latter figure is certainly not too low for such short spans, where rigidity is an important consideration, and impact an important factor.

HENRY W. HODGE, Jun. Am. Soc. C. E.—Mr. Waddell's paper will doubtless help to advance the science of bridge designing to a nearer approach to that uniformity which all engineers look forward to, and the suggestions he makes will be most eagerly welcomed by all those

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unfortunates whose duties compel them to make the computations necessary for bridge designs under modern specifications. That wheel concentrations are an unnecessary refinement and a source of much needless labor, all engineers are agreed, but the question is where to draw the line between figuring with wheel concentrations and with uniform load.

In the writer's opinion it is undoubtedly best to use wheel concentrations for stringers, floor beams, long verticals and such secondary members, also for plate girder spans; as any one can readily make a table of exact maximum bending moments for all spans, for any particular engine, in two or three hours, which is good for all time, and consequently requires no labor in practical use. Furthermore, such a table is as easily prepared and more quickly used than Mr. Waddell's table of equivalent uniform loads. Such plate girder spans might be used up to 75 feet, as Mr. Waddell's limit of 90 to 100 feet seems rather long for practice. Such girders become too deep and heavy to transport, besides requiring several covers, and this latter feature is objectionable, as Mr. Waddell points out. Wheel concentrations might also be used for lattice girder spans up to the same or a greater length, say to 90 feet, but for all spans over this length a heavy uniform load with one concentration would seem to answer every purpose.

It seems to be going from one refinement to another to change from wheel concentrations to a uniform load varying not only for every length of span, but also for the chords and webs; and as one of the main objects of any change is to simplify the routine work of bridge computers, let it be simplified, and do not drop one burden to assume another, as Mr. Waddell's scheme of loading would do.

Take a large enough load, say 5 000 pounds per linear foot, with an additional 30 000 pounds concentrated at any point for spans up to 125 feet, and 4 000 pounds per linear foot, and an additional 30 000 pounds concentrated at any point for spans over this length. These loads are now in use by the Southern Pacific Railway and they seem to be heavy enough to cover any further increase in live load that may arise in the near future. Doubtless this simple loading will be objected to on the ground that it is not exact and scientific; but those wishing scientific loading had by all means better use wheel concentrations, as nothing as scientific can be found, and the only object of the change is simplicity, which this loading gives most completely.

Mr. Waddell calls attention to the indirect effect of wind loads, and while such effects would seem to be a very unnecessary refinement, yet if they are to be considered at all, one of the most important ones would doubtless be the increased load on the windward stringers due to the overturning effect on the train, amounting to about 300 pounds per linear foot in ordinary cases; yet Mr. Waddell does not include this in his list of such indirect effects.

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It certainly seems unnecessary to increase the general allowance for wind pressures of 150 pounds per linear foot of upper chords and 450 pounds per linear foot of lower chords, for ordinary spans, or to add one more burden to the computer by making him figure the area of his truss to get some equally arbitrary amount derived from such area. That the present pressures are fully sufficient, seems to be well proven by the fact that there are many old railroad bridges standing to-day, having weathered hundreds of storms, with laterals of one-quarter the strength required by modern practice. While such bridges are not cited as examples of good engineering, they do seem to prove that four times their lateral stability should certainly be safe.

Mr. Waddell's floor system is certainly a good one, but if willing to incur such expense, it would seem better to use a buckle plate or trough section floor, with ties in ballast, which would be very little more expensive if maintenance is considered. As to unit stresses, there certainly seems room for a large amount of simplification in modern specifications, as most of them attempt to split hairs under this head, whereas the true strength of the material is, to a large extent, uncertain. While it would seem that our English fellow-engineers have possibly reached an extreme position in their ordinary specifications of "compression n tons per square inch : tension m tons per square inch," yet a nearer approach to such delightful simplicity would certainly be a boon to bridge designers.

J. A. L. Waddell, M. Am. Soc. C. E.—In beginning this *résumé* of the preceding discussions, I desire to express to the gentlemen, who have contributed thereto, my hearty thanks for the time and attention they have given to the paper and for their valuable additions to the literature of bridge engineering.

Without their contributions my paper would have had but little effect; but with them it bids fair to accomplish a number of much needed reforms, and to effect a simplification of bridge engineering practice, all of which will be clearly set forth at the end of this *résumé*. Although some forty members of the Society have taken part in the discussion, making it probably the most thorough and exhaustive that has ever been given to any of the Society's papers, nevertheless it is to be regretted that the names of half a dozen prominent American bridge engineers are conspicuous by their absence.

In preparing this *résumé*, I shall take up the various subjects treated in the paper one at a time, in the order in which they there appear, and shall make cross-references in respect to the discussions by inserting in parentheses the numbers of the pages where the points treated are to be found, at the risk of spoiling the typographical appearance of my second contribution; for I consider that such cross-referencing will prove to be a great convenience to those who read and study the paper thoroughly.

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Uniform vs. Concentrated Loads.—It is a matter of regret to me that several of the gentlemen discussing have failed to appreciate the true intent of my proposition to substitute equivalent uniformly distributed loads for engine concentrations followed by uniform car loads. If they had not misunderstood, they would surely never have stated that there is less labor involved in either the concentrated wheel load method, or the locomotive excess method, than there is in the method of equivalent uniform loads.

If it be remembered that it is my desire to have in every railway bridge specification a diagram or diagrams of equivalent uniformly distributed loads, similar to those contributed by Messrs. A. C. Stites and Lee Treadwell in their discussion (page 257), together with a table of constants for the end shear formula, or perhaps, preferably, another diagram giving end shears for all plate girder spans, and to have it stated distinctly in the specifications that these equivalent loads are to be used exclusively instead of engine diagrams for all plate girders and trusses of single intersection; I think it will be evident that what I propose will be a great labor saving scheme. Perhaps the misunderstanding is partially my fault, for I may not have made my statements as clear as is desirable.

Let us follow rapidly the steps that it will be necessary for a computer to take in finding the stresses in a single track long span bridge with broken top chords, when using diagrams of equivalent uniform loads similar to those of Messrs. Stites and Treadwell.

First.—In proportioning stringers, find from the diagram the equivalent live load for the panel length adopted, add thereto the weight per foot of two stringers and the track, and divide the sum by two, calling the quotient w; then the moment will be $\frac{1}{8} w l^2$, where l is the panel length.

Second.—To obtain the concentrated load on a floor beam at the point of attachment of stringers, find the equivalent load for a span of $2L$, to it add as before the weight per foot of two stringers and the track, and divide the sum by two, calling the quotient w'; then the concentrated load sought will be $w' L$.

Third.—To find the dead load stresses in trusses, assume the dead load per lineal foot per truss (w''), and compute the panel dead load, and the dead load reaction at each pedestal, then by a single graphical manipulation find the chord and web stresses on one-half of the truss, checking all the work by computing analytically the top chord stress at mid span.

Fourth.—To find the live load stresses on chords and inclined end posts, take from the diagram the equivalent load for the entire span length and divide it by two, calling the quotient w''', then set a slide rule to the ratio $\frac{w'''}{w''}$ and read off from it continuously (by using the

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dead load stresses previously found), all the live load stresses in chords and inclined end posts.

Fifth.—To find the live load stresses in web members, assume a reaction of 10 000 pounds at one pedestal caused by a load (its amount need not be calculated) at the first panel point from the other pedestal, then by a single graphical manipulation ascertain and write down all the web stresses produced thereby from end to end of span. Next calculate the shears R_1, R_2, R_3, and at the head of the train for all positions of the same, using the slide rule. Tables of such shears given in most text-books on bridges will shorten these calculations. Then with the slide rule again find the correct web stresses by the following proportion: as 10 000 pounds is to any one of these shears, so is the corresponding diagonal stress just found to the correct stress.

Now can any imaginable method be simpler, easier or prettier than this? I doubt it. Contrast it with the long and wearisome method by engine diagram, involving as it does for stringers and floor beams the shifting of wheels, calculation of reactions for each wheel load in order to obtain total reactions, and the computing of moments from these reactions and loads; also very often a repetition of the calculations for a different position of wheels. Then when it comes to the trusses, what interminable labor is involved! I have seen a good computer of one of our largest bridge companies calculate the shear on the lower half of a main diagonal, for five different positions of the moving load, in order to obtain the absolutely greatest value. This was before Professor DuBois published his method of ascertaining the position of wheels for maximum shear with inclined top chords, to which reference is made in my paper (page 80), and which Professor DuBois himself is only too glad to acknowledge to be extremely laborious in its application.

For such a bridge as the one assumed, the time required for computing exact maximum stresses is, as stated by Mr. Thacher (page 139), fully ten times as great as that required by the equivalent uniform load method; and for the most simple case of a short span with equal panels and parallel chords, I am convinced that I am understating the case in my paper (page 79) by admitting the ratio of time required for the two methods to be as two is to one.

Now comparing the method of using a uniform car load, constant for all span lengths, headed by a concentrated load, or the more accurate one of using the same car load and two concentrated loads separated by about fifty feet or two ordinary panel lengths, advocated by Messrs. Burr, Swain, Ricketts and others, with that of the equivalent uniform load, we will find that as far as the floor system is concerned there is but little difference in the time and labor for the two methods; but that in respect to the trusses, both of these are more than twice as great for the former method as for the latter. This is evident for the

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reason that, in addition to making all the calculations indicated in items Nos. 4 and 5 in the previous comparison, it is necessary to find the stress in every chord and web member for either one or two panel excess loads, which in itself involves more labor than does the finding of the stresses by equivalent uniform loads. Moreover, I doubt that the method of constant car load, combined with one or two concentrated loads, can be made to give for all cases as correct results compared with the theoretically perfect method, as will that of the equivalent uniform loads.

A proof of this is given by the results of some calculations made by Messrs. Stites and Treadwell, at the time the tables given on pages 84–88 were computed. I had anticipated using for web members a uniform car load with two engine excesses until I saw the results of the calculations just mentioned. The following table gives the shears in web members by the engine excess method, and a comparison of them with those computed by the theoretically perfect method, also a comparison of the percentages of error for this case and that of the

Span.	Member.	Shear in Pounds.			Percentage of Error.	
		By Diagram	By Uniform Load.	By Uniform Car Load with two engine excesses.	By Uniform Load.	By Uniform Car Load with two Engine Excesses.
150-foot	End Post	205 285	200 745	208 833	2.21 Danger	1.73 Safety
	1st M. Diagonal	134 585	133 830	141 000	0.56 Danger	4.76 Safety
	2d M. Diagonal	78 142	80 298	85 667	2.76 Safety	9.63 Safety
	1st Counter	36 576	40 149	42 833	9.77 Safety	17.11 Safety
200-foot	End Post	281 874	275 716	286 500	2.18 Danger	1.64 Safety
	1st M. Diagonal	209 992	206 787	216 875	1.53 Danger	3.28 Safety
	2d M. Diagonal	148 784	147 705	156 625	0.72 Danger	5.27 Safety
	3d M. Diagonal	97 484	98 470	105 750	1.01 Safety	7.45 Safety
	1st Counter	55 768	59 082	64 250	5.94 Safety	15.21 Safety
	2d Counter	26 220	29 541	32 125	12.66 Safety	22.52 Safety
250-foot	End Post	357 668	349 875	363 100	2.18 Danger	1.52 Safety
	1st M. Diagonal	285 278	279 900	292 400	1.88 Danger	2.50 Safety
	2d M. Diagonal	220 844	217 700	229 200	1.42 Danger	3.78 Safety
	3d M. Diagonal	164 804	163 275	173 500	0.93 Danger	5.27 Safety
	4th M. Diagonal	116 264	116 625	125 300	0.31 Safety	7.78 Safety
	1st Counter	75 887	77 750	84 600	2.45 Safety	11.48 Safety
	2d Counter	43 645	46 650	51 400	6.85 Safety	17.80 Safety
300-foot	End Post	433 260	424 182	439 167	2.10 Danger	1.36 Safety
	1st M. Diagonal	360 438	353 485	367 750	1.93 Danger	2.03 Safety
	2d M. Diagonal	293 893	289 215	302 583	1.59 Danger	2.96 Safety
	3d M. Diagonal	234 684	231 372	243 667	1.41 Danger	3.82 Safety
	4th M. Diagonal	181 734	179 956	191 000	0.97 Danger	5.02 Safety
	5th M. Diagonal	135 034	134 967	144 583	0.05 Danger	7.08 Safety
	1st Counter	95 206	96 405	104 417	1.26 Safety	9.68 Safety
	2d Counter	62 431	64 270	70 500	2.95 Safety	12.95 Safety

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equivalent uniform load method, omitting from consideration the four panel 100-foot span, which really should never have been incorporated therein because of several objectionable features, more especially its unsightly appearance. The only reason for incorporating it was to fill out the table; for every 100-foot span should have at least five panels.

By comparing the last two columns it will be seen that the locomotive excess method for web members always gives an error on the side of safety, and that these errors are much larger than those given by the equivalent uniform loads. Taking the average of said errors we find that for the locomotive excess method to be 7.34 per cent. on the side of safety, while that for the equivalent uniform load method is a trifle less than 1 per cent., also on the side of safety.

Time will not permit me to compute the errors in chord stresses by the use of the locomotive excess method; but I feel quite certain that the results would show, as in the case of the diagonals, the errors all to be upon the side of safety, but by no means as great as those for said diagonals. Mr. Treadwell has figured by the locomotive excess method the moments for the 200-foot span, which can be taken as an average case, and finds that the greatest error is 4.64 per cent. and the least 1.68 per cent., all errors being on the side of safety, and the average error being 3.53 per cent. For the equivalent uniform load the greatest errors are 1.45 per cent. on the side of safety and 2.14 per cent. on the side of danger, the average being 0.16 per cent. on the side of danger.

The large errors in chord stresses by the locomotive excess method are due partially to the fact that in this method cars precede as well as follow the locomotives, while in the engine diagram method there are no cars ahead of the engines. In my opinion, as cars ahead of engines are an unusual load, and when they are found there, the chances are that some of the cars are not loaded up to the limit of the specifications, it is well to ignore such unusual loading.

Mr. Treadwell finds that with Cooper's Class A as a standard, the engine excess in order to give exact agreement for plate girder spans, should vary from 11 300 pounds to 23 600 pounds, the average being 20 000 pounds. Adopting the latter, the greatest percentages of error would be 5.24 danger and 5.39 safety. For floor beams he finds the average concentration to be 20 700 pounds, with extreme errors of 4.12 per cent. danger and 4.11 per cent. safety. But if 20 000 pounds be adopted as the proper concentration, the last mentioned errors would be 5.16 per cent. and 3.58 per cent. respectively. These figures show very plainly that the method of using a constant uniform car load with two engine excesses, as far as the trusses are concerned, cannot be made to give stresses approximating to the theoretically exact stresses as closely as can the method of equivalent uniform loads.

Again, it is evident that a single concentration at the head of a train will not give as close an agreement as will two concentrations separated

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by two ordinary average panel lengths, for the latter method distributes the load more nearly in accordance with the actual distribution. It is true that the concentration at the head of the train, need not equal the difference between the total weight of two locomotives with their tenders and the weight found by multiplying the extreme length of said two locomotives and tenders, coupled, by the car load per linear foot, although such a method would be considered by most engineers to be the proper one; but it is more than likely that, if the single concentration were adjusted so as to give for all truss members the best average in respect to theoretical correctness, the errors would be greater than those found by using equivalent uniform loads, and that the errors for the floor system would be still greater.

My opinion of the single-concentration-with-constant-car-load system is, that it could be used advantageously as a standard, if deviations on the side of safety of considerable amount be not deemed objectionable; but that, to replace any particular standard load of two engines followed by a constant car load, it is inapplicable. Certainly, as far as simplicity and saving of labor are concerned, it is much preferable to the engine diagram system. But why adopt it when there is another method that is about twice as simple of application and gives results agreeing much more closely with the theoretically correct ones?

An objection that may be raised to my proposed equivalent uniform loads is, that chief engineers and bridge engineers of railroads will not adopt them; but it is hardly fair to assume that these gentlemen are unreasonable and unwilling because of a little extra trouble, to concede a point that is evidently for the general good. I purpose making it my business to communicate in the near future with all of these gentlemen whose names are given in Poor's Manual, so as to obtain their views on this and one or two other points.

I would say here that the preparation of diagrams giving equivalent uniformly distributed loads for all spans—and end shears on plate girders, to correspond to any proposed standard train load—involves by no means a great amount of labor, and that they can be furnished by any expert bridge engineer for a small fee. One who has made a number of such diagrams, can easily make others by finding a few points on the new curve and using a previously found curve for a similar load as a guide. Where the plotted points show slight irregularities, it is much more scientific to draw a curve of some regularity without deviating from them essentially than it would be to follow the variations; for the latter are due to peculiar relations between the span lengths and the wheel spacings, which would not exist for slightly different engines. I have called attention to this in the paper, but do so again here on account of its importance.

Answering Mr. Thomas H. Johnson (page 113), I would suggest that it is not quite fair to condemn the equivalent load method because

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the shear on a counter for a 100-foot span through bridge having only four panels (a structure which, as before stated, should never be built, and which was employed in the table merely for the sake of continuity) involves an error of 20 per cent. (*vide* page 88). If we discard the 100-foot span from the tables, we find that the errors for counter stresses vary from 1.26 per cent. safety to 12.66 per cent. safety, the greater errors being always in the smaller counters. Are not such errors in the method an excellent fault? What practical engineer is there who proportions light counters for the stresses shown on the strain sheet, even in competition?

If we leave out of consideration the counters as well as the entire 100-foot span, we find that the greatest errors in chords and diagonals are 2.21 per cent. danger and 2.76 per cent. safety, the averages being approximately 1.8 per cent. danger and 1.2 per cent. safety, and the average for all members of all structures only 0.3 per cent. danger. Surely these errors are small enough in all conscience, and be it remembered that they cover all stresses in main members (counters excepted), for all spans from 150 to 300 feet.

Still replying to the same gentleman, I would state that the shear formula or diagram for plate girder spans will rarely be used by computers, who always endeavor to have an excess of section in web plates. It is only in the case of shallow girders, or middle girders for double track structures, that the webs need to be tested for shear.

Professor Eddy writes (page 138) as follows: "But in short spans, and in stringers and other details of long spans, where the wheel concentrations exert a more preponderating influence, I am led to believe that it will still continue to be required that the designer and computer shall take account of the wheel concentrations, however much he may dislike the work, or however certain he may be in his own mind that it involves unnecessary labor." If Professor Eddy will study the equivalent load system, he will find that for stringers, floor beams, etc., it gives results which are practically exact (excepting the shear formulas or diagrams that, as before stated, need hardly ever be used), and that the errors involved occur in the trusses.

Mr. Fulton's method (page 199) of plotting the loads and spans in order to find actual concentrations at panel points is antiquated and extremely laborious, and is justifiable only in case of a double system of cancellation.

Mr. Douglas' calculations (page 205) do not militate at all against the equivalent uniform load method, as said method is not applicable to trusses having multiple systems of cancellation. It merely affords another corroboration of my objection to the Whipple truss as being uneconomical as well as unscientific. Where two heavy concentrations are thrown upon one system, as in this case, there is evidently a want of economy in the design.

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Mr. Hodge (page 262) does not understand the proposed equivalent uniform load method, for it is really correct for "stringers, floor beams, long verticals, and such secondary members; also for plate girder spans." By adopting it the engineer, who prepares the specifications, in furnishing equivalent uniform load diagrams saves for bridge computers for all time to come, the useless labor involved in dealing with wheel loads.

Mr. Hodge's proposed moment table is no more easily used than my diagram; for he has to compute the dead load moment by the formula $M = \frac{1}{8} w' L^2$, and add it to the moment given by his diagram; while I add the equivalent load w given by my diagram to the dead load w' making the sum $= w''$, and obtain the moment by the equation $M = \frac{1}{8} w'' L^2$. Mr. Hodge is mistaken in stating that the equivalent uniform load must vary for chords and webs; for I give but one equivalent load for each span length.

I would like to call the reader's attention to Prof. Du Bois' pointed and forcible remarks upon the subject of uniform vs. concentrated loads (pages 154–156).

Of the forty-two gentlemen who have contributed to the discussion of this paper, four have not touched at all upon the subject of live loads, and seven more have expressed no decided preference for any of the three systems. Of the remaining thirty-one, sixteen are in favor of the equivalent uniform load system, eight are in favor of the locomotive excess system, and seven are in favor of the engine diagram system. It is fair to assume, therefore, that the engineering profession is tired of the engine diagram system, and is prepared to adopt either the equivalent uniform load system, or the locomotive excess system, the preference being decidedly in favor of the former.

Now, I am going to ask all of the forty-two contributors to the discussion, to be so kind as, after reading carefully all that has been said upon the subject, to vote upon the question as to which of the three methods they finally favor, and to this end I shall ere long communicate with each of them.

To the gentlemen who have advocated the locomotive excess method I would say that all of the rather limited calculations which I have been able to make, induce me to conclude that it is impracticable to find such a leading concentrated load as will give results for both floor system and trusses, which will agree at all closely with the theoretically correct results obtained by the engine diagram method; and that the equivalent uniform load method does give such an agreement close enough for all practical purposes, provided that multiple systems of cancellation be not used. The *onus* is now on them to show that I am mistaken, and that the locomotive excess method does give an agreement with theory that is close enough for all practical purposes.

Be it remembered, though, that I do not claim such agreement to be

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essential, for a uniform car load preceded by a single concentrated load can be used as a standard instead of a uniform car load preceded by two engines. I think, however, that most railroad engineers would prefer to adhere to the latter as a standard, and use instead the equivalent uniform loads for all ordinary cases, rather than to adopt a load that does differ considerably in its effects from those loads to which bridges are apparently subjected. I hope that the gentlemen who have advocated the locomotive excess method, and perhaps even some of those who advocated the engine diagram method, will conclude to vote for the equivalent uniform load method after reading my explanations as to what that method really is, and what a great amount of labor it saves.

There is surely no valid objection to a man's changing his opinion; and I think that the remainder of this *résumé* will show that I am quite ready to change mine when either proven in the wrong, or when something better than mine is offered. To begin with, in respect to the theory of floor-beam concentrations, I am quite ready to cry *peccavi*, the more so as my friend Professor Du Bois lets me down so gently; but, although an acknowledged "sinner," I am by no means a repentant one, for I am well pleased to have made the oversight, because of the attention that has been called to an important fact which was not generally known by the engineering profession.

The Proper Live Loads for Modern Bridge Specifications.—Answering Mr. Thomas H. Johnson (page 113), I would state that for roads with comparatively light grades the growths of weights of engines and of cars appear to have kept pace with each other fairly well; but for mountain roads the engine weight is greater in proportion to the car weight than my proposed standards give, hence said standards would not apply to mountain lines. I fear that the general managers of railroads throughout the United States would at present, and with good reason, object seriously to paying for bridges designed for a car load of 4 000 pounds per linear foot, excepting those on the great trunk lines. My proposed standards give railroad men a choice of loads, and if none of them suit, let their engineers adopt others, but furnish with their specifications diagrams of equivalent uniform loads similar to those given by Messrs. Stites and Treadwell in their discussion. At the same time I believe it would be advisable to modify my standards so as to include loads for mountain lines, but reducing the total number of standards to a minimum.

It is my intention, when communicating with the chief engineers and bridge engineers of railroads, as mentioned in the beginning of this *résumé*, to consult with them as to what standards they desire, so as to endeavor to prepare a set of standards that will satisfy the large majority of railroad engineers.

On account of certain statements made in several of the discussions,

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I desire to adopt the following loads concentrated on two pairs of wheels spaced seven feet centers, to be used only for stringers, floor beams and primary truss members with their connecting details, when the panel lengths are less than 15 feet, and to incorporate the results in the diagrams of equivalent uniform loads and of end shears in plate girders.

Class Z	75 000 pounds.
Class Y	80 000 "
Class X	85 000 "
Class W	90 000 "
Class V	95 000 "
Class U	100 000 "

Amounts of Wind Pressure.—I endorse heartily Mr. Lindenthal's remarks (page 126) to the effect "that the lateral bracing of the bridge is more often and more severely strained by the lateral blows from swiftly moving engines and cars, causing lateral vibration, than from the wind itself," and would suggest that if the series of tests of actual intensities of working stresses hereinafter proposed be carried out, said effects of live loads on lower lateral systems be determined.

Answering Mr. Bouscaren (page 176), a pressure of 50 pounds per square foot on the empty structure is, as shown by Mr. Thacher (page 142), sufficient to buckle the bottom chords of nearly any single track pin-connected bridge in this country. It is much better to make the specified wind pressure low and proportion properly, letting the structures take their chance of being struck by a cyclone, rather than to adopt a wind pressure that would necessitate stiff bottom chords for most bridges without giving any assurance of safety in said cyclone.

Mr. Osborn (page 213) is right in stating that it is a nuisance to have to figure the wind pressure on bridges, and that some simple formula ought to be provided. Mr. F. H. Lewis, in a most valuable and interesting paper on "Soft Steel in Bridges," presented to the Engineers' Club of Philadelphia, gives the following: "The bottom lateral bracing in deck bridges and the top lateral bracing in through bridges, must be proportioned to resist a uniformly distributed lateral force of 150 pounds per linear foot of bridge for all spans of 200 feet and under, and an addition of 10 pounds per linear foot for every 25 feet increase in length of span over 200 feet." "The bottom lateral bracing in through bridges and the top lateral bracing in deck bridges must be proportioned to resist a uniformly distributed force the same as above, and an additional force of 300 pounds per linear foot of bridge, which will be treated as a moving load." Provided there be no screens or hand-rails, the specification quoted will answer, but otherwise I would prefer to adhere to the method given in my paper (page 91).

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Answering Mr. Skinner (page 219), I would state that it is not customary to consider impact in wind loads, for the reason that the latter as assumed are of rare occurrence, and are ample in every respect.

Effects of Wind Pressure.—Referring to Mr. Douglas' remark (page 206), viz., "It does not seem necessary for the railroad company to tell the contractor how to compute the stresses. The latter is supposed to know that "—I would reply that such is exactly what the contractor does need to be told, and in most cases what he wants to be told, as is evidenced by Mr. Cowles' remarks on pages 164 and 165.

Replying to Mr. Hodge (page 262), my reason for not considering the transferred load (amounting, as he states, to 300 pounds per linear foot) on the leeward stringer is because such a load is rare, and therefore its effect should not be considered unless the amount thereof exceed say 25 per cent. of the live and dead load, which it does not. To be perfectly consistent, however, one should take into account the load transferred to the leeward truss by the wind pressure upon the train in finding the truly greatest stress that can come upon the leeward bottom chord. For single track bridges it will amount to no more than 200 pounds per linear foot, while the transferred load from wind pressure on trusses varies from 300 pounds to 700 pounds per linear foot. This difference between the two transferred loads, however, must be noted: the former acts simply as an increase to the dead load, while the latter causes an increased reaction on the leeward pedestals and a constant stress from end to end of bottom chord, as explained on pages 91 and 92. Decidedly, the transferred load on the leeward bottom chord from wind pressure on a train should not be ignored.

Styles and Proportions of Bridges.—Replying to Mr. Thomas H. Johnson (page 114) and Mr. Snow (pages 157 and 158) on the subject of pony truss bridges, I would state that while I would not condemn an existing structure merely because it is a pony truss bridge, still I would be quite suspicious of it, and in no case would I design one, unless, perhaps, it be of the style mentioned by Mr. Swift on page 222. The general opinion of engineers concerning pony trusses is that their construction should be relegated to the past.

The limiting length of plate girder spans appears to be still a disputed point; and it is evident that railroad and bridge engineers desire to make it as great as practicable, while manufacturers (who have all the trouble of loading, shipping and erecting) prefer to keep it down to about 70 or 80 feet. Mr. Snow's deck plate girder bridge described on page 158 may be a very good one, but it is unnecessarily expensive, as would be discovered by the general manager of any line of road who would try the experiment of building such structures.

Answering Mr. Thos. H. Johnson (page 114) and others, my reasons for using sub-struts instead of sub-ties in divided panels are:

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First.—The former carry the loads more directly to the piers, and

Second.—As they have fully twice the area given to the sub-ties, they permit of only one-half of the deflection of the top chord at the middle of the long panel. As for ambiguity of stress in the counter-braced panels, I would state that it is my custom to proportion all members in and about these panels so that the stress can travel by either the sub-strut or the counter; and I contend that this is good practice, for the reason that the members in such places are generally light enough, in all conscience.

In answer to Mr. Swift's remark on pages 221 and 222 concerning thickness of connecting plates in lattice girders, I would state that it is my latest practice to make such plates comparatively thin and large, and to use an apparently excessive number of rivets, connecting both legs of every web angle iron to the plate, and employing, when practicable, the built star section for all diagonals and verticals.

Prof. Swain (page 120) advocates the use of suspended floor beams. In certain cases of very shallow floors they would be advantageous, provided they be properly stayed, and that plate hangers be used. These, however, necessitate pins of large diameter, which may be objectionable in small bridges. As a shallow floor is a feature in bridge designing that it is desirable to avoid when possible, we may conclude that suspended beams should be used in special cases only.

Much to my regret, but little has been said about riveting floor beams to posts. The detail, although it may not be theoretically scientific, is, nevertheless, a good one for beams of ordinary depth. Should in the future the top connecting rivets wear loose, they can be readily replaced, and no harm will be done, provided there always remain an ample number of rivets to take up the shear.

But little has been said about the advisability of increasing clear roadways so as to provide 8 feet between the center line of the nearer track and the innermost portion of the truss. A number of well known railroad engineers have lately expressed to me their approval of the proposed change.

Mr. Buck, on page 134, writes thus: "But there is no economy in attempting to make the bar act as a beam in sustaining its own weight. The nearer it can come to assuming the catenary due to its own weight and the tensile stress upon it, without acting as a beam, the better." I am glad that Mr. Buck has taken this stand, for the point is one that needs attention, and I hope it will soon receive it at the hands of bridge engineers. In my opinion, the idea advanced is erroneous, and I am strongly opposed to the use of long, light, shallow eye-bars in bridges, if for no other reason than because of their sag (which is often apparent to the eye) and their vibration.

Mr. Buck states, on the same page, that it is apparently impossible, where eye-bars are used for bottom chords, to have the axes of the

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lateral diagonals pass through the intersections of the axes of posts and chords. Mr. Skinner (page 220) appears to agree with Mr. Buck in this opinion. Time and space do not permit me to describe here the method of accomplishing this desideratum; but I have in my office drawings for the Pacific Short Line Bridge, which is to cross the Missouri River at Sioux City, Iowa, in which the "focusing of all lines of strain at an absolute point at the center of a main truss lower chord connection" is effected perfectly.

In my opinion, the results given by Mr. Osborn (page 213) concerning stresses in eye-bars because of their own weight, are not satisfactory, for the reason that he has ignored the reverse bending moment, which is equal to the direct pull multiplied by the deflection. Professor Johnson (pages 136–138) takes this into account, but at the same time makes an assumption which I do not think is warranted, viz., that the deflection v is the same when there is direct tension on the bar as it would be if there were none. My method of settling this point would be to place a number of bars of various lengths and sizes in a testing machine, subject them to various pulls (P), and to measure with great accuracy the actual values of the deflection (v), then substitute for P and v in Professor Johnson's equation 3 (page 137) and solve for f. My table of limiting sizes of eye-bars (page 96) was determined partially by some rather crude calculations, and partially by practical experience and judgment.

There seems to be considerable difference of opinion in respect to the proper spacing of stringers, which would be hard to reconcile; but there is nothing in the discussion to cause me to change my opinion that the best distance between stringers is 8 feet. Perhaps the ultimate solution of the question will be the entire abandonment of wooden floors and the substitution of corrugated metal with ballast thereon, in which case the distance between stringers would be dependent upon the size and strength of the corrugated plate. The principal objections to this style of floor are its liability to rust and its excessive weight. The latter would apply principally to long spans.

Answering Mr. Nichols' question (page 227) concerning the spacing of guard rails in my proposed floor system, the distance from the center of track to the center of outer guard rail is 4 feet, leaving about 14 inches clear between the head of the rail and the outer guard, and the corresponding clearance for the inner guard is 7 inches. The question as to whether pine or oak should be used for ties and guards is still unsettled, but the general disposition seems to be to favor yellow pine.

I am glad to have my remarks on the exclusive use of steel for bridges endorsed by so high an authority as Mr. Burr, although Mr. Thacher, another high authority, differs with us both on several important points. Mr. Lewis' paper on "Soft Steel in Bridges," before referred to, throws a good deal of light upon the subject. It is to be

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hoped that the discussion of this important matter will be continued, although if it is not, the effect in my opinion will be only a very short delay in the abandonment of wrought iron for bridges. I am in favor of using medium steel for all parts of bridges, excepting for rivets, for which I would use soft steel. Answering Messrs. Gates and J. M. Johnson, I would state that I failed to specify the grade of steel for two reasons; first, medium steel was understood, as that is what nearly all bridge builders are now using; and, second, my paper was not intended (and, in fact, was not permitted) to be a specification.

Intensities of Working Stresses.—In respect to this subject it appears that we are all at sea; and we are liable to remain there until such time as the much needed experiments on actual intensities of working stresses, that I have been advocating for years, be made, after which we shall be able to determine upon a system of intensities that will be logical. Meanwhile we shall have to jog along in the best way that we can, letting each engineer use his own judgment concerning the intensities to employ, or perhaps (which is the best thing to be done under the circumstances) obtaining a consensus of opinion as to what system to adopt for a temporary expedient. The method adopted by Mr. Schneider of reducing all live load stresses to their equivalent static stresses before applying a constant intensity, is undoubtedly the scientific way to proportion bridges; but until we have some real knowledge of the effects of dynamically applied loads, it does not seem advisable to develop a system that in all probability will have to be considerably modified in the future.

It would not be such an immense undertaking to make an exhaustive series of tests of the effects of live loads applied to bridge members with varying velocities. Perhaps a year's time and an expenditure of say $50 000 would suffice. If not, more time and money should not be begrudged upon such an important matter. The United States Government is willing to appropriate annually millions of dollars for the United States Engineers to experiment with upon hydraulic problems, so why cannot the bridge engineers and the railroads obtain from Congress a small appropriation to decide one of the most vital questions in bridge building? If the United States Government refuse to make such an appropriation, cannot one of America's millionaires be persuaded to donate the money as a contribution to applied science?

In my opinion the proper steps to take after obtaining the money would be as follows:

First.—To appoint a committee of seven members of the American Society of Civil Engineers, who are acknowledged bridge experts, to act as an advisory board, and let them lay out the series of tests (to be modified later if thought by them advisable), appoint a committee of three well paid expert bridge engineers to make the tests under their instructions, attend to all payments of money, make arrangements

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with railroad companies for the use of their lines and bridges in making the tests, etc.

The first practical step to take would be to investigate all the machines thus far invented for measuring extensions and compressions in bridge members, so as to decide upon what kind of apparatus to adopt, designing new ones if necessary. These machines should be tested thoroughly to determine their accuracy as far as static loads are concerned, and to prove their reliability in case of dynamically applied loads. After the machines are shown to be satisfactory, experiments should be begun systematically upon all parts of bridges of modern design, for trains varying in velocity from zero to the greatest attainable speed. Sufficient tests of all kinds should be made to give good average results. Both tension and compression members should be experimented upon, and if the machines prove to be very accurate, even such intricate problems as the distribution of stress in plate girders might be solved. This field of experiment is a most inviting one, especially because of the great utility of the results; so there would be no difficulty in obtaining an expert committee to make the tests. Mr. Woelfel's remarks on the subject of measuring the actual intensities of working stresses (pages 167 and 168) are most interesting, and are worthy of a careful perusal.

Next to the series of tests just described, the most needed experiments are some on steel, especially in full-size compression members of all kinds. A systematic effort on the part of the members of the American Society of Civil Engineers ought to secure this also. If we had the results of such a series of tests, we would be able to make diagrams giving working intensities for all kinds of compression members, and thus avoid all difficulty due to numerous and varied formulas. Now on account of these reasons I do not consider it either advisable or in any way beneficial to discuss theoretically the proper intensities of working stresses or column formulas, so I shall take no further notice of several elaborate mathematical dissertations that are to be found in the discussions.

Mr. Thos. H. Johnson (page 114) objects for æsthetic reasons to the use of stiff bottom chords near the ends of spans. Such struts in most single track bridges are a necessity, hence "a due regard for the eternal fitness of things" would cause their adoption, even if "the change from eye-bar to the built form is an offense to the eye." It is not good engineering to sacrifice excellence to mere appearance, and I do not think that any portion of a design which gives *prima facie* evidence of its use can be cause of offense to the eye of an engineer.

Mr. Johnson (page 115) objects to my top chord formula, stating that for $\frac{L}{r} = 50$ it is 27 per cent. too high, and at $\frac{L}{r} = 120$ it is 16 per cent. too low. He says that the formula is utterly without support in

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reason or experience, and is a purely arbitrary assumption in every particular. That it is an "arbitrary assumption" I grant, but in respect to the other assertions I beg to differ with the gentleman. For values of $\frac{L}{r}$ not exceeding 50, it is customary with some engineers to strain steel compression members a certain fixed amount based upon the corresponding intensity used for tension members. Mr. Bouscaren, whose standing as a bridge engineer neither Mr. Johnson nor any one else will be disposed to question, strains short compression members 12 000 pounds per square inch, where he strains the corresponding tension members 14 000 pounds per square inch. My intensities for compression and tension are in the ratio of 11 000 pounds to 12 000 pounds, which is different, it is true, from Mr. Bouscaren's ratio, but not so very different. As for my formula giving results 16 per cent. too low for cases where $\frac{L}{r} = 120$, I wonder why Mr. Johnson did not assume $\frac{L}{r} = 500$ in making his comparison! Who ever heard of $\frac{L}{r} = 120$ for the top chord of a railroad bridge? It scarcely ever exceeds 65 in the lightest structures, and even in inclined end posts it goes no higher than 85. The fact that my formula discourages the use of large values of $\frac{L}{r}$ is a point in its favor. Moreover, until we know more about the resistance of steel columns, it is probably as good as any other, and is just as likely to be correct.

Answering Prof. Swain (page 120), I would call his attention to the fact that I use the form of the fatigue formula (page 99) merely as a compromise until something better and more reliable can be established. I agree with Prof. Swain that the sudden change in intensity for flanges of plate girder spans at a length of 20 feet is not scientific; but I adhered to it, first because it is to a certain extent customary, and secondly because it tends to discourage the use of short panels. Prof. Swain is in error when he states as follows (page 121): "And surely the allowable stress for chords in a truss 500 feet long could be greater than for chords in a truss 100 feet long on account of the difference in impact, *although by Mr. Waddell's formulas the same would be allowed in both cases.*" (The italics are mine.) He must have forgotten that the dead load per foot for a 500-foot span is much greater than that for a 100-foot span, while the live load per foot in the former is less than that in the latter. For these two reasons $\frac{\text{min. stress}}{\text{max. stress}}$ for the 500-foot span is much greater than it is for the 100-foot span. Referring to some of my office records, I find the following:

For a 100-foot span, dead load = 1 200 pounds.
live load = 3 300 pounds.

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$$\therefore \frac{\text{Min. stress}}{\text{Max. stress}} = \frac{1\,200}{4\,500} = 0.226.$$

For a 500-foot span, dead load = 4 000 pounds.
live load = 3 500 pounds.

$$\therefore \frac{\text{Min. stress}}{\text{Max. stress}} = \frac{4\,000}{7\,500} = 0.533.$$

The corresponding intensities for bottom chords are 13 356 pounds for the 100-foot span and 15 198 pounds for the 500-foot span.

Respecting Mr. Bland's remarks upon the New York Elevated structure (pp. 248 and 249), is it not evident to any bridge engineer who rides over some of the older lines that they are wretchedly designed, especially in details? This is probably the reason that they begin to show signs of failure. One of my favorite axioms, viz., that "two rivets do not make a connection," appears to be violated constantly in these structures.

I desire to correct a small error which, in spite of all care, crept into my paper. On page 103 the formula for lateral struts should read

$$p = 17\,000 - 65\,\frac{l}{r}$$

instead of

$$p = 17\,000 - 55\,\frac{l}{r}.$$

Combined Stresses.—Concerning the fixedness of inclined end posts at pedestals there appears to be a difference of opinion, and there is reason on both sides. I am ready to concede that with a heavy end floor beam rigidly attached to the inclined end posts, the conditions are much better than those for the assumed free ends; but, on the other hand, the end floor beams are not very thoroughly riveted to the inclined end posts, there is play in the pin hole, and the anchor bolts do not hold down the expansion pedestal very firmly. Perhaps after all I do not differ in effect essentially from Messrs. Burr and Swain, because, while I use a long lever arm in finding the bending moment, I strain the metal on the extreme fiber very high; and they use a short lever arm and probably strain the metal much lower.

Answering Mr. Bouscaren (p. 183), my reasons for straining metal in inclined end posts so high are as follows:

1st. My assumed lever arm is really greater than necessary in a well designed bridge, although I believe with Mr. Bouscaren that it is never twice too great. 2d. The great intensity is on the extreme fiber only, while the average over the whole section does not greatly exceed that allowed for live and dead loads. 3d. The member under such an intensity is essentially a beam rather than a strut, consequently it can be strained higher. 4th. The wind loads assumed are greatly in excess of those that are ever likely to come upon the structure. 5th. The

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combination of greatest live load and greatest wind pressure is highly improbable. 6th. Even if the extreme fiber stress should ever reach the limit, no harm would be done, although it would not be advisable to make a practice of straining metal to such an extent.

Time and space will not permit me to give Mr. Skinner the proof of how the addition of 17 square inches to the section of a bridge member can "increase the working strains so enormously as 18 per cent.," but if he will investigate mathematically for himself the two cases following, I think he will become convinced of its possibility.

1st. Take an eye-bar 1 x 6 inches, with pin holes on axis, and strain it 60 000 pounds. 2d. Add at one edge of said eye-bar, metal 1 x 6 inches, thus forming a 1 x 12-inch bar, and apply the same stress of 60 000 pounds, but do not change the position of the eyes. 3d. Compare the extreme fiber stresses in the two cases. Mr. Fulton's remarks concerning wind stresses (p. 203) indicate that he is not in accord with modern American practice, for surely it is proper to strain metal higher for a combined live, dead and wind load than for a combined live and dead load.

Plate Girder Proportioning.—Answering Prof. Swain (p. 123), my practice is to use at least four vertical rows of rivets, spaced three inches, with two three-eighths x 12-inch splice plates at each web splice, and in addition for each flange two long narrow plates, one on each side of the vertical legs of the flange angles, to compensate for the divided web at the place where compensation is most needed for bending.

Mr. Snow (page 160) desires my reason for the following statement: "Of course if the web be counted in, the intensities of working stresses of the flanges will have to be decreased accordingly." It ought to be evident that if we design a plate girder for a flange stress of 10 000 pounds per square inch, ignoring the aid of the web in resisting bending, and consider it to be properly designed, it would not be correct to redesign the girder by a formula that provides for relying upon the assistance of the web in bending, without making the working intensity less than 10 000 pounds. Otherwise we would find a smaller flange area than was found for the first design. This is a point that has often been taken advantage of in competition.

The preceding explanation will answer also Mr. Hutton's first remark (page 161). In answer to his second, I would state that relying upon the web to resist bending, does not militate against its resisting the shear. In answer to his fourth remark (page 162), I would urge that I have not been "a partisan of the flange-only-for-bending theory in its most extreme applications" (even so far back as ten years ago), as my work on "The Designing of Ordinary Iron Highway Bridges" will testify; but have conceded of late years to general custom, knowing that the results of the two methods do not differ essentially, when

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the formulas are properly adjusted. Mr. Hutton evidently has in mind a decision of mine respecting an existing structure supposed to have been designed in strict accordance with certain standard specifications that ignore the resistance of the web for bending. Of course I had to be governed by said specifications in making my report.

In reference to Mr. Douglas' remarks (page 206) concerning the moment of inertia formula for plate girders, I willingly concede its greater exactness; but for all ordinary cases the approximate method will give sufficiently accurate results, with the saving of considerable time in computing. Such a girder as Mr. Douglas instances is abnormal, although circumstances may occasionally call for its use. Economic considerations require that the weight of the flanges should be about equal to that of the web and its details, and a girder designed according to this rule will always give a depth sufficient for stiffness. For long spans other considerations than economy control the depth. It might be well to specify that whenever the area of the flanges exceeds that of the web by more than 50 per cent. the exact formula be used, and in other cases the approximate one.

I am glad that Mr. Osborn has called attention (page 212) to Mr. Thacher's neat and expeditious method of computing lengths for cover plates by means of the slide rule; for I am a firm believer in the use of that instrument as well as of all other labor saving devices.

General Details of Construction.—Mr. Gayler (page 124) is right in placing the minimum thickness of metal in railway bridges at three-eighths of an inch; and in my opinion it is only a desire to comply with the wishes of those who pay for the structures, which prevents engineers from adopting this limit.

In regard to the reaming of steel, I am quite ready to be convinced of my error by a general consensus of opinion; mine on this point, I must acknowledge, is mostly second-hand. It is evident that reaming is a good precaution; but the question is, "Is it worth the cost?" If it be decided that medium steel should be reamed, I think that wrought iron also should be required to receive the same treatment. Mr. F. H. Lewis' paper on "Soft Steel in Bridges" sheds much light on this subject; but I think that a further discussion thereon by those who are most competent to give an opinion would be of great value.

An extended series of experiments is needed to determine the proper lengths for stay plates. I, as well as all the bridge engineers with whom I have discussed the subject, believe that Mr. Bouscaren's requirements (page 185) are excessive, especially in large bridges; but upon the subject of latticing I agree with the remarks made by that gentleman.

Conclusion.—In bringing this *résumé* to a close, I desire to enumerate the results which, I hope, will be accomplished by this paper and the discussions thereon.

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First.—The abandonment of the engine diagram or concentrated load method of computing stresses, except for unusual cases such as multiple systems of cancellation; and the adoption of the method of equivalent uniform loads.

Second.—The adoption of certain standard engine and train loads.

Third.—An exhaustive set of experiments to ascertain the dynamic effects of live loads applied at various speeds on bridge members of all kinds.

Fourth.—An elaborate series of tests of full-size members of steel bridges, especially compression members of all kinds.

Fifth.—Ultimately, the adoption by the profession of standard specifications for bridge designing, which shall specify clearly and concisely in every particular such important matters as loads, intensities of working stresses, quality of materials, workmanship, etc., but at the same time shall not infringe upon the individuality of the designer.

To the attainment of these ends, it is my intention to devote all the time which I can spare from my practice; and I herewith invite the co-operation of all American bridge and railroad engineers, fully recognizing the fact that one engineer unaided can accomplish practically nothing, but, with the hearty assistance of his professional brethren, there is nothing in reason that cannot be effected. Finally I would suggest that further discussions of this paper with its discussions and the *résumé* are in order, and that they can be published in later issues of the *Transactions* of the American Society of Civil Engineers.

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TRANSACTIONS

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AMERICAN SOCIETY OF CIVIL ENGINEERS.

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No. 523.

(Vol. XXVI.—April, 1892.)

AVAILABILITY OF THE CAÑONS OF THE COLORADO RIVER OF THE WEST FOR RAILWAY PURPOSES.

By ROBERT BREWSTER STANTON, M. Am. Soc. C. E.

WITH DISCUSSION.

Before entering upon the subject of this paper, a short explanation is necessary, on account of the apparent tardiness in its presentation. It was the intention of the writer, immediately on the completion of the survey down the Colorado River, in May, 1890, to prepare an account of that work and some of its peculiar engineering features, to be read before the Society; but pressing engagements, making a report for the railway company, etc., prevented its preparation that summer. At the Cresson convention, in June, 1890, the Society did the writer the honor to spend the greater part of one evening looking at some one hundred and fifty stereopticon views of the cañons through which the survey passed, while he gave a running talk and description of some of the ex-

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periences of the journey, and in answer to many questions attempted to explain the engineering difficulties of the route.

Although the subject has thus been presented before the Society, it seems proper to put in a more permanent form a record of the methods employed and the results obtained in an undertaking which has at least one attractive feature, that of novelty. This novelty is twofold: the very boldness of the conception of constructing a railway at the bottom of the supposed impassable cañons through which this great water course runs, and on account of the numerous rapids, falls and cataracts in the river, the dangerous and exciting work necessary while navigating it, in order to make what would have been otherwise a simple exploratory survey.

It would be entirely out of place in such a paper as this, to enter into a description of the many adventures of the expedition while making the survey, its perils by land and by water; or to picture the wonderful and beautiful scenery of those remarkable cañons. However, although it shall be confined to the engineering problems encountered, and to such a description of the country as may relate to a railway route, yet it will be necessary, in order to make clear the nature of the work and the methods employed, to refer to some incidents of the journey that were not technically engineering experiences.

The question of a low grade railway line from the western base of the Rocky Mountains to tide water on the Pacific Ocean, by way of the Colorado River of the West, is not a new one. It has been discussed by engineers for twenty years at least, ever since the first explorations of the great cañons of that river by Major J. W. Powell in 1869. This exploration by Major Powell was undoubtedly the first journey ever made through these cañons. Some wild stories are told of two men having gone through the Grand Cañon on a raft of small cottonwood logs. This story was first published in General Palmer's report on the Pacific Railway Surveys,* and afterward republished with many embellishments by the author, and illustrated with some outlandish pictures, in Bell's "New Tracks in North America."†

While no records have yet been found that show any earlier exploration of the Colorado River, by passing along its waters and through its

* "Report of Surveys Across the Continent," 1868, General William J. Palmer.

† "New Tracks in North America," William A. Bell, London, 1870.

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cañons, than that of Major Powell in 1869, some discoveries seem to point to an attempt at least, made more than thirty-three years before.

While making the survey through the lower part of Cataract Cañon the writer discovered an inscription, or rather a name and date, thus:

"1836
D. Julien."

This in itself would not appear very wonderful; but when taken together with the following facts is significant. This inscription was cut with a tool resembling a dull center punch. It is upon the wall of the cañon under an overhanging cliff, and at such a height and in such a position—where the water fills, and undoubtedly at that time, filled the whole gorge from wall to wall—that it could only have been cut from a boat (or other float), and at quite a high stage of water. The point is on a level, smooth stretch of the river, but above some heavy and narrow rapids, on account of which it would have been practically impossible to reach this stretch of water from below. The inscription is considered entirely genuine, both from its position, as well as the style and shape of the letters and figures used. Hence the writer is led to believe that this was cut by a party descending the river.

A very slight investigation of this shows that about the time of the date given upon the wall of the cañon, a party of French voyageurs, traveling in the Northwest, divided into two parties, one starting to explore in the Southwest. It has as yet been impossible to push this investigation any farther, though it is believed that some records of these voyageurs still exist; but whether these records will show the previous history of the man who cut the name and date in the cañon or not, it is extremely doubtful if anything farther can be learned of the results of his journey, or how much farther down the river he went before he was swallowed up in its mighty cataracts.

For the purposes of a convenient record it will perhaps not be out of place to give a short account of the earliest examinations of the lower or valley division of this notable river.

The Gulf of California and the mouth of the Colorado were first discovered, as far as we have any account, by the naval division of the expedition of Francisco Vasquez de Coronado, in the spring of 1540. On the 26th of August, 1540, two Spanish shallops, under command of Fernando Alarcon, started up the river. They traveled fifteen and a half days, and then turned back. On the 14th of September of the

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same year Alarcon again started up the river, and continued for a distance of 290 miles, when he returned to the mouth.

It has generally been supposed that the first attempt to navigate the lower Colorado with steamboats of any size was made by Lieutenant Ives in 1858. By some recent investigations it is found that this is not the case. Captain George A. Johnson, who now resides at San Diego, Cal., first went to the mouth of the Colorado in 1851. Soon after that he built at that point three steamboats, and as early as April, 1852, went up the river by steamboat as far as Fort Yuma, at the mouth of the Gila River.

Captain Johnson continued to navigate that portion of the river a distance of nearly 150 miles, carrying supplies to the Government troops and settlers till the fall of 1857. In December of that year Captain Johnson made his first trip to any distance above Fort Yuma. This journey up the Colorado was made in the steamboat *General Jessup*, a boat 108 feet in length, of 28 feet beam, and drawing 2 feet 6 inches of water. With a military escort under Lieutenant James White, of the 3d U. S. Artillery, they started from Fort Yuma in December, 1857, and steamed up the river to El Dorado Cañon, about 68 miles below the mouth of the Rio Virgin. Captain Johnson was engaged in this expedition between three and four months, and while coming down the river met Lieutenant Ives and his Government party some distance above Fort Yuma. Lieutenant Ives reached the mouth of the Colorado the 29th day of November, 1857, and started up the river with his little steamboat, *The Explorer*, on the 21st of December, 1857. After passing Captain Johnson on the river he proceeded up to El Dorado Cañon, where the *General Jessup* had been before him, and for some few miles beyond into the Black Cañon.

A very interesting account of this expedition may be found in Lieutenant Ives' report published by the Government;* but it is rather remarkable that he does not mention Captain Johnson's boat, which was large enough to have stowed away out of sight Lieutenant Ives' little *Explorer*. The river is navigable for steamboats drawing from 3 to 4 feet for about 575 miles to the mouth of the Rio Virgin. Its navigation has some peculiarities. One trip of the steamboat *Gila*, a boat 180 feet in length, required nearly four days from El Dorado to the

* Report of Lieutenant Joseph C. Ives, 1857-8, Senate Ex. Doc., 30th Congress, 1st Session.

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Rio Virgin; but in returning with a load of 80 tons of salt the same distance of 68 miles was made in three hours and ten minutes.

Since 1869 many attempts have been made to descend the Colorado in boats, but without success until the expedition of which the writer of this paper was in charge, beginning at Green River Station, Utah, May 25th, 1889, and finally completed at tide-water at the head of the Gulf of California, in the Mexican State of Sonora, on April 26th, 1890. During the years 1870–73, the United States Government made a careful and extensive geological survey of the country through which the Colorado flows. The results of this work are found in the very able and extensive reports of Captain C. E. Dutton to the Director of the Geological Survey.

In order to clearly understand the engineering features of this section of our country for a railway route, it will be necessary, and it is hoped both profitable and interesting, to give a short account of the geological creation of this whole area, and also some history of the formation of this most remarkable and peculiar river course.

By referring to a map of the western part of the United States (Plate XXVI) it will be seen that the Colorado River of the West is formed by the junction of the Grand and the Green rivers. The Green River rises in the Wind River Mountains in the northwestern part of Wyoming, and flows in a southerly direction through Wyoming and Utah. The Grand River rises in the northern central part of Colorado; its head waters being the Grand Lake, which is situated at the western base of the Rocky Mountains, close up under the Continental Divide, and about 42 miles west and 33 miles north of the City of Denver. Flowing in a generally southwestern course, it forms a junction with the Green River in the western part of the Territory of Utah. The waters of these two form the Colorado. This river flows in a generally southwestern course to the mouth of the Colorado Chiquito, in the Territory of Arizona. Thence its general course is west to the mouth of the Las Vegas Wash, about 30 miles below the mouth of the Rio Virgin, in the State of Nevada; and thence its course is almost due south, forming as it does the boundary line between the Territory of Arizona on the east, and the States of Nevada and California on the west, until it empties into the Gulf of California, in the Mexican State of Sonora. Taking the Green as the main branch, this river course is something over two thou-

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sand miles long. The distance from Grand Junction to the Gulf is in round numbers twelve hundred miles.

For the purposes of railway examination the Colorado River may be divided into two general divisions :

First.—That from the mouth of the river up to the mouth of the Grand Cañon, a point about 38 miles east of the Rio Virgin, and near the eastern line of the State of Nevada. This may be called the Lower or Valley Division.

Second.—That from the mouth of the Grand Cañon to the town of Grand Junction, Colorado (including that portion of the Grand River above the Colorado). This may be called the Upper or Cañon Division.

The Creation of the River.

From the western side of the Rocky Mountain range—speaking only in general terms—the whole country through which the Colorado River runs is a series of terraces and high plateaus, extending as far west as the Grand Wash, or end of the Grand Cañon, where the surface descends to a much lower elevation. This low country reaches to the eastern side of the mountain ranges of Nevada and California, which separate it and the valley of the Lower Colorado from the Pacific Ocean. This high "plateau country" is divided into valleys, terraces and plateaus, ranging in height from 4 000 to 10 000 feet above the level of the sea, with mountain peaks, extinct volcanoes and cinder cones reaching to an elevation of some 13 000 feet or more above the sea.

Geologists tell us that over this whole western part of our continent, during the long geological ages, from the Carboniferous to the Eocene periods, the bed of the great ocean that covered it gradually sank, and as it sank there was deposited under the waters the strata of rock we now see exposed, amounting in all to from 14 000 to 20 000 feet in thickness. About the beginning of the Eocene period there came a turning point in the origin of this part of the continent. First came a great upheaval on the south of the province where the San Francisco Mountains now are in New Mexico. The eastern portion rose gradually, and lastly and more suddenly rose the far west, forming a great Eocene lake, with the older Uinta Mountains on the north, and the Rocky Mountains to the east. Thus what is called the "Plateau Country" was formed into one continuous lake or inland sea, surrounded in the most part by mountain ranges. This plateau lake seems to have been one of the

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largest and most enduring of the great lakes of those ages. The volume of sediment accumulated upon its bottom ranged from 1 200 to upward of 5 000 feet in thickness. At length, as the ages rolled on, there was a cessation of this deposit and a beginning of the gradual upheaval of the whole country. An outlet for the elevated waters of this lake was formed in the southern or southwestern portion.

Following the careful and convincing argument of Captain C. E. Dutton, from the evidence which seems clearly to be written, "we must also infer that upon the floor of this basin, as it emerged, a drainage system was laid out. Such a drainage system would necessarily conform to the slopes of the country then existing. Taking the supposition already made, that the uplift was somewhat greater upon the eastern than upon the western side of the province, the configuration of the principal drainage channels would be very much like that now existing. The trunk channel would flow southwestwardly and westward, while the tributaries would enter it on either hand, very much as the larger and older tributaries now do."

"With the final desiccation of the Grand Cañon district began the great erosion which has never ceased to operate down to the present time. Concerning the details of this process we know but little, and we can only guess at its general character during the earlier ages. Erosion is here associated with a large amount of uplifting, and we may conjecture that as the uplifting went on the irregularities produced by erosion became greater and greater, the valleys grew deeper and the intervening mesas stood in high relief."

This earlier upheaval was coincident with the earlier volcanic eruptions, and is roughly estimated at from 2 000 to 3 000 feet.

"The river found its base level soon after the uplifting ceased, and the cañons slowly widened by the recession of their cliffs."

As the ages rolled on, quoting again from Captain Dutton, there came "the final upheaval which has already brought the region to its present condition. The Colorado River, after remaining without erosion at the level of the esplanade of the cañon during the greater part of the Pliocene time, at length resumed the operation of sinking its channel. A new paroxysm of upheaval set in; the faults increased their displacement; the volcanic vents reopened. This time the upheaval was greater than before, amounting probably to from 3 000 to 4 000 feet, and the narrow inner gorge was swiftly cut."

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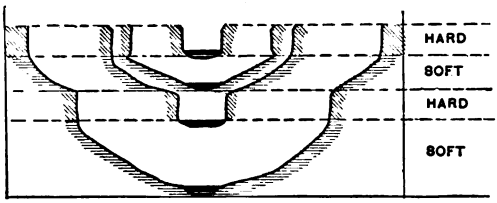
"During the last stage of the evolution the Glacial period intervened between the arid climate of the Pliocene and that of the present time. The glacial period here was not icy, but rainy, and very probably colder than the present. In some localities it began to excavate systems of local drainage channels and to carve out minute details of topography."

Thus the process, which was twofold—erosion and weathering—has gone on from age to age. At times the gorge of the main river has been partially filled up by the inflow of great streams of lava from the volcanoes above. In places this lava filled the whole gorge to the depth of hundreds of feet; but the mighty volume of water has cleared out its channel a second time, as if the huge masses of basalt were but piles of chaff and straw, till to-day we have this marvelous river flowing through a stupendous chasm, from 1 to 13 miles wide on top, and at a depth of from 1 000 to 6 200 feet below the general level of the surrounding country, with a system of lateral gorges from one-half mile to a mile in depth, and extending many miles back into the country on either side, and with a fall in the distance surveyed of over 4 500 feet, and containing, in less than 500 miles, 520 rapids, cataracts and falls.

With such a picture as this, it is not surprising that the practicability of building a railway at the bottom of the gorge should be seriously questioned. Before entering upon the discussion of this part of the subject, as bearing particularly upon it, one more quotation will be made from Captain Dutton's valuable book.



Development of Cañon Profiles.*

"Since the attack of erosion, under the conditions prevailing in the plateau country, is mainly directed against the edges of the horizontal strata, and since these strata vary among themselves in respect to hardness or durability, it follows that the different beds would, if the exposures were equal, weather at different rates. The softer beds would disintegrate rapidly and undermine the edges of the harder beds overlying them. The harder beds being robbed of support, cleave off by the joints, and the fragments fall. The fragmental material thus produced

* "Tertiary History of the Grand Cañon District," Capt. C. E. Dutton.

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is not immediately carried away, but remains in part and forms a talus."

It is not necessary to quote farther. The nature of this talus for a road-bed will be explained for each separate cañon.

Remembering, then, how these cañon walls were formed in general, and the fact that in the Grand Cañon, where the depth is from 3 000 to 6 200 feet, the width on top is from 6 to 13 miles, some light is thrown upon the supposed perpendicularity of these cañon walls, and the narrowness of their defiles. The important consideration, however, was, what were the details of formation that would affect the construction of a railway in order to take advantage of the easy grades offered by this water course?

No attempt had been made, as far as is known, in the geological surveys to gather and put on record any detailed information as to the nature and formation of the immediate banks of the river, or in other words the lower 100 to 200 feet of what are called the cañon walls. When the question was to be settled as to the practicability of this water-course as a railway route, it therefore became necessary to make a complete examination and survey at the bottom of the cañons along the river itself.

The subject proper will be divided under two heads:

First.—The preparations and outfit necessary for such a work, together with the methods employed in making the survey.

Second.—The results obtained.

In the spring of 1889, the Denver, Colorado Cañon and Pacific Railroad Company was organized for the purpose of making a preliminary survey from Grand Junction, Colorado, the point where the Denver and Rio Grande Railway crosses the Grand River, down the Grand and Colorado rivers to tide-water at the head of the Gulf of California. The promoter of this project, the Hon. Frank M. Brown, of Denver, was made President of the company, and being very enthusiastic on the subject, determined to accompany the engineer corps on all the surveys. President Brown's connection with this expedition, and his sad and tragic death by drowning during its progress, together with two of the boatmen of the party, in the following July, need only be referred to here. The causes which led to such a catastrophe will be stated farther on.

The party left Denver by rail the 22d of May for Green River Station, Utah, where had been gathered together the boats, supplies and necessary

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outfit, and consisted—with the writer—of sixteen men: four instrument men, one photographer, two rodmen, two flagmen, two axemen, and one cook, besides President Brown and two guests invited to accompany him.

In addition to the various duties on the survey, each man was expected to act in the capacity of boatman whenever required, and as far as it was possible was chosen with this end in view. It being impossible to transport the supplies for the subsistence of the men in any other way, President Brown had fitted out in all six boats for the expedition. Five of these were ordinary clinker built cedar hunting boats, 15 feet in length, both bow and stern being pointed exactly alike, and decked over about 3 feet back. In each end was fitted a light copper air-tight compartment. The other boat was a common board skiff that had been used on the Grand River survey.

Provisions were provided for three months, it being expected to replenish the supplies at Lee's Ferry, and at Peach Springs. To guard against loss, the provisions were separated into five parts, packed in canvas sacks and so placed that each boat contained almost exactly the same amount of every article of food. All other supplies, instruments, etc., were divided as far as possible in the same manner, except the cooking utensils, which, much to our future sorrow, were all in the last or "cook boat," and were nearly all lost when that boat went to pieces in the rapids. Everything was reduced to the lowest possible weight. No tents or shelters of any kind were taken. Each man was allowed 10 pounds of extra clothing and 15 pounds of bedding. A complete supply of medicines was carried, in triplicate, and packed in three different boats. For the purposes of the survey two complete sets of instruments, transits, levels, rods, chains, etc., were taken; besides an extra transit with solar attachment.

While outfitting the party the question arose how most effectually to do the work so as to clearly present the results not only to engineers, but to capitalists and possible investors.

This question was one of vital import on account of the fact that up to this time the cañons of the Colorado had been considered not only impracticable, but even impossible, for railway purposes, on account of the supposed perpendicularity of their walls.

It was therefore decided to add a new assistant to the corps of engineers in the form of a photographer with a duplicate set of photographic

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instruments and some 1 800 films or paper rolls. The work of the photographer in the actual survey will be more fully set out farther on.

The survey from Grand Junction to the mouth of Grand River or head of the Colorado having been previously completed under Mr. Frank C. Kendrick, our party, on the 25th of May, embarked on the Green River, the main branch of the Colorado, at the crossing of the Denver and Rio Grande Railway. This branch was chosen for the purpose of reaching the Colorado on account of there being but one bad rapid in a distance of 120 miles.

This first outfit provided for the expedition was defective in three points. The boats were totally unfit for the work they had to do, no life preservers were provided for the men, and no adequate protection from the water for the provisions. When it is understood that from the point where we embarked to the gulf, the river has a fall of over 4 500 feet, and that in a distance of less than 500 miles contains 520 rapids, cataracts and falls, it will be seen that these defects were of vital importance to the success or failure of the survey.

We reached the head of the Colorado May 29th, and on the 30th took up the survey that had been brought down the Grand River. In order to make clear the nature of the work and the necessity for an entirely new outfit before the survey could be completed, it will be proper to briefly relate a few incidents of the journey from the 1st of June to the middle of July. On the 1st of June we encountered the heavy rapids of Cataract Cañon. It was but a few days till our cook boat was jammed between two rocks in Rapid No. 10, and nearly all of the cooking utensils and a large batch of supplies lost in the river. Disaster followed disaster. All of our boats were badly damaged, two of them entirely gone.

On the 16th of June, by the upsetting of the boats all of our provisions were lost except a sack and a half of flour, a little coffee, sugar and condensed milk. This looked very desperate for a party of sixteen hungry men in such a country as the heart of Cataract Cañon. The next morning President Brown, with a picked boat's crew, started for the placer mines, some 35 or 40 miles down the river, to procure supplies. The scarcity of provisions and the separating of the party alarmed the men, and the greater number decided to desert the work and to follow Mr. Brown, leaving, as they did, their instruments standing upon the line. Knowing that if we abandoned the survey then we could not

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return to that point and complete it, and believing that the work could be completed to Dandy Crossing with the food on hand, which was divided equally among all the men, the writer determined not to leave, if a sufficient number of men would remain to assist him. The first assistant, Mr. John Hislop, together with C. W. Potter, one of the flagmen, and the two negro servants, Henry C. Richards and G. W. Gibson, the cook, volunteered to remain and continue the survey. These men deserve special mention and praise for the faithful and uncomplaining manner in which they pushed on the work till the evening of the 20th. It requires more than ordinary nerve to do the work of an engineering party over such a country with only about one-tenth the amount of solid food that a healthy man requires.

On the evening of the 20th the party was met by a relief boat with supplies from the placer mines, and soon completed the survey to Dandy Crossing. At this point a reorganization of the force and work was decided upon. Mr. W. H. Bush was left with one party to carry the survey through Glen Cañon, as far as Lee's Ferry. President Brown, the writer, the photographer and five men with three of the cedar boats started, in accordance with President Brown's desire, to make a rapid examination of the lower cañons, doing no instrumental work except that with the camera. This advance party reached Lee's Ferry July 2d, and remained seven days waiting for new supplies. July 9th it left Lee's Ferry and entered Marble Cañon. The next day, while running Rapid No. 108, the first boat was upset, and in less than two minutes after starting from the shore President Brown was drowned. He had no life preserver and it is practically impossible for one to swim in those surging waters without some such assistance.

Being very desirous to complete the work, the writer concluded to push on, even then not fully appreciating the utter worthlessness of the boats and other means provided for making such a journey. On July 15th another accident in Rapid No. 133 upset one of the boats, and the two boatmen, Peter M. Hansbrough and Henry C. Richards, were both drowned. It was then decided to abandon the attempt to go farther down the river, and to return to Denver, every one of the party determining as soon as a new and proper outfit could be secured to again take up the work and complete the survey through the cañons. All supplies, instruments, etc., were cached in a marble cave about 40 miles below Lee's Ferry, and after some hard climbing out of the gorge, we reached

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he Mormon settlements north of the river, and on the 27th of July the emnant of the expedition arrived at Denver.

The Second Outfit.

The writer immediately set about preparing for a second expedition. Profiting by the sad experiences of the summer before, the new outfit was vastly different from the first. From special plans, the R. J. Douglas Company, of Waukegan, Ill., built three boats for this second expedition. They were entirely of oak, 22 feet long, 4½ feet beam and 22 inches deep. The ribs were 1½ x ¾ inches, placed 4 inches apart and planked with one-half-inch oak, with butt joints calked with oakum, the whole riveted together with copper rivets. Both ends were built exactly alike, decked over 3½ feet back, and a deck 6 inches wide ran along on each side. At each end were water-tight lockers for storing supplies, above these under the decks air-tight compartments made of heavy galvanized iron, and along the sides, under the 6-inch decks, were placed four air-tight tanks of the same galvanized iron. These side tanks extended down about 10 inches under the water line when the boats were fully loaded. Fitted in this way, with ten separate air-tight compartments in each boat, it was found, even in the roughest water, that they could not be upset, and when loaded with 3 200 pounds in each and filled with water they would not sink (Plate XXVII).

The boats were fitted for two oarsmen using 8-foot oars, and steered with a 12-foot oar. Along each side was fitted a strong life line. To the bow was attached one short 20-foot line and one long one 250 feet for the purpose of letting the boats down the rapids when it was impossible to run them; to the stern was one 50-foot line, all of them five-eighths inches in diameter. In each boat was carried a small set of carpenters' tools, copper rivets, nails, screws, oakum, pitch and sheet copper for repairs.

All provisions, clothing and supplies, except bedding, canned goods and meat, were packed in forty-eight heavy rubber bags made expressly for this expedition. They were 12 inches in diameter and 20 inches long, with mouthpieces 16 inches in length (Plate XXVIII). After the bag was filled this mouthpiece or flap was folded and rolled very tight and laced over with strong lace cord; over them was then fitted a strong canvas sack to save the rubber from being chafed. These bags when laced tight were found to be very effective in excluding the water, one boatload being

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under the water for several hours without injury. Besides these rubber bags, in each boat were fitted, in special compartments near the bow, two 5-gallon kegs, filled with rice, beans, sugar, coffee, etc.; this as a special precaution against accident, as they would float, while the bags would sink. As before, all supplies, including this time the cooking utensils, were divided equally between the three boats. A supply of medicines, rifles, shot guns, ammunition, etc., was taken. Expecting to be out all winter four small tents were provided; but these were soon thrown away as being a cumbersome luxury. On account of the winter weather more blankets, overcoats and clothing were necessary. Four months' provisions were taken at the start, and each boat carried when fully loaded about 3 200 pounds, besides the four men. A false bottom was placed in the boats, under which all canned goods were packed next to the keel, with the salt meats and hardware in the lockers at each end. Everything was so packed and strapped to the boats that in case of an upset nothing could be detached unless the boat was completely smashed.

The new photographic supplies consisted of two 6½ x 8½-inch cameras and one 4 x 5-inch detective instrument, and 2 000 films in rolls with extra roll holders. Each roll of films was soldered up in a tin box and these packed in the rubber bags. As each roll was exposed it was again soldered up in the tin boxes and the duplicates carried in separate boats. All necessary instruments, transits, levels, rods, barometers and thermometers were, of course, supplied. The best cork life preservers were provided for all the men, and they were required to wear them whenever on the water. These life preservers proved of greatest service. Mr. John Hislop, the first assistant, and the writer were both thrown from their boats while running the rapids, and their lives only saved by such assistance.

The party consisted of twelve men in all: Mr. John Hislop, first assistant; Mr. Harry McDonald, Mr. Langdon Gibson, Mr. Elmer Kane, Mr. F. A. Nims, photographer; Mr. Reginald Travers, Mr. W. H. Edwards, Mr. A. B. Twining, Mr. H. G. Ballard, Mr. L. G. Brown, Mr. James Hogue.

Only six, besides the writer, went through the cañons to the end of the journey. Mr. Nims, photographer, broke his leg January 1st, 1890, and was left at Lee's Ferry; McDonald left of his own accord in February, after reaching the Grand Cañon; Twining, Brown and Hogue,

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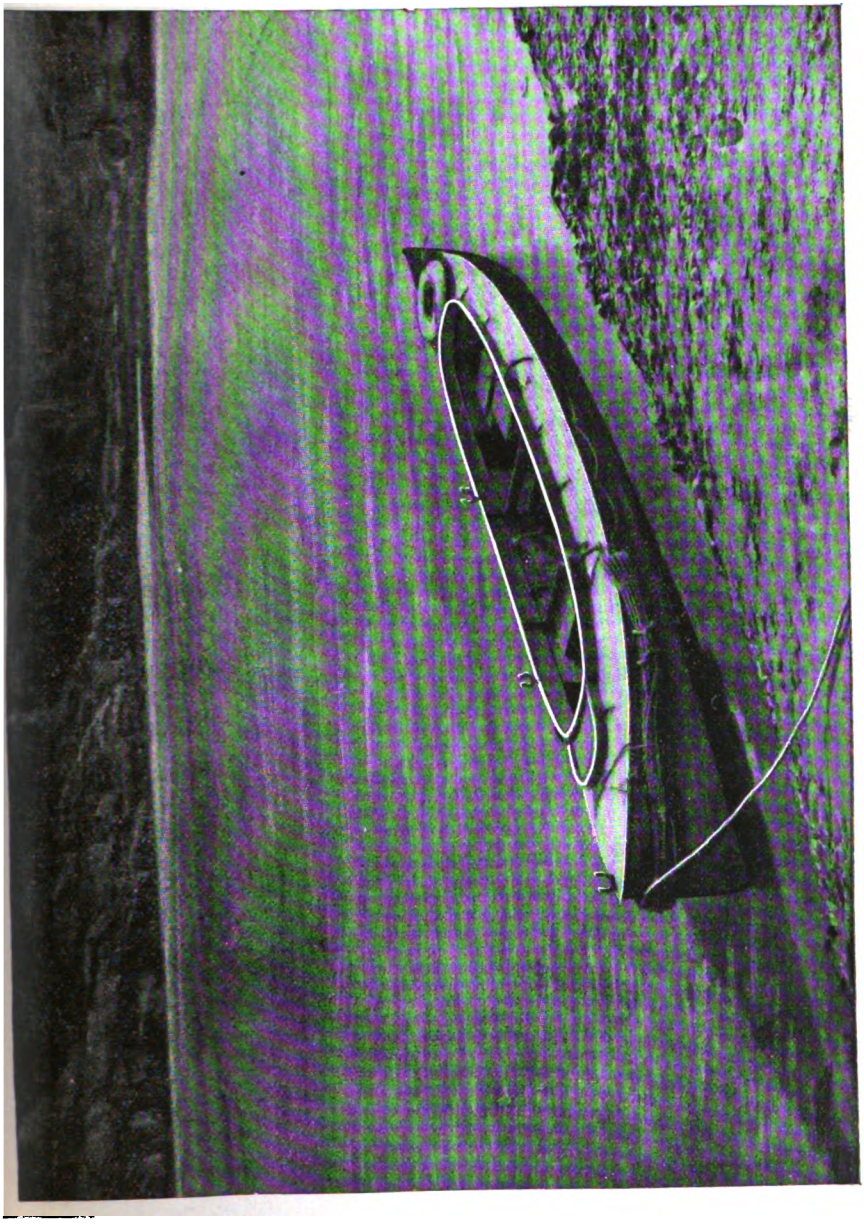
PLATE XXVII.
TRANS. AM. SOC. CIV. ENGS.
VOL. XXVI, NO. 523.
STANTON ON CANONS OF THE COLORADO.



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having lost their boat, were sent out at Peach Springs. The men remaining were chosen for the double duty of an engineer corps and as boatmen to navigate the river.

This second expedition was completely successful in every way, accomplishing the journey to the gulf without disastrous mishap. To avoid the unnecessary hardships of Cataract Cañon, the boats were hauled by wagon 120 miles from Green River Station to the mouth of Crescent Creek, 4 miles above Dandy Crossing. The expedition embarked December 10th, reached the mouth of Diamond Creek March 1st, emerged at the lower end of Grand Cañon March 17th, reached the end of the survey at tide-water April 26th, and returning to Yuma was disbanded on April 30th. One boat was completely destroyed in Rapid No. 249; but only two sacks of provisions were lost in the whole journey.

Thus had the two expeditions, considered as one, traveled by boat a distance of over 1 400 miles, had passed over—running nearly all of them—520 rapids, falls and cataracts in less than 500 miles, making a total fall of 4 500 feet, and had passed successfully through the dark cañons of one of the most tempestuous rivers of the world.

Methods Employed in Making the Survey.

The maps of the Government Geological Survey gave the general course and length of the river, its fall by sections, and all the general information required. What was needed, however, to settle the practicability of this as a railway route, was detailed information upon the alignment and fall of the river, the nature and material of which its banks were formed, and careful inspection of the lower 100 or 200 feet of the cañon walls. It will be readily seen that much of the work usually done on a preliminary survey might be dispensed with, and much other work of a very different character would be required.

For the purposes of right of way, a continuous transit line was run for the first 355 miles. Over 600 miles of transit line was run altogether. Such work was done at all points where there was any question as to possible difficulty of curvature. Where long stretches of the river occurred with slight curvature, such work was not thought necessary.

All measurements were made by stadia, and triangulation. On all transit lines run contour topography was sketched. Levels were only taken at points where excessive fall in the river required a settlement

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of the question of grade, or at points where the line would require sharper grades to cross long bends or rise from one ledge to another. Where the average fall of the river did not exceed 10 to 12 feet per mile, and there did not occur any reason why a road-bed should not follow nearly parallel with high water mark, no such work seemed to be required. For a very large proportion of the whole distance there was no difficulty in the way of running a line along the shore in the usual way without the use of boats. This will be readily seen by an inspection of the photographs. At other points the work could only be accomplished with the help of the boats.

In those portions of the river where the rapids were heavy and thick, it was the usual method to take the boats ahead, make camp, and return along the shore for the line, which was carried ahead of the camp. In the smooth portions of the river, in Narrow Cañon, all through Glen Cañon, and on the lower river below the cañons, the first boat carried the front flagman ahead, the second boat brought the transitman and photographer up to the next station, and the third boat assisted the topographer and did other special work. Permanent backsights were set up at all turning points. On some occasions when for special reasons only one boat could be used many devices by triangulation, not necessary to be described, were employed to carry on an accurate line.

Careful readings of the barometer and thermometer were taken three times each day. In addition to the usual notes kept by the transitman, leveler and topographer, the writer kept an extensive series of notes, as much in detail as possible. Besides a record of all the work done upon the river, these notes covered a careful description of each section of the shore line (connected by numbers with each of the photographs as taken), its form and general appearance, with numerous sketches showing cross-sections and peculiar forms, requiring special work, careful classification of all material in each section, a record of every waterway required, from 10 feet opening up, with a general description of the whole country, and its bearing upon a railroad line as to its grade, alignment, methods of construction, nature of the road-bed, and requirements for maintaining the permanent way. These notes cover nearly two thousand pages. The system adopted for classification was not that generally used on construction, as no estimate was possible or necessary in cubic yards, but a classification into kinds or classes of work. For example: Level bottom lands, hillside slopes,

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rough talus slopes, smooth, solid rock slopes, cliff bench work, heavy through rock cuts, heavy embankments, rock bluffs, tunnels, vertical cliffs and half tunnels.

It is believed that this classification, with the numerous photographs, will give a clear understanding of the whole nature of the work.

This brings up for consideration one of the most important features of the survey. While photography had been used in many ways to illustrate engineering works, it is believed that never before had the camera taken so active and important a part in making a preliminary railway survey and exploration through a hitherto almost unknown land. As intimated above, the object to be attained was to bring back indisputable evidence of the nature and possibilities of a country practically inaccessible except through dangers to both life and limb. The process was very simple. Photographs were taken in such number all along the line as to show almost a complete panorama of the river banks and the walls of the cañons, from the head of the Colorado to the Gulf of California. Through the most difficult portions these pictures were taken so as to overlap each other and leave no detail of difficult construction without its negative to supplement and demonstrate the notes and opinions of the engineers. This continuous panorama was not carried out in full over the whole distance for several reasons. During the stampede of the first expedition, only a few photographs were taken in the lower Cataract and Narrow cañons. The work there, however, being so uniform in its character, the pictures taken fully illustrated the work when used in connection with the instrumental and other notes. On the lower river, where, for miles at a time, the line would run over identically the same formation, pictures were taken less frequently.

About 2 200 negatives were taken in all, most of them in duplicate and some in triplicate. Nearly 1 000 views were secured that illustrated the route. These views, arranged in their order, noted on the maps, and connected by numbers with all notes, are of such value as not only to convince the most incredulous of the entire practicability of the route, but also cover the line so in detail that, with the notes of distances and classification, will enable engineers anywhere, familiar with rough mountain construction, to make as correct an approximate estimate of the cost of such a road as can those who accompanied the expeditions.

It is a notable fact that while the President of the Company and two

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boatmen were drowned, provisions, instruments and boats destroyed, not a single negative was lost in the river. Of course, special care was taken to preserve them; yet it is almost marvelous that during the many disasters and mishaps, this most valuable evidence was kept from even being wet. As noted, the photographer, Mr. Nims, who did all such work down to the middle of Marble Cañon, fell January 1st and broke his leg. No other man in the party had ever taken a photograph. It was with much anxiety and misgiving, on account of the importance of securing pictures of the Grand Cañon, which was still below, and the most important section of the river, and the utter impossibility of securing in time another professional operator, that the writer undertook the work; for he had never before as much as focused a camera.

It may not be out of place to note here, that while exposing his first negative, and while in the act of dashing his hat upon the ground and tearing his hair in his excitement and perhaps rage at his complicated situation, the cook took a snap shot at him with the "detective" and preserved for his future contemplation the absurd picture he was making of himself. Twelve hundred exposures were made without his being able to see a single developed negative, as all such work was done afterward in Denver, but it is gratifying to state that out of the whole number taken only about 10 per cent. were not clear pictures, and this 10 per cent. were sufficiently clear to be used to illustrate the survey.

The first work done on starting the survey at the head of the Colorado, was to establish a true meridian. This was done at various points for the purpose of checking the instrumental work. The results were very satisfactory, and reflect much credit upon Mr. John Hislop and Mr. W. H. Bush, for the accuracy with which they handled their instruments under so many trying circumstances.

It may not be out of place to record one experience of the writer's peculiar to the depths of these wild gorges. It was the burden of anxiety ever pressing upon him that a mile might be passed over and important features not be sufficiently observed and noted at the time, for the reason that while in his many experiences in exploration in the Rocky Mountains, if an item or two were forgotten, it was but a matter of a day and some hard climbing to correct the error, yet here in these awful cañons, with death staring the party in the face at almost every rapid, it was impossible to return. And especially was this the case in the winter, when, while the wild flowers were in bloom in the valley be-

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low, the plateau above was covered with from 3 to 6 feet of snow, and in case of loss of provisions, worse than death was in store for those who attempted to retreat.

In the description of the river line and nature of the country, the names and divisions of the cañons will be used as adopted in the Government reports. The river course is divided into various cañons, though these divisions are entirely arbitrary. No natural marks of separation exist, except at the end of Glen Cañon, at the crossing of the river known as Lee's Ferry.

The Results of the Survey.

The whole subject under consideration has been divided under two general heads:

First.—The feasibility and practicability of the route for a railway line.

Second.—The advisability of such a railway.

For the purposes of this paper only the first of these will be considered at any length.

Under the first general head are the following subdivisions:

A—Detailed description of the line, giving the nature and character of the material encountered along the river banks.

(1) The Grand River Division, from Grand Junction to the head of the Colorado.

(2) The Cataract and Narrow Cañons, from the head of the Colorado to Dandy Crossing.

(3) The Glen Cañon, from Dandy Crossing to Lee's Ferry.

(4) The Marble Cañon, from Lee's Ferry to the Little Colorado.

(5) The Grand Cañon, from the Little Colorado to the Grand Wash.

(6) From the Grand Wash to the mouth of the Rio Virgin.

(7) From the Rio Virgin to the Las Vegas Wash.

(8) From Las Vegas Wash to The Needles.

(9) From The Needles to the Town of Yuma.

(10) From Yuma to the head of the Gulf of California.

B—Statement in tabulated form of the results of this detailed survey.

C—Methods of handling such a work in so difficult and inaccessible a country.

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D—The nature of the line itself.

(1) The grades.

(2) The alignment.

(3) The distances.

(4) Maintenance of the permanent way.

(1) The Grand River Division.

From the notes and maps furnished by Assistant Engineer Kendrick the following statements of this division are compiled.

It must be borne in mind that during the survey down the Grand River no photographs were taken. The general class of work being known, however, it can be well illustrated by pictures taken further down the river.

From Grand Junction the line follows parallel with the Rio Grande Western Railway down the valley of the river 13½ miles, when it turns due west, and in less than two miles reaches the bank of the river. Following along the right bank of the river, on alternate stretches of bottom land and hillside slopes of talus (loose and solid rock), to a point about 22 miles from the beginning, the line crosses Grand River, and in cutting off a long bend uses the first and longest tunnel on this division. This tunnel would be 5 900 feet long and save a distance of nearly 4 miles of heavy work. Keeping, then, upon the left bank of the river, there follows the same succession of bottom lands, hillside slopes and talus of loose rock, with occasional rock bluffs and short cliffs requiring heavy work.

Beyond here the river enters the Granite Cañon of Grand River. Mr. Kendrick did not make his survey through this gorge, but followed the high plateau over which the Rio Grande Western Railway runs to the neighborhood of the "Sisco Pumphouse." A final located line would, however, pass through this Granite gorge. From the point opposite the Sisco Pumphouse the survey followed the bank of the river, the work being heavier with an increased proportion of solid rock excavation, and a second tunnel of 3 000 feet, cutting off another long bend of the river. Seventy-five miles from the starting point the Little Castle Valley, a rich agricultural district, is reached. Through this valley there are 10 miles of earthwork. For 15 miles farther on are found the same hillside slopes and talus of earth and loose rocks, and points of cliff and solid rock bluffs. The nature of these slopes

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will be more particularly described and illustrated by photographs later on. The line passes the Little Grand Valley, another farming district, beyond which, and nearly all the way to the head of the Colorado, the Grand River is much more tortuous, and there are long bends returning upon themselves. A succession of four tunnels, respectively 3 200 feet, 3 000 feet, 4 000 and 2 700 feet in length, with the two tunnels described above, and aggregating $4\frac{11}{100}$ miles, save a total distance of $23\frac{1}{4}$ miles. In the last 36 miles of the Grand River Division there are 6 miles of bottom lands, and a large proportion of light hillside work of earth and loose rock, given in Mr. Kendrick's estimate as "pick and shovel work."

TABLE OF CLASSIFICATION IN MILES FROM GRAND JUNCTION TO THE COLORADO RIVER.

DISTANCE IN MILES.	GENERAL CHARACTER OF WORK.	Characteristic photographs illustrating each class of work.
36.323	Level bottom land, earth, gravel, etc	Nos. 25 and 33
19.273	Hillside slopes, E. & L. R	Nos. 40 and 48
61.035	Rough talus slopes, L. R. & S. R	Nos. 52 and 69
6.206	Solid rock slopes	No. 91.
13.040	Granite walls	See Grand Cañon Pictures.
4.110	Tunnels	
139.987	Total	

NOTE.—Photographs selected from those taken on the Colorado.

It would be impossible in such a paper as this to reproduce all the photographs whose numbers are used to illustrate these tables, much less the 900 or 1 000 views accompanying the writer's report to the railway company. As soon as it can be prepared it is the intention of the writer to present to the Society an album of all these views for preservation in its library. These views will be arranged in order and numbered correspondingly to the numbers given here.

As stated above under the description of the methods employed, the measurements, notes and descriptions of the route along the Colorado were taken so as to show the nature of river bank or cañon wall, and the class of material of which it was composed for each 100 feet, if any change in its character or form occurred so often, and at each point where such change was noted, one or more photographs were taken to completely illustrate it. These notes, covering nearly 2 600 pages, were

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then tabulated, giving the length of each piece of work, and different class of work, in the order of their occurrence, in this form, starting at the head of Cataract Cañon :

21 050 feet level bottom land.

2 400 feet rough slopes, L. and S. R.

2 700 feet heavy embankment under cliff.

13 400 feet hillside slopes.

4 000 feet level bottom land.

These tables covered many pages. They are not necessary, in so detailed a form, to the understanding of the subject, and will only be given here in condensed tabulated form, with general descriptions of the more marked characteristics of the several cañons and subdivisions.

(2) The Cataract and Narrow Cañons.

The length of this division, from the head of the Colorado to Dandy Crossing, is 54.214 miles.

Condensed Table of Classification in Miles, Cataract and Narrow Cañons.

Miles.	General Character of Work.	Characteristic photographs illustrating each class of work.
11.543	Level bottom land	Nos. 39 and 114
5.436	Hillside slopes, E. & L. R.	Nos. 48 and 78
14.545	Rough slopes, L. R. & S. R.	Nos. 52 and 69
2.557	Solid rock slopes	No. 106
19.110	Cliff bench work	Nos. 85 and 90
.739	Heavy embankment under cliff	No. 44
.284	Tunnel	
54.214	Total	

Some marked characteristics in this division deserve special notice.

1st. The large amount of bottom lands consist of flats formed at the bottom or foot of the generally sloping walls. They are composed of earth gravel and small loose rock. In places large detached rocks from the cliffs above lie upon these flats, but not to such an extent as to change their character from that of earthwork (Plate XXIX). See Photos. 30 to 38.*

2d. The "Hillside Slopes" are formed of the same materials, with a

*The photographs referred to throughout are to be deposited at the house of the American Society of Civil Engineers for reference.

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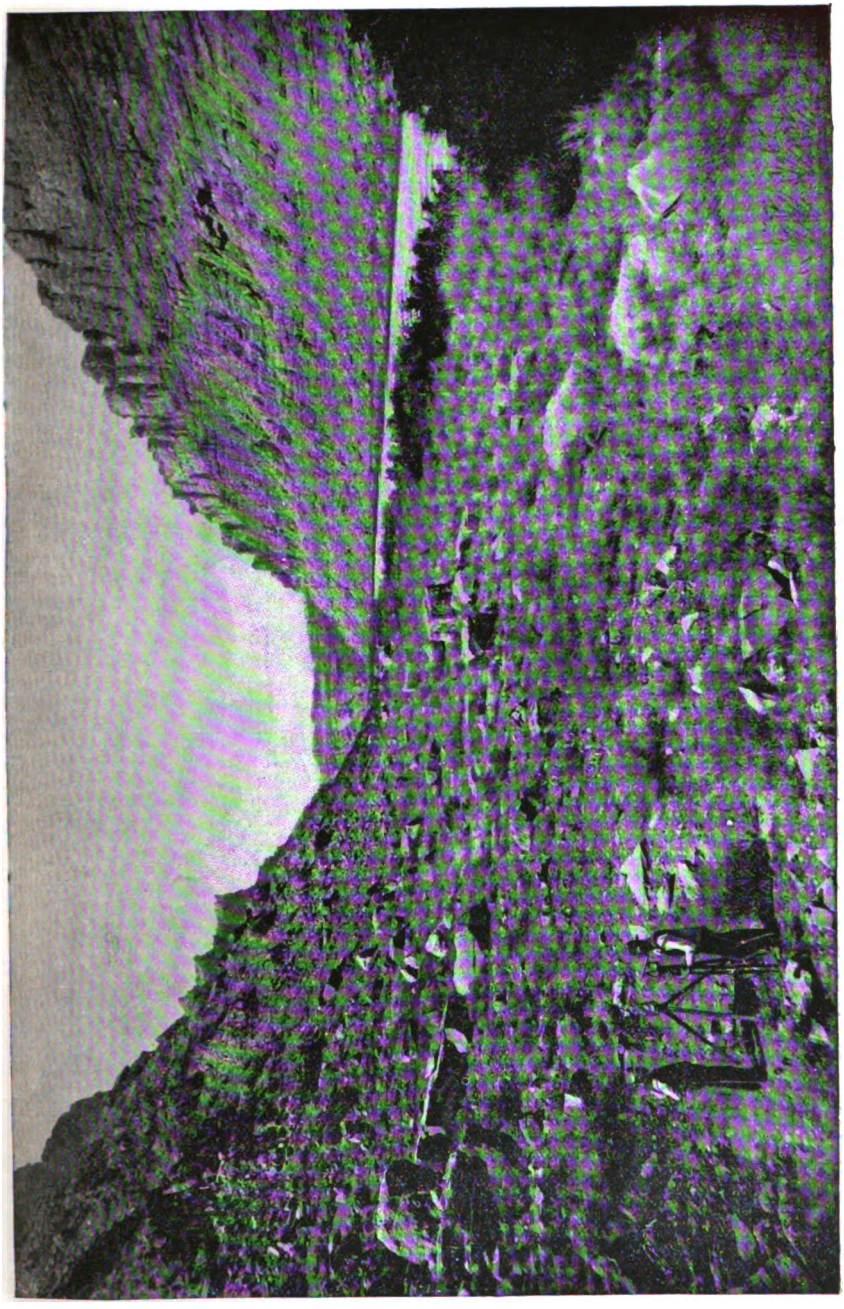

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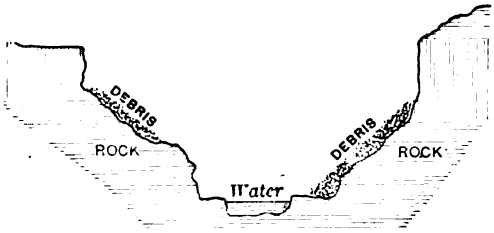
larger proportion of loose rock. In many cases these slopes are simply decomposed and broken rock covering a slope of the original formation of stratified rock in place. This is of the "softer" sandstone, and generally in horizontal layers. See Photos. 77 to 78.

A cross-section in this formation is thus shown:



At other points the earth and loose rock compose the whole slope, but are well cemented together, and would form a secure and solid roadway. See Photos. 70 to 76.

3d. The "Rough Slopes" are of the same nature, but formed of much more coarsely broken rock and boulders.

4th. At but one point on this division does there occur any really perpendicular cliff that interferes with the railroad line. This is passed by the 1 500-foot tunnel.

5th. Beyond this tunnel point the character of the cañon walls changes entirely. The gorge is much narrower, and in many places the cliffs rise vertically from the water, and frequently overhang from 10 to 30 feet. They vary from 20 to 80 feet in height from low water to the first bench, and above this is a slope to the next cliff, which varies in height as the one below. These slopes or benches are of different widths, generally about 50 feet wide.

The rock is a very hard sandstone above, with limestone ledges next to the river. Occasionally two or more benches are broken together, forming a slope down to the water. This character of "cliff bench work" predominates through the remainder of Cataract and Narrow cañons.

For a distance of nearly 6½ miles the cañon is narrow, from 250 to 450 feet between lower walls. The benches have generally low cliffs—25 to 40 feet—between them, the first rising directly from the water's edge. These cliffs are mostly of solid, hard limestone. In places the cliffs are ragged and broken into points, and occasionally stand out into the river in bold bluffs; these will require to be blasted away and thrown into the

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river. See Photos. Nos. 87, 88, 89 and 90. In a few places the second and third benches are broken down, so that the first bench is very wide and the line would be located well back from the river.

6th. At the lower end of Narrow Cañon a new formation comes in. The limestone has sunken under the river, and a massive red sandstone forms smooth, naked slopes. This first occurs opposite the mouth of the Dirty Devil River. See Photos. Nos. 100 and 101.

High and Low Water.

From the head of the Colorado to Dandy Crossing the difference between high and low water mark varies from 25 to 60 feet, and perhaps in places even more than this, the greatest difference being, of course, in the narrow parts of the Cataract and in the Narrow cañons. In those portions where the walls stand vertically from the water, the exact high water mark is hard to determine. From the very nature of the case, however, this point is almost entirely below the lower edge of the first bench; for the extreme rise is caused by the extreme narrowness of the bottom gorge, and the perpendicularity of its walls. The railway line being necessarily here located on the upper part of the first bench, and in some places on the second, in this narrow portion will be well above high water, and for nearly the whole distance upon a solid rock road-bed not affected by the rise and fall of the river.

(3) Glen Cañon.

This division has some peculiar features not found anywhere else on the whole route. It will therefore be best to explain its characteristics before giving the detailed table of classification.

First.—The average fall per mile of the river in Glen Cañon, that of 2.17 feet, is less than in any other portion above the mouth of the Las Vegas Wash.

Second.—This light fall has formed for the greater part of the way a very wide channel, in places as much as 1 800 feet, and has allowed the formation of a large amount of bottom land. These flats or bottoms are very extensive and in many places very wide. They are above all ordinary high water, and it is believed above extreme high water at this time. On account of the excessive crookedness of the river in this cañon some of these flats are peculiarly situated. Sketch made June 29th illus-

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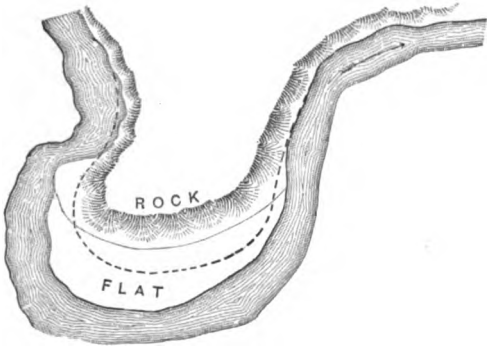
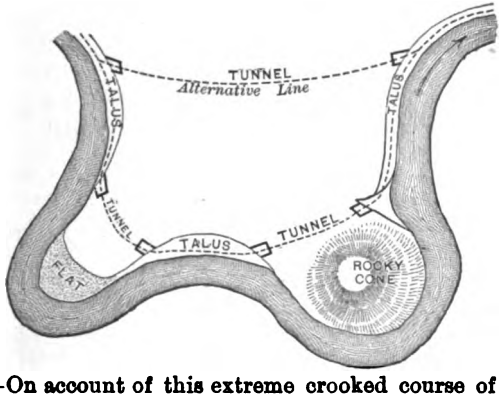
trates this—the flat being at the extreme end of the long bends. Others extend from 1 to 5 miles in length in straight lines and gentle curves. These flats are all earth-plow and scraper work. See Photos. Nos. 134 and 135.

Third.—Through nearly all of Glen Cañon the walls are of a massive red sandstone standing in vertical cliffs. For many miles under these



cliffs there is a high, solid and permanent talus. This is a true talus, composed entirely of broken rock, but on account of the extreme width of the river, and its gentle fall is perfectly secure and adapted for a secure road-bed. In the lower part of this cañon the river is very tortuous and in places has washed away the talus, and it becomes necessary to build a roadway at the foot of the cliff in the river.



Fourth.—On account of this extreme crooked course of the river in Glen Cañon, it is necessary, in order to locate a good line, to use a large number of tunnels. The survey was made following the river bank and the tunnels located by platting. In this way two lines were estimated; the first requiring seventeen tunnels ranging from 200 to 4 640 feet in length,

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and the second or alternative line having nineteen tunnels ranging from 200 to 6 600 feet in length, this last line making a saving of 18½ miles in distance. The nature of these two lines in this respect is well illustrated by the sketch on page 307.

Which of these two lines is the better can only be determined by a careful location and a proper comparison of estimates in dollars and cents. The tunnels would be through the massive red sandstone, soft enough to make cheap work, and yet hard enough, as is shown by the vertical cliffs, to stand for many years without requiring either timbering or arching. As bearing materially upon both the cost of construction, and the maintenance of such a line, the following condensed tables are given of classification of both main and alternative lines and comparison of tunnels:

From Dandy Crossing to Lee's Ferry by main surveyed line..... 156.580 miles.
By alternative "B"..... 138.100 "

CONDENSED TABLE OF CLASSIFICATION—GLEN CAÑON.

MILES.	GENERAL CHARACTER OF WORK.	Characteristic photographs illustrating each class of work.
57.270	Level bottom lands.....	Nos. 134, 212.
10.928	Hillside slopes, E. & L. R.....	" 141, 155, 174.
55.926	Rough talus slopes, L. & S. R.....	" 178, 233.
21.284	Solid rock slopes and bluffs.....	" 179, 318.
6.553	Heavy embankment under cliff.....	" 209, 210.
.501	Heavy through rock cuts.....	" 197, 260.
4.118	Tunnels (17).....	" 241, 242.
156.580	Total.	

ALTERNATIVE LINE "B"—GLEN CAÑON.

MILES.	GENERAL CHARACTER OF WORK.	Characteristic photographs illustrating each class of work.
47.576	Level bottom land.....	Nos. 134, 212.
10.473	Hillside slopes, E. & L. R.....	" 141, 155, 174.
45.519	Rough talus slopes, L. & S R.....	" 178, 233.
17.812	Solid rock slopes and bluffs.....	" 179, 318.
5.417	Heavy embankment under cliffs.....	" 209, 210.
.445	Heavy through rock cuts.....	" 197, 260.
10.858	Tunnels (19).....	" 241, 242.
138.100	Total, making a saving of 18½ miles by using longer tunnels.	

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LENGTH OF TUNNELS—GLEN CAÑON.

Surveyed line—length in feet.		Alternative Line "B."—Length in feet.		
1	300	1	300	
2	500	2	500	
3	1 000	3	6 600	
4	550	4	4 000	
5	1 500	5	4 500	
6	4 000	6	4 000	
7	200	7	200	
8	800	8	800	
....	....	9	6 000	
....	....	10	4 600	
9	1 000	11	1 000	
10	350	12	350	
11	300	13	2 640	
12	700	14	5 300	
13	4 640	15	4 640	
14	600	16	600	
15	500	17	5 300	Distance saved by using the tunnels of Line "B," 18¼ miles.
16	1 500	18	2 000	
....	....	19	4 000	
17	3 300			
4.118 miles.		10.858 miles.		

In concluding the notes on Glen Cañon two points deserve special mention. For about one-half of the 45 to 55 miles of "rough talus slopes" in the estimate, the massive red sandstone stands up above the second stratum of softer rock, and this forms a slope at about 45 degrees of thin horizontal layers of sandstone in place, only covered with a few feet of débris from above, thus reducing largely the amount of embankment to be protected from the river. See Photos. Nos. 173 to 179.

The great width of this cañon reduces the difference between high and low water to a minimum. It varies from 6 to 30 feet.

(4) MARBLE CAÑON.

Through Marble Cañon, and all the rest of the route to the gulf, the dividing lines between the different classes of work are not so distinctly drawn as in the cañons above, as can readily be seen from the photographs. The classification was therefore divided directly, as near as possible, into miles and parts of miles.

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The distance from Lee's Ferry to the Colorado Chiqueto, or Little Colorado, is 62.510 miles. A distance of 3 miles is saved by tunnels.

CONDENSED TABLE OF CLASSIFICATION—MARBLE CAÑON.

MILES.	GENERAL CHARACTER OF WORK.	Characteristic photograph illustrating each class of work.
.500	Bottom lands	Nos. 357, 364.
26.510	Heavy talus slopes. See note	" 275, 365.
.125	Heavy through rock cut	" 273, 323.
32.250	Cliff bench work	" 318.
.750	Heavy embankment under cliff.	
2.375	Tunnels (4).	
62.510	Total.	

NOTE.—Under "heavy talus slopes" are placed all slopes of talus and stratified rock in place covered by loose rock and débris.

There is now for consideration the one great cañon of the Colorado, although it is arbitrarily divided at the mouth of the Little Colorado into two—the Marble and Grand.

First.—The walls of Marble Cañon are composed of various strata of limestone—in places these have been turned into true marble. The strata are of different degrees of hardness, lying upon each other, first a hard, then a soft, and so on. The softer strata are almost always composed of thin layers of horizontally stratified rock, and they have worn away in slopes from 30 to 50 degrees from the horizontal. These slopes of rock in place are covered with loose broken rock or débris. This is the nature of the 26½ miles of "heavy talus slope" given in the table, and also of a large part of the 32½ miles of "cliff bench work" (Plates XXX, XXXI). See Photos. 273 to 282, inclusive.

Second.—The cliff bench work only differs from the talus slopes in the fact that the line is located on the slope above a vertical cliff from 50 to 100 feet above the water. This class of work is very noticeable about ten miles below Lee's Ferry, where the river cuts into a much harder stratum of rock, forming a narrow inner gorge, with vertical walls next to the water. Above these walls the cañon slopes back in a general cross-section of this form: (See next page and Photos. 287 to 294.)

In this inner gorge, being so narrow, the difference between low and extreme high water varies from 40 to 95 feet, but from the very nature of the case, a railway line would be located on the first sloping bench above the inner gorge, and well out of the way of the most extreme rise.

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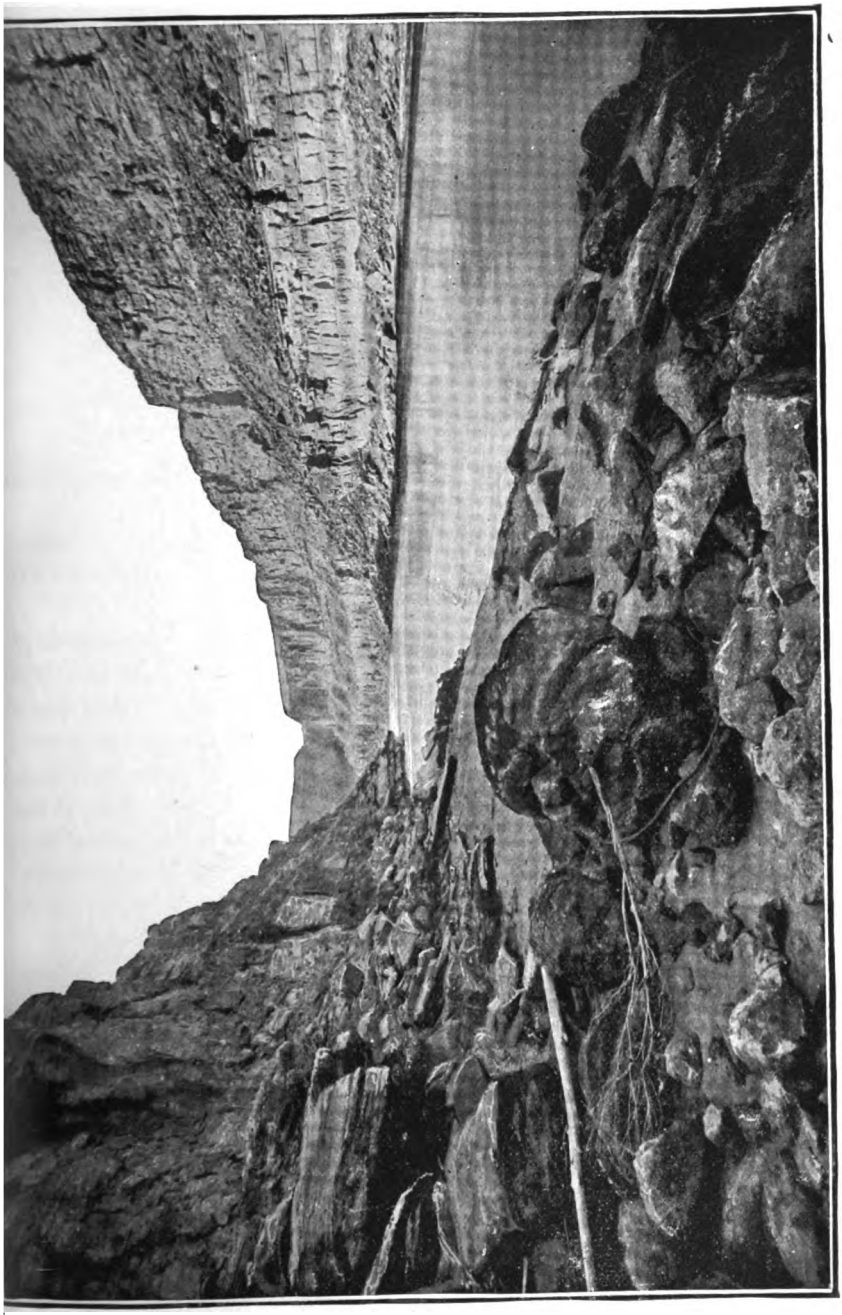
PLATE XXX.
TRANS. AM. SOC. CIV. ENGS.
VOL. XXVI, NO. 523.
STANTON ON CANONS OF THE COLORADO.



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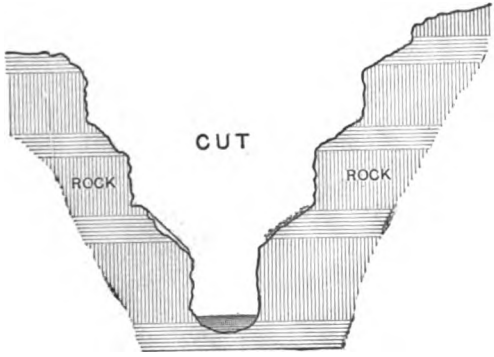
Third.—At about the middle of Marble Cañon, which has been called Point Retreat, the river makes a sharp turn to the left and then again to the right. At this point the marble stands up in vertical cliffs about 300 feet high from the water's edge, while the sandstones bench back in



slopes and cliffs to 2 500 feet in height. Just beyond this the cañon is narrowest, being a little less than 400 feet from wall to wall. The marble rapidly rises till it stands in perpendicular cliffs 700 to 800 feet high, with patches of broken down cliff forming small benches and talus slopes at their bases. See Photos. Nos. 324 to 338, inclusive.

A located line would pass through Point Retreat with a tunnel about 1 mile in length, cross the river with a single span (Photo. 330) from cliff to cliff, and through the opposite point with a shorter tunnel, coming out upon the same general class of cliff bench work. This section of some 10 miles (two above Point Retreat) in Marble Cañon is, in fact, the most difficult to locate, and perhaps the most expensive 10 miles to construct anywhere upon the whole river. It is necessary to cross the river for the first and only time. The closeness of the marble cliffs offers a very suitable place for a single span bridge. Although expensive, a permanent and first-class line can be located through this 10 miles. Beyond this section of tunnels and bridge the same cliff bench work continues for about 12 miles, with a tunnel through Point Hausbrough, saving a mile in distance. (See Photos. Nos. 332 to 348.) For the last 17½ miles above the Little Colorado the cañon widens, the marble benches retreat, new strata of limestone, quartzite and sandstone come up from the river, forming successive slopes and benches. The slopes are at an angle of about 40 degrees from horizontal, formed of thinner layers of rock in place, and covered with débris. Some of

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these slopes rise gradually from the water's edge to a height of from 200 to 1 000 feet. See Photos. Nos. 349 to 372, inclusive.

(5) The Grand Cañon.

The Grand Cañon of the Colorado has been pronounced by those who have carefully studied it to be "by far the most sublime of earthly spectacles." While this perhaps is the case, and while it is one of, if not the grandest gorge on the face of the earth, neither its grandeur nor its dimensions have anything whatever to do with the possibilities of building a railway through it. The fact that the Grand Cañon is 218 miles long—from the Little Colorado to the Grand Wash—and in cutting its way through the Kaibab Mountains the river has formed a chasm from 5 000 to 6 200 feet deep, and from 6 to 13 miles wide on top, has nothing to do with the case whatever. It is only the lower 200 feet or so of the cañon that a railway would have anything to do with directly, but in order to show the nature of this lower 200 feet, and its indirect connection with that above, a general description must be given of the whole gorge.

This cañon is divided into smaller sections by marked changes in the character of its formation as follows:

1st. 18 miles above the granite.
2d. 35½ " of granite.
3d. 35½ " to the Kanab Wash.
4th. 21 " of narrow marble benches.
5th. 54½ " to the lower granite.
6th. 39 " of granite.
7th. 14½ " to the end of the cañon.

This first 18 miles is in fact no cañon at all. The whole country is broken up and back for many miles from the river, and hills and knolls rise up between it and the outer walls beyond, these being separated by washes and gulches running in every direction.

Taken as a whole, the river runs through quite a wide valley. By an examination of Photos. Nos. 376 to 413, inclusive, and the table of classification, it will be seen that all classes of work exist, from earth to cliff bench work, but the larger proportion being comparatively light work.

The second section of 35½ miles of granite walls is the first real cañon portion of the great gorge. As compared with other well known

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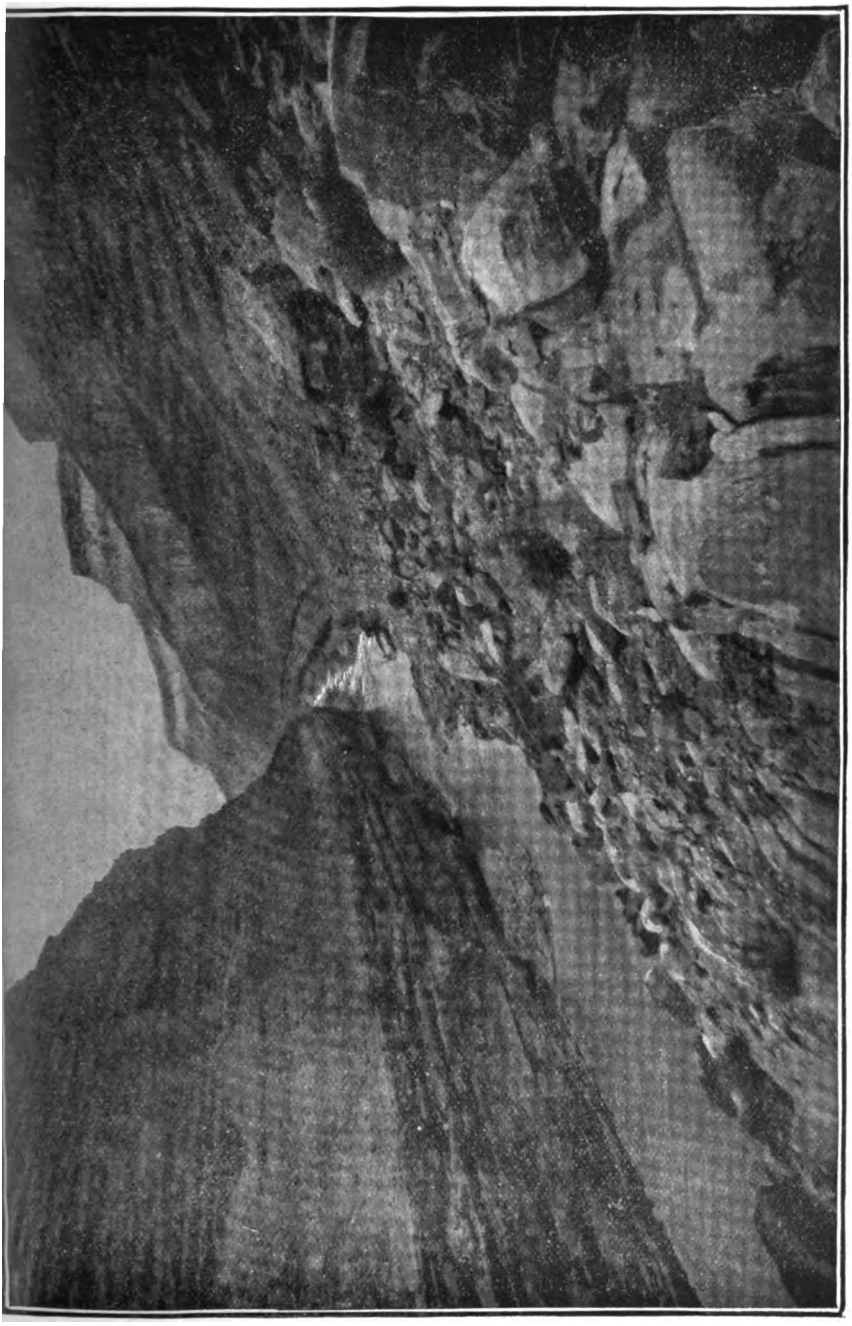

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cañons in the Rocky Mountains through which railways have been built —the Royal Gorge of the Arkansas and Clear Creek Cañon—this has a form peculiar to itself. Its walls start from the water's edge with generally a few feet—10 to 50—of vertical cliff, and then slope back in a ragged, irregular slope 300 to 1200 feet or more, at an angle varying from a few degrees to 45 degrees from vertical, with some small patches jutting out boldly into the river and towering hundreds of feet high, forming almost perpendicular cliffs, or, more accurately speaking, they form buttresses and towers to the general slope of the wall. Above the granite rise the sandstone, limestone, marble and higher sandstone ledges in cliffs, benches and slopes, stepping up and back till the chasm is from 6 to 13 miles wide. The lower granite walls, with which we have to do directly, are divided into two distinct classes. Those from the head of the gorge to the Bright Angel Creek are mostly of hard massive gray and red granite, and stand in solid, bold slopes. At the Bright Angel this granite changes into a softer kind, generally black and gray, much broken, cut into sharp pinnacles and crags, and seems as if formed of closely stratified slate. The slopes are much flatter and more cut up by side washes, and the whole country opens back into the widest portions of the Grand Cañon (Plates XXXII, XXXIII). See Photos. Nos. 414 to 540, inclusive. In this section were noted four short tunnels aggregating one-half mile.

The section of 35½ miles from the granite to Kanab Wash assume an entirely new form. The granite, except in a few patches, has sunk under the river, and the softer strata of sand and limestones, which form the great slopes above the granite, have come down next to the river, and rise from the water's edge in great talus slopes (underlaid with the same horizontally stratified rock in place) of from 300 to 600 feet high, at a general angle of 40 degrees from vertical. The high cliffs of marble and red sandstone bench back from the tops of these slopes. Although these outer peaks and cliffs have drawn in close upon the river, the cañon itself, that is, the inner gorge, is much wider than above, being measured from the tops of the great talus slopes. This section is shown by photographs (Plate XXXIV). See Photos. Nos. 541 to 597, inclusive.

From the mouth of the Kanab Wash for 20 miles down is perhaps the narrowest and deepest part of the great inner gorge. The lower sandstone and limestone ledges have sunken under the river, and the

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marble and upper sandstones have come close into the water. At the bottom the gorge is from 150 to 200 feet wide, and the river runs between vertical walls, but vertical only for about 80 feet up. The walls of this portion of the cañon (and it comes nearer being a true cañon than any other part of the river) rise above the water over 2 000 feet. They have the least slope, the benches are narrow, and the vertical cliffs between the benches higher than in any other section. For 19 miles, however, there is a continuous bench about 30 feet above high water and running almost parallel with the grade of the river. This bench is of solid imperfect marble or limestone, from 50 to 80 feet wide, and its alignment is such that it could be occupied the whole distance by tunneling some of the short, sharp points (Plate XXXV). Photos. Nos. 597 to 612 show its general character, and also the detailed work necessary for its occupancy.

In the next section of 54½ miles, which reaches to the lower granite gorge, there are several points worthy of note. The large proportion of "hillside slopes," over 37 miles (see Photos. Nos. 625 to 655—59 and 60), and also the large amount of "heavy talus slopes," nearly 15 miles. In fact, it seems a misnomer to call this section, except at a few points, a cañon at all. It is a valley broad and in some places flat, with gently sloping hills. In this section occurs that portion of the cañon so broken up and rended by volcanic action. Here also are found bold points of basalt which in a few places will require very heavy work.

The lower granite gorge, commencing about 2 miles above Diamond Creek, is 39 miles in length. This second gorge is almost of the same form and proportion as the great granite gorge at the head of the cañon, except that the granite, which is gray and black, with patches of red, rises higher than in the gorge above. Its walls slope from the water at angles varying from 10 to 50 degrees from vertical. It has not, however, the regular slope of the upper gorge. In its first half the buttresses of massive rock that jut out over the slopes into the river are bolder and more numerous, the granite is more compact and solid, and is cut through by more side cañons and washes. The second half is, as in the gorge above, shattered and broken, and has much flatter slopes. This gorge is shown in Photos. Nos. 661 to 732, inclusive.

In Photo. No. 710 is seen one of the most formidable points of granite on the river. The cañon is very narrow; the wall is vertical for 500 to 600 feet above the water. It is on a slight curve and long bend of the

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PLATE XXXII.
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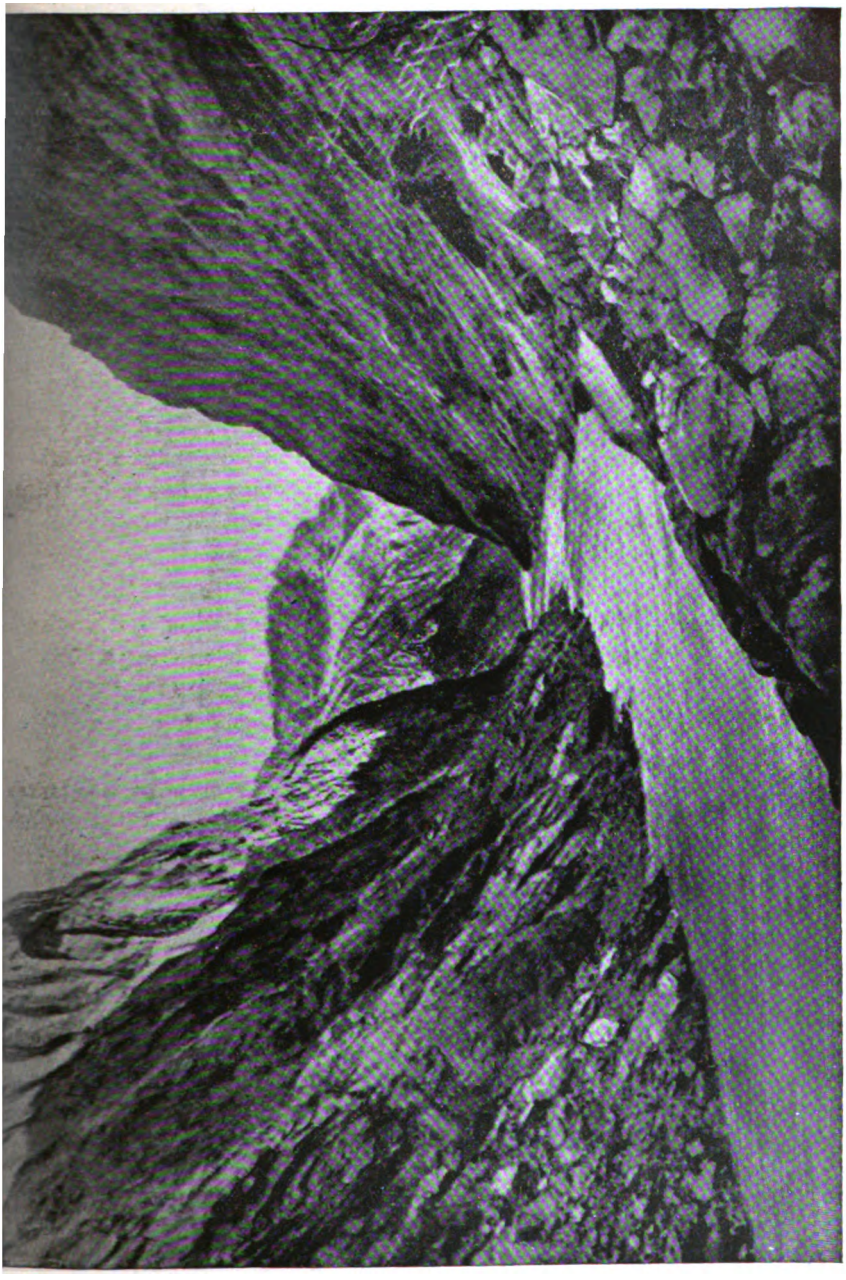

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river, and there seems no other way of rounding this point except by half tunneling. The rock is very compact and solid, and well adapted for such work. In the whole route, as shown by the preliminary survey, there will not be required more than one-half mile of this half tunnel work.

From the end of the granite to the Grand Wash cliffs the cañon is but a repetition of the lower end of Marble Cañon. The lower 100 or 200 feet of "the walls" consist of great slopes of the softer limestones, covered with loose débris. For miles these slopes extend to a height of 500 to 800 feet. In this section of the cañon are numerous streams of clear water coming in from immense springs in every direction and at different heights above the river. This water is strongly impregnated with carbonate of lime, and in running over the cliffs and slopes has left deposits of lime which in places stand up 100 feet high.

The following table gives the condensed statement of the classification of the Grand Cañon.

The distance through this cañon is 217.150 miles.

CONDENSED TABLE OF CLASSIFICATION—GRAND CAÑON.

MILES.	GENERAL CHARACTER OF WORK.	Photographs illustrating each class of work.
51.050	Hill side slopes, E. & L. R. & S. R.	Nos. 375, 553
43.750	Heavy and light talus slopes, L. & S. R.	Nos. 456, 735
11.600	Cliff bench work—sandstone	Nos. 542, 555
3.—	High cliffs and bluffs "	Nos. 393, 409
1.250	Solid rock slopes "	
.200	Heavy embankment in river	No. 414
19.000	Marble bench work	Nos. 602, 606
85.925	Sloping granite walls	Nos. 420, 469
.875	Tunnels (6).	
.500	Vertical cliff and half tunnel.	

(6, 7, 8, 9 and 10) LOWER OR VALLEY DIVISION.

At the Grand Wash cliffs the whole country drops, and has been washed away by the drainage of the prehistoric lake above to a much lower level, and the cañon ends in an open though hilly valley, with gentle, easy slopes compared with those the line has passed over. While the general character of the sections which have been numbered 6, 7, 8, 9 and 10, and which will be considered together, is that of an open valley, in places the granite rises up on either side of the river and forms a number of short "cañons," notably the Hualapai, Boulder, Black and Mojave. Generally the walls of these cañons are low and flat, and the rock much broken up.

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In two points, a couple of miles each, at the head of the Boulder and Black cañons the walls stand vertically from the water's edge, and necessitate several short tunnels in order not to obstruct the river, for it must be borne in mind that the Colorado is a navigable stream for large steamboats from its mouth to the Rio Virgin.

Very much the larger portion of the line from the Grand Wash to the Gulf is over open flat lands—and low bluffs and slopes of "gravel-wash"—many miles through the Colorado, Mojave and Chemhuevis valleys being perfectly level bottom lands of earth. (See Photos. 745 to 905, inclusive.)

The following general table gives the results of the whole survey in condensed form: (See next page.)

In order to grasp more clearly the question of practicability of a railway route through the great cañons and gorges of the Colorado River, the following comparison of the results of the survey is given in percentages, the distances and percentages being in round numbers.

The Cañon Division, from Grand Junction to the end of the Grand Cañon. Distance, 630 miles. Of this—

62½ per cent. is comparatively light work, earth, loose rock and talus slopes of loose and solid rock.

35½ per cent. is heavy, solid rock, and cliff bench work and sloping granite walls.

2 per cent. is tunneling and half tunneling.

Or, if the shorter line in Glen Cañon is used, this tunneling increases to 3 per cent.

The Lower or Valley Division. Distance, 408 miles. Of this—

75 per cent. is earth, and gravel work.

24½ per cent. is solid rock work.

0½ per cent. is tunneling.

"C." Methods of Handling such a Work.

It is in no way connected with the purposes of this paper, nor is it considered necessary to here insert the writer's estimate of the first cost of constructing such a railway as was contemplated in this survey. With the tables and descriptions given, and the full series of photographs, any one familiar with such work can very readily form an approximate estimate. A few facts that would bear particularly upon this first cost will be of interest to engineers. Through a gorge of such length it has been considered that work could only be carried on from

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PLATE XXXIII.
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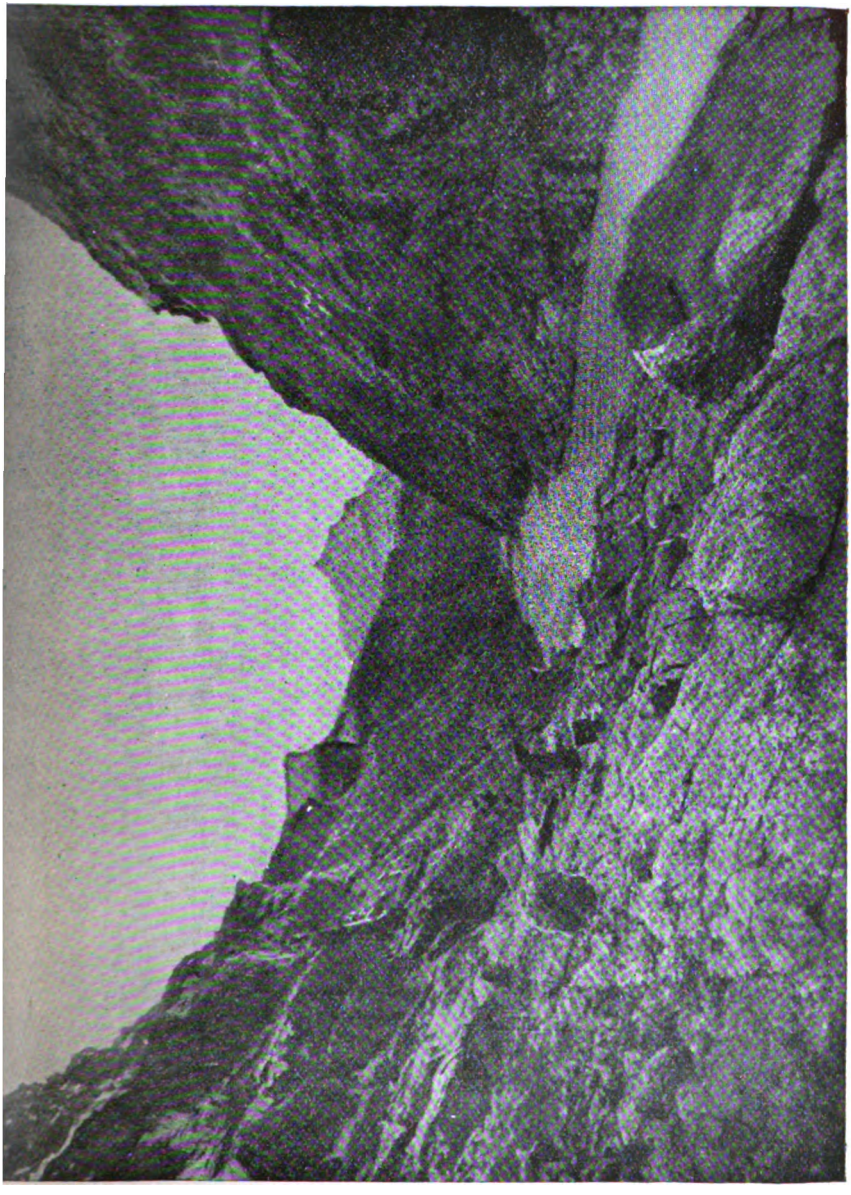

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General Table of Classification in Miles.

Divisions.	Level bottoms—earth and gravel slopes—"earthwork."	Hillside slopes, R. and L. R.	Rough talus slopes of L. R. and S. R., and boulders.	Smooth, solid rock slopes.	Cliff bench work—sandstone and marble.	High cliff work—not vertical.	Sloping and heavy granite walls.	Heavy through rock cuts.	Heavy embankment in river.	Rock bluffs in lower cañons.	Tunnels.	High vertical cliffs and half tunnels.	Totals.
Grand River	36.323	19.273	61.035	6.206	.....	.....	13.040	.....	.....	.....	4.110	.....	139.987
Cataract and Narrow Cañons	11.543	5.436	14.545	2.557	19.110	.....	.....	.....	.739	.....	.284	.....	54.214
Glen Cañon	57.270	10.928	55.926	21.284	.....	.....	.....	.501	6.553	.....	4.118	.....	156.580
Marble Cañon	.500	.....	26.510	.....	32.250	.....	.....	.125	.750	.....	2.375	.....	62.510
Grand Cañon	.....	51.050	43.750	1.260	30.600	3.000	85.925	.....	.200	.....	.375	.500	217.150
To Rio Virgin	26.500	.....	.....	.....	.....	.....	.....	.....	.250	10.750	.....	.....	37.500
To Las Vegas Wash	16.500	.....	.....	.....	.....	.....	.....	.....	.....	11.500	.500	.....	28.500
To Needles	67.750	.....	.....	.....	.....	.....	.....	.....	.....	27.750	1.000	.....	96.500
To end of Survey, near Gulf	197.255	.....	.....	.....	.....	.....	.....	.....	.....	47.500	.....	.....	244.755
Totals	413.641	86.687	201.766	31.297	81.960	3.000	98.965	.626	8.492	97.500	13.262	.500	1 087.696
If the long tunnel line in Glen Cañon is used = Totals.	403.947	86.232	191.359	27.825	81.960	3.000	98.965	.570	7.356	97.500	20.002	.500	1 019.216

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the two ends, thus increasing very much the time and cost of construction. The line would start from the town of Grand Junction, Colo., situated in a large and rich agricultural valley.

At this point it would connect with the Denver and Rio Grande Railway and the Colorado Midland, and all their railroad connections from the East, and the Rio Grande Western Railroad from Salt Lake City and the West. By its recently built line, the Rio Grande Western road comes down to the Grand River at a point about 22 miles below Grand Junction, and practically follows the river for a distance of 30 miles farther. Seventy-five and 100 miles down the river, are well settled agricultural valleys connected by ordinary wagon roads with Grand Junction and the railroad.

Going now for the purposes of transportation to the Green River, by the building of an easy wagon road of about 5 miles, connection can be made with the town of Blake on the Western Railroad, and a point on the Green River below the lowest rapid. From there to the head of the Colorado, a distance of 115 miles, supplies and materials can be transported by means of steam launches and flatboats. From a depot established on the flat at the head of the Colorado, these supplies can be carried at first by "jack-train," and afterward, as the work progresses, by wagon, up the Grand to meet those from above, and down the Colorado to meet those from Dandy Crossing, 54 miles below. The opening of these "jack-trails" could be accomplished with little expense, not only in this section, but through a large part of the Cañon Division.

Crescent Creek and Dandy Crossing are connected with the railroad by good, though sandy, wagon roads, 120 miles in length. From Dandy Crossing there is already a good trail, 20 miles, down to the placer mines. Supplies can be also carried down on flatboats through parts of Glen Cañon, as heavy boilers and machinery have so been carried as far as Tickaboo. There are also wagon roads down to the river at a number of places in Glen Cañon, such as at Smith Brothers', Hall's Ferry and other placer mines.

At Lee's Ferry the river is connected by good wagon road—90 miles—with Kanab, Utah, on the north, and Flagstaff, Arizona—190 miles—on the Atlantic and Pacific Railroad, on the south. Supplies can be carried up river from Lee's Ferry for some distance by steam launch and down into Marble Cañon by jack-trail. A wagon road now exists to the brink of Marble Cañon, about 40 miles below its head, at a point oppo-

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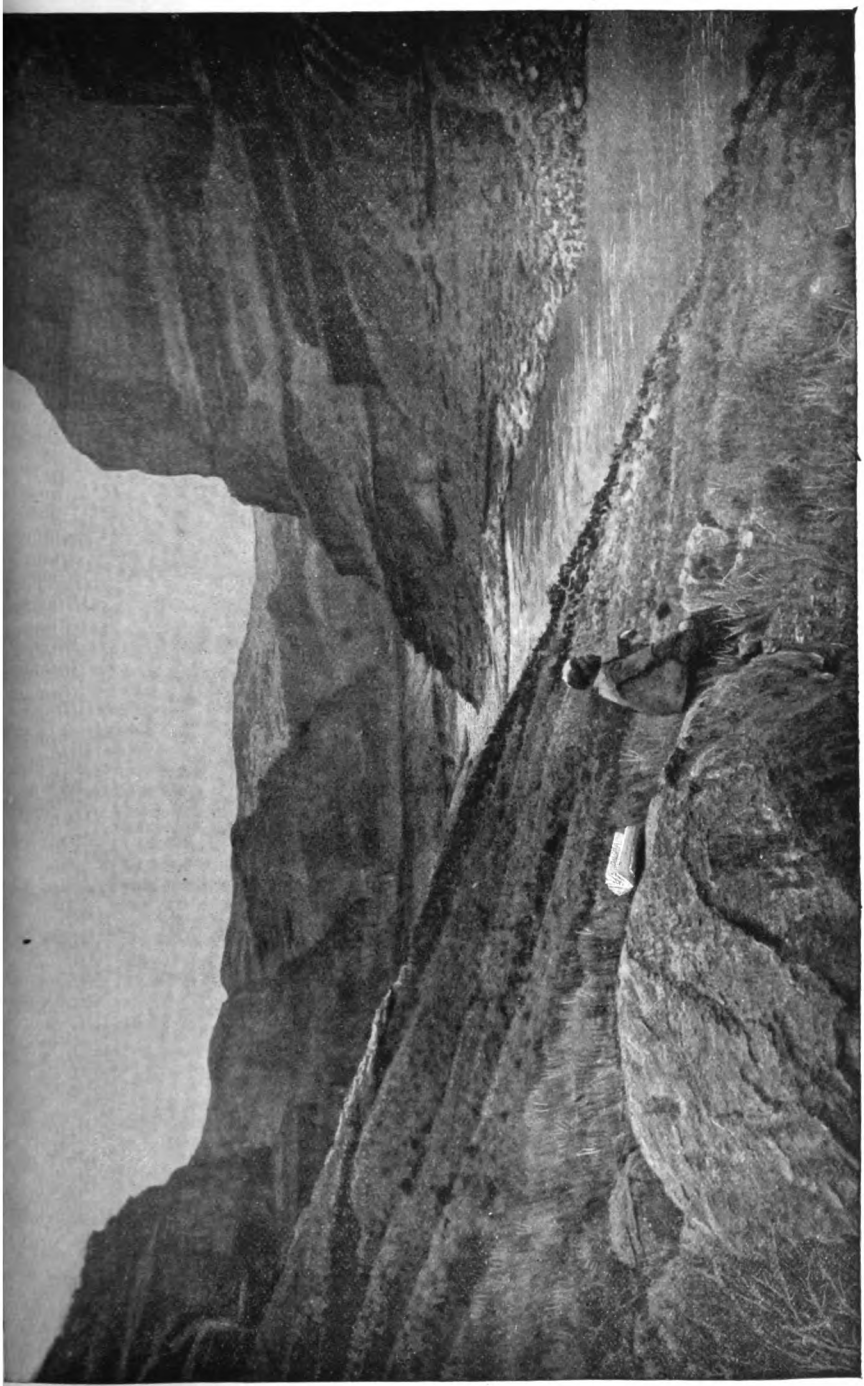

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site Point Retreat, and an easy trail can be cheaply made down to the river. Such a trail already reaches the river by way of the Nan-Co-Weap.

At the head of the Grand Cañon there is a wagon road to the top from the railroad at Flagstaff, and a trail down to the river, just below the Little Colorado. This with some expense could be so improved as to deliver supplies for the lower end of Marble and upper end of Grand Cañon. Through the Grand Cañon, on account of its depth, there exist as yet but few ways of reaching the river. There is a good trail down Kanab Wash—only requiring some improvement to be used for supplying the whole section for 35 miles above.

At the mouth of Diamond Creek there is a good wagon road from the river to the town of Peach Springs, 24½ miles distant, on the Atlantic and Pacific Railroad. Diamond Creek is 53 miles from the mouth of the Grand Cañon. Below the cañon is the old road and ferry at the Grand Wash, and the roads and ferries at Scanlon's and the Rio Virgin connecting both with the Atlantic and Pacific Railroad and the rich country around St. George and St. Thomas and that of southwestern Utah.

Thus it will be seen that in the Cañon Division work could be carried on simultaneously at at least forty different points, while below the Rio Virgin supplies and material could be delivered at any point by the steamboats now running on the river. The only additional expense in construction would be the extra cost of transportation and preparing trails in the Cañon Division, while on the other hand there are a number of reasons why the cost of construction would be much reduced.

For the first three items in the general table of quantities no new factor of either increase or decrease of expense would enter. Of the 403 miles noted as "earthwork," the greater part is light plow and scraper work. The 86 miles of "hillside slopes" and 191 miles of "talus slopes" would be very largely side hill cut with no haul for waste.

The next seven items in the table consist chiefly of solid rockwork. So much of this—including the 99 miles of granite walls—stands in slopes approaching 40 degrees from vertical, that it would be the cheapest kind of rockwork. While from the nature of the slopes the road-bed would be almost wholly in excavation, only a minimum cross-section would be necessary and the waste thrown directly into the river, thus reducing to a minimum the handling of the rock, and the number of men and animals required on the work, so that this large proportion of solid rockwork would not nearly approach the cost of such an amount

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in a level or settled country. The heavy embankments in the river would be very expensive to build. The tunnel-work would only be affected in cost—by the inaccessibility of the country. And the total amount—20 miles—when is considered the large amount of curvature and distance (42 miles) saved, is not far in excess of such work on a number of already constructed roads.

It is the opinion of the writer that one feature of modern machinery might be used with great economy on this rockwork and tunneling—that is, electrical drills—by employing the rapid current, falls and cataracts of the river to generate the electricity.

No estimate of the cost of such a plant has been made; but it is believed that with current wheels and movable power stations great saving would result not only in the work itself, but very largely in the reducing of the number of men employed and the transportation of material and supplies for their subsistence.

"D." The Nature of the Line Itself.

First.—The Grades.—Starting from the town of Grand Junction it is possible to build a railway with a continuous down grade the whole length of the line to the Gulf of California, and to have a returning grade with a maximum not exceeding 12 feet per mile, except for a distance of 20 miles, and then it need not exceed 20 feet. It would be neither economical nor advisable to construct such a line. From the surveys made it is believed that in no place would it be necessary to use a grade in either direction to exceed one-half of 1 per cent. or 26.4 feet per mile. A careful location may make it advisable to increase this maximum.

The following table gives the average fall of the river through the various sections. The distances are by the river and not by the survey: (See next page.)

Second.—The Alignment.—The Grand and Colorado Rivers are large streams, flowing, as regards their detail, in long sweeps and gentle curves. This is well shown in the series of photographs. Reference has already been made to the sharper bends requiring tunnels. From the surveys made it is believed that an economical line can be located with curves not to exceed 10 degrees, or, in other words, with a radius of 573 feet.

Third.—The Nature of the Road-bed and Maintenance of the Permanent Way.—Along those portions of the route where it would be necessary to construct the road-bed upon low, flat bottom lands, the river is broad and

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PLATE XXXV.
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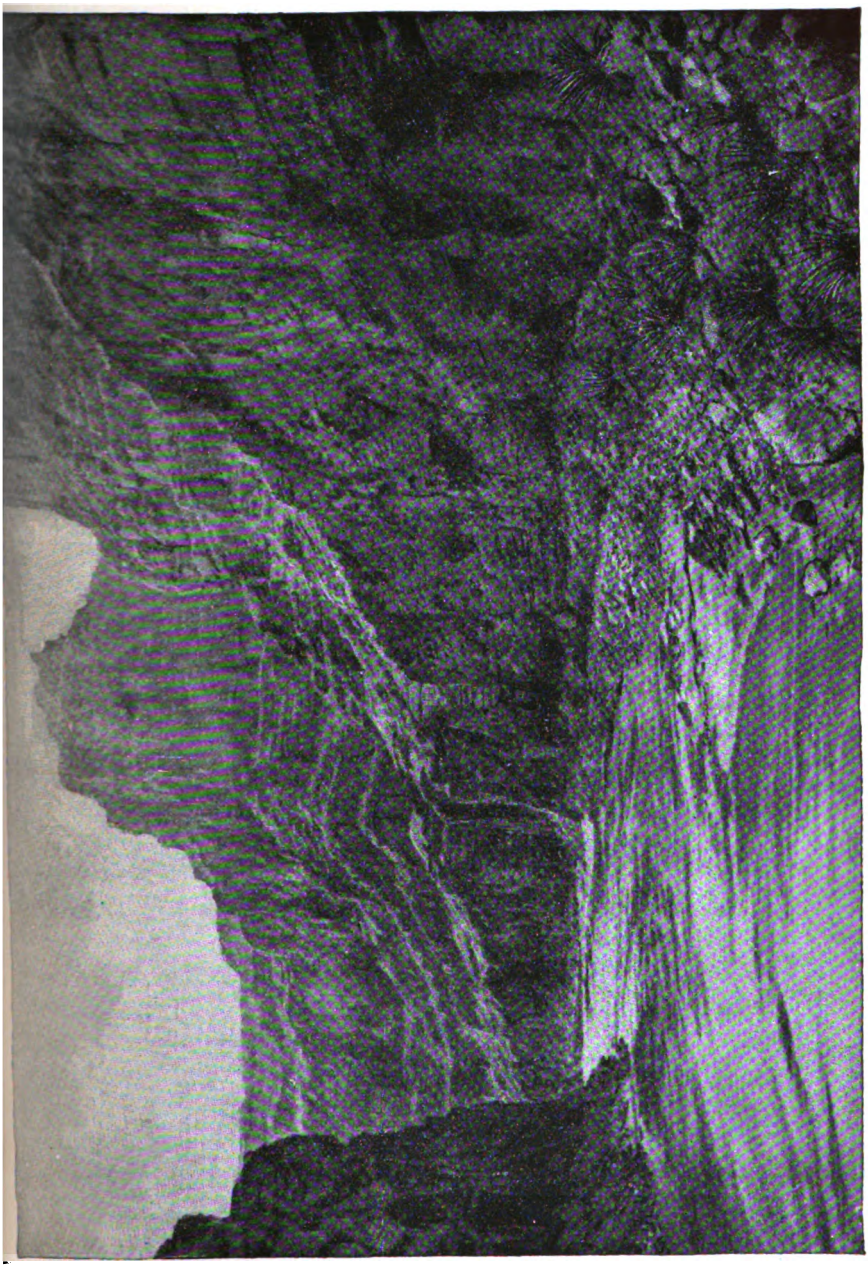

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shallow and its fall is very light. These flats are above all ordinary high water; the line can be securely built across them, and rip-rap and material for slope wall protection can be easily and cheaply procured. In the same section, as explained above, where it is necessary to build some miles of the road-bed in the river, under the cliffs, the same facts exist, and the difference between low and high water is a minimum. These embankments would be built almost entirely of solid rock. Along the solid rock benches and sloping walls of sandstone, marble and granite the road-bed would be almost entirely in excavation. One notable fact is the almost entire absence of great slopes of loose rock or slides, which are generally so difficult and dangerous to handle. It is almost universally the case, as is clearly shown by the photographs, that these so-called talus slopes are composed of horizontally statified rock in place, covered with a thin layer of loose débris, so that the road-bed in almost its whole extent through the Cañon Division would be built on solid and durable foundations.

Divisions of the Grand and Colorado Rivers.	Distances in miles.	Total fall in feet.	Average fall per mile.
Grand Junction to head of Colorado	162.4	650	4.00
Cataract and Narrow Cañons	55.	355	6.45
Glen Cañon	150.	325	2.17
Marble Cañon	65.2	510	7.82
Grand Cañon to the Granite	18.2	110	6.04
" " to the Granite Falls	10.	210	21.00
" " to Powell's Plateau	26.4	320	12.13
" " around Powell's Plateau	10.8	100	9.26
" " to head of Kanab Division	4.8	50	10.42
" " Main Kanab and Uin-Karet Divisions	65.2	310	4.75
" " Sheavwits Division to Granite	12.	70	5.83
" " to Diamond Creek	18.	210	11.66
" " in Granite below Diamond Creek	7.2	25	3.47
" " " " " "	10.8	100	9.26
" " to Grand Wash	35.	175	5.00
From Grand Wash to Las Vegas Wash	68.	300	4.41
Total to Las Vegas Wash	719.	3 820	5.31
From Las Vegas Wash to Gulf or end of Survey*	425.	700	1.64
Grand total	1 144.	4 520	4.00

* This last distance does not take in all the many windings among the Sand Islands in the lower part of the river.

The features peculiar to this route and affecting the maintenance of the permanent way, remain to be considered. They are the drainage across the road-bed and the possible effect of loose material from the cliffs above. During the survey, as stated above, notes were taken of all

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waterways from 10 feet upward, and estimates made of all other drainage required. As might be expected from the formation of the country, many side streams and cañons enter the main gorge. Many of these a little way back from the main chasm are from 1 to 3 miles wide on the top; but they enter the Colorado through the lower and harder strata of compact sandstone, marble and granite, and through narrow channels that can be spanned by from 50 to 100 foot bridges. These are clearly shown by many of the photographs taken. There are some exceptions to this class where the mouths of the side gorges have been washed out into wide flats. Such are the Nan-Co-Weap and Kwagunt branches of the Marble Cañon. These do not present any more difficulties than the crossing of any wide water course not in such a cañon. Of the smaller waterways the granite and marble give solid abutments on either side, and the channels are deep and narrow.

There is one feature of the drainage peculiar to such cañon-work; that is the small streams that come over the tops of the cliffs. Where the cañon is wider and the talus slopes exist at the foot of the cliffs, these small streams have worn out basins back, next to the cliff, into which they fall, and from which they can be carried under the road-bed in the natural channels that now exist. In other portions, where the road-bed would be cut along the benches, these small streams (which only flow during storms) and the débris brought down with them can be carried over the road-bed by a simple device of overhead drains. Taken as a whole the drainage does not present any insurmountable difficulties or any very difficult problems not found upon other mountain roads.

The question of falling rock and loose material from above is not different here from what obtains on hundreds of miles of operated railroads through the Rocky Mountain region. The immense width of the cañon on top prevents the great mass of rock loosened from above by storms reaching the inner or lower gorge in a way to do any damage. The lowest gorge is of a harder material and not so easily affected.

One feature largely affecting the maintenance of way, and also the traffic of such a road, is the almost entire absence of snow and hard freezing from so large a portion of the line in the great cañons. During the winter of 1889–90, the expedition experienced only two hours of snow storm at the level of the river, while the whole upper plateau was covered with from 3 to 6 feet of snow. It may be remembered that the winter of 1889–90 was one in which the transcontinental railroads through the

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Western mountains suffered more from snow blockades than they had for years previous.

A brief abstract from the register of the thermometer from November 28th, 1889, to May 1st, 1890, may be of interest.

Sections of the Route.	Max.	Min.	Average.		
			6 A.M.	Noon.	6 P.M.
	Degrees	Degrees	Degrees	Degrees	Degrees
From Green River to Dandy Crossing, November 28th to December 10th, 1889	65	10	34½	52½	45½
Dandy Crossing to Lee's Ferry and to middle of Marble Cañon, Dec. 10th to Dec. 31st, 1889	66	30	39½	54½	47½
Month of January, 1890, in lower Marble and Grand Cañons	68	24	37⅝	48	43
In Grand Cañon, Feb. 1st to March 17th, 1890	82	29	47	61½	55
End of Grand Cañon to Fort Mojave, March 17th to March 23d, 1890	76	42	52	66	61
Fort Mojave, The Needles, Yuma and the Gulf, March 23d to April 30th, 1890	93	40	52	83	75

Taking, then, the whole line into consideration, when once properly built it would not only not be highly expensive to maintain, but in many points would be far below the average of mountain railways, and in the matter of winter transportation would have advantages over any line crossing the country from the Rocky Mountains to the Pacific Coast. The scarcity of water through this same section that so affects the present transcontinental roads would be entirely done away with, the supply from the river being the best possible when allowed a little time for the settlement of the sand.

Second.—The Advisability of such a Railway. Through Passenger and Freight Business.

In the investigation of that part of the subject under consideration—the advisability of opening a new line of transportation across the western half of the continent—the engineer is called upon to go beyond the limit of what is ordinarily called the science of engineering and to answer the question, Will it pay? There are, however, a number of elements entering into this question which are outside the scope of this paper; but the more limited inquiry of what business would such a railway find to do, is one entirely proper to be answered by the engineer who examines the country for the purpose of gathering all possible information bearing upon the final success or failure of the enterprise.

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One important item must be first made clear. It will be remembered that the original object in the projection of this railway was to connect the coal fields of Colorado with the Pacific Coast. These very extensive coal fields are already connected with Grand Junction by several railroads. The scarcity of cheap fuel on the Pacific Coast is well known. The much larger proportion of the fuel used there is imported from foreign countries. The question first proposed was how to connect these two distant points with each other, so as to cheaply and profitably transport the abundant fuel of the one to the extensive fuel market of the other. The nature and extent of this coal trade will be considered further on.

For the purposes of such a business as the transportation of large quantities of coal, coke and other fuel, it was thought best to reach as quickly as possible tide-water at the head of the Gulf of California. The great difficulty and danger of navigating this gulf, and the almost impossibility of securing a safe harbor at its northern end, puts this as a terminus out of the question.

Without further explanation, it will be considered that the road would be built down the river to the neighborhood of Yuma, and then, crossing the Sierra Nevadas at an elevation of about 2 600 feet, terminate at the extensive land locked harbor of San Diego.

It was also first expected to secure all fuel for the Pacific Coast in Colorado. Other coal fields, however, exist many miles nearer that market, that would be reached by and have all the advantages of this low grade means of transportation. It will therefore be considered that the road for which it will now be proper to look up its present and future business consists of a line from Grand Junction to the port of San Diego, with branch lines to the various coal fields, and especially one up the Rio Virgin to the extensive coal deposits of southwestern Utah.

First.—Passenger Traffic.—It will be conceded that any road built across our Western country would get a share of the passenger travel between the East and the West. (This may be also said of the through freight traffic.) As to the passenger business of this proposed road being in excess of all other lines across the continent, one or two short quotations bearing upon this subject will be sufficient: "Those who have long and carefully studied the Grand Cañon of the Colorado, do not hesitate for a moment to pronounce it by far the most sublime of all earthly spectacles. If its sublimity consisted only in its dimensions, it

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could be sufficiently set forth in a single sentence. There are in the world valleys that are longer and a few which are deeper; there are valleys flanked by summits loftier than the palisades of the Kaibabs, still the Grand Cañon is the sublimest thing on earth. It is so not by virtue of its magnitude, but by virtue of the whole, its ensemble."

"To the conception of its vast proportions must be added some notion of its intricate plan, the nobility of its architecture, its colossal buttes, its wealth of ornamentation, the splendor of colors, and its wonderful atmosphere. All of these attributes combine to produce a whole which at first burdens and at length overpowers."

What is likely, then, to be the attractiveness of such a route for passenger travel? Remembering that this line, passing through this wonder of the world, would forever have a monopoly of such travel—since no other line could parallel it—it is believed that the passenger travel alone would be sufficient to pay interest on a very large extra cost of construction.

Second.—Freight Traffic.—More and more every year the question of time enters into the transportation of through freight from East to West and from West to East. At no distant day the consideration of how to attain quicker and cheaper transportation of through freight will be one of vital importance to all the transcontinental roads. It is the opinion of the writer that the solution of this question lies in the railway of the future—the railway of lighter grades, lighter curves, lighter rolling stock, lighter trains, more of them and quicker time. What a field of investigation this thought opens up! Even now the transcontinental roads are considering what effect the opening of the Nicaragua Canal will have on their future business in cost of carrying through freight.

What, then, would be the advantages of a railway line, which over the very roughest portion of the section traversed by all these roads would have 1 000 miles of its distance with grades not to exceed one-half of one per cent. or 26.40 feet per mile? And what advantage would it also have for the whole year's business, when this 1 000 miles, the very worst for winter travel, would be almost entirely free from frost and snow? Such a line would of course cross the Sierra Nevadas, but so far south and so low that it would have but little to fear in winter.

It is here only estimated for a line to Grand Junction and across the Rocky Mountains by existing roads. The crossing of the Continental

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Divide has never yet been accomplished so as to secure the best advantages. This cañon road carried up the Grand River through Middle Park and across the range on a line located by the writer more than ten years ago, it is believed would secure many advantages and reduce the rigors of winter travel and transportation to a minimum.

The following profile (Plate XXXVI) on a reduced scale and those of existing roads, taken from their published statements, together with the table of comparative distances, illustrate this part of the subject.

Railroads from Denver to— (Distances in Miles.)	San Diego.	San Francisco.
Union and Central Pacific	2 035	1 516
D. and R. G. and Central Pacific	2 185	1 666
Atchison, Topeka and Santa Fé	1 638	1 724
Denver, Colorado Cañon and Pacific Route	1 472	1 961

Distances from Chicago to—	San Diego.	San Francisco.
Northwestern, Union and Central Pacific	2 933	2 419
C. B. and Q., D. and R. G. and " "	3 208	2 694
Atchison, Topeka and Santa Fé	2 347	2 577
Southern Pacific and "	2 650	2 805
Northern Pacific	3 847	3 238
Denver, Colorado Cañon and Pacific Route	2 500	3 009

Local Business.

Woe to the railroad that has nothing but through passenger and freight business to depend upon! In order to understand the possibilities of the immediate local freight business, and what may be reasonably expected as the future development of such a trade, it will be necessary to rapidly glance at the whole extent of country along the route. This becomes more necessary when it is remembered that this whole section has generally been considered totally barren of any such possibilities.

The initial point of this proposed railway is at the town of Grand Junction, Colorado, the largest and most prosperous agricultural center in the western part of the State. Mesa County, in which it is situated, and which at this point contains a valley of nearly 500 000 acres of the finest agricultural and fruit land, has also varied and extensive mineral resources. Its coal fields are considered inexhaustible. Many large bodies of coal have been discovered and developed. Some extensive deposits lie in close proximity to this proposed route. Many of the

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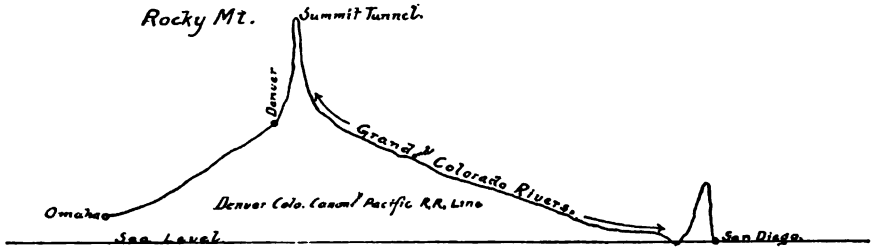
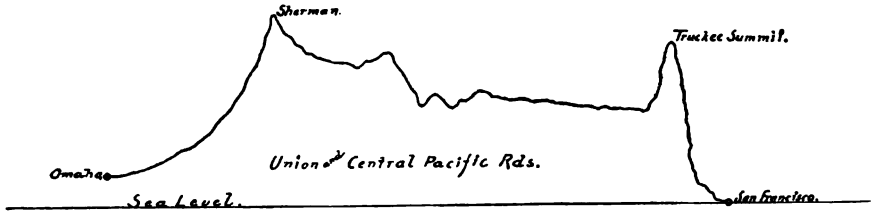
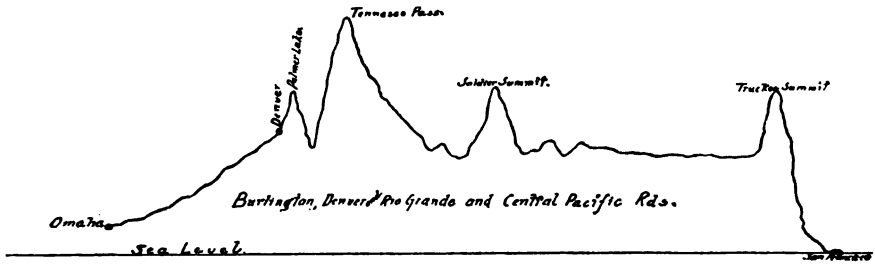
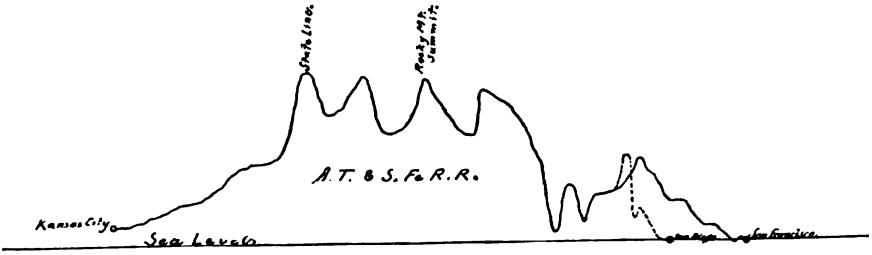
PLATE XXXVI.
TRANS. AM. SOC. CIV. ENGS.
VOL. XXVI, NO. 523.
STANTON ON CANONS OF THE COLORADO.









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tensive anthracite coking and steam coal fields of western Colorado
e connected with Grand Junction by rail. This point is practically the
stern terminus of all the railroads coming into Denver from the East.
ne section of country around Grand Junction is rapidly settling up.
llowing the Grand River, 75 to 80 miles, is the Little Castle Valley.
is, with the Little Grand and other valleys along this river, form a
ge tract of rich, though only partially developed, agricultural land.
thracite coal has been discovered within 7 miles of Richardson, on
e Grand, and other large deposits of coal have been located at various
ints. The present cattle interest in this section is considerable. It is
imated that there are in the valleys and on the mountain ranges
butary to the Grand and Colorado rivers 1 500 000 head of cattle,
tween this point and the Kaibab Mountains.

The line from here to Dandy Crossing, through the lower part of the
and River and through Cataract and Narrow cañons, does not en-
unter any productive land near the river. Back at the heads of many
the side cañons are large tracts of grazing land, many of them occu-
ed by herds of cattle.

From Dandy Crossing to Lee's Ferry, a distance of 150 miles, the gold
acer deposits are almost continuous the whole way. In past ages
ile the river was cutting its way down through the red sandstone of
len Cañon, there were deposited on the successive levels of the stream
st beds of fine and coarse gravel, into which has settled great quanti-
s of fine gold dust. Whence this gold has come is a question on
ich there is a great difference of opinion; this is of but little import-
ce to the present investigation. These beds of placer gravel are found
l along this whole extent of 150 miles, on the benches of the cañons, at
rious heights, some being 100 feet and more above the level of the
ter, while the larger amount of the deposit is from 10 to 50 feet above
w water, with very extensive bars in the bed of the river, which are
erflowed during high water.

From personal examinations the writer considers these gold deposits
t only very extensive, but very rich and valuable. On account of
eir situation, so much above the level of the stream, and the fact
at through this section the river has so little fall, these bars can only
worked by using more machinery and much more extensive plants
an are usually necessary in such cases. This is the reason why these
rs have not been more extensively worked up to this time. With a

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railroad through this cañon these mines would be largely and profitably handled. The transportation of machinery, lumber, mining supplies, provisions and all the traffic incident to such a population as would be engaged in even placer mining, extending as it would along the river for 150 miles, could not but create quite a local business through this section. From some measurements and estimates made on particular bars, it would be difficult to wash out these deposits within the next 100 years.

Through this section of Glen Cañon at various distances from the river many other valuable mineral deposits have been discovered. These are as yet undeveloped. The mineral and coal deposits of the Henry Mountains are within 25 miles of the river. Coal has been found at several other points from 6 to 20 miles back in the side cañons above Lee's Ferry. In the neighborhood of the San Juan and Escalante rivers are well defined deposits of petroleum.

By a line up the San Juan River it is possible to make connection with the rich Montezuma Valley, and the coal and other mineral deposits of that part of Colorado. At Lee's Ferry connection is made by wagon road with the agricultural and grazing country of northern Arizona and southern Utah. This is largely subject to the necessities of irrigation, yet great quantities of oats and barley are successfully raised without irrigation. It is considered that all the cattle, sheep, wool and other products of that section—and they are of no mean account—would be taken to the river for a means of transportation rather than driven to the railroads running north. From Lee's Ferry, for some 50 miles down through Marble Cañon, there would not be expected any local business, except perhaps the shipment of building stone. The great variety of building stones through all the cañons, sandstones, limestones, marbles and granites, would undoubtedly create some considerable business for the road. The fact that the largest building being erected to-day in the City of Denver is being built of the same stone that forms the upper walls of the Grand Cañon, and that this stone, shipped from Flagstaff, Arizona, is carried as far east as Kansas City and Chicago, shows that a traffic for this road in building stones is not beyond the probabilities.

Some 15 miles above the head of the Grand Cañon, begins the great mineral belt that extends all the way through and on either side of this great chasm. In the lower end of Marble Cañon, as the sandstones

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limestones rise and recede from the river, there come up other
a of limestone, sandstone and quartzite which lie above the granite,
between these are extensive veins of mineralized matter. At the
of the Grand Cañon, above the granite, are immense veins, mostly
zonal, of iron ore and silver, lead and copper deposits; and at one
t a large bed of roofing slate has been located. As the granite rises
tz veins of various sizes are seen in every direction and running at
y angle, while the horizontal veins of mineralized matter—silver,
copper and iron—above the granite extend all the way to the Grand
h cliffs.

Through the whole cañon the writer saw and took samples from
erous and extensive croppings of silver, lead, copper and iron ores,
deposits of salt and mineral paints. While it would not be proper
ate positively—from so short and rapid an examination—that these
s and deposits are rich in precious metals, yet with such extensive
s and such fine croppings it is not unreasonable to conclude that
h real value does exist. The mineral lies from 100 to 1 000 feet
e the river, and consequently from 4 000 to 5 000 feet below the
eau country. While it would be a very easy matter to bring this
down to the river to a means of transportation, it is practically
ssible to take it up to the plateau above. Here again, then, with a
oad through the cañon, would be developed a large local business
only in the transportation of ores out, but of machinery, supplies,
visions, etc., in. One other item through the Cañon Division would
ish some little, at least, local business—the timber that covers the
eaus and Kaibab Mountains. As the demand for tie timber grows
e and more every year, this timber, though it is far above the river,
and will be brought down, rather than be carried up to a means of
sportation.

Below the great cañons is found an entirely different country. Taken
a whole it is a broad and open valley. At the mouth of the Rio
gin connection is made with the old settled and richly cultivated
ntry of southwestern Utah. Along the valley of the Rio Virgin and
tributaries are rich agricultural lands. At the mouth of the river
raised to-day, besides grain, all the varieties of European and native
pes, peaches, plums, pears and nectarines, limes, pomegranates, figs
l almonds.

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The valley of the Colorado from the Virgin to Yuma must be considered under two heads:

1st. As a mineral country.

2d. As an agricultural country.

About 6 to 18 miles up the Rio Virgin are located great mountains of rock salt, much of it pure, but the greater part of a pinkish purple color. The lower valley of the Colorado is flanked on either side by ranges of hills and mountains. These ranges are cut in every direction by mineral veins. Many of them that extend from the tops of the mountains down to level of the valley can be seen for miles. This country, as a mineral section, has been known for years. The minerals are gold, silver, copper and lead. The old mining camps in Gold Basin, Lost Basin and El Dorado Cañon are well known. The mines in El Dorado have been worked, more or less continuously, since 1861, and the gold and siver mill at that point since 1864. At some points the ore is very rich. The greater quantity, however, is of a lower grade, and will not bear the cost of mining and milling, on account of the scarcity and high price of fuel. At El Dorado, coal brought from Galup, New Mexico, by rail and up the river by steamboat, costs $14 per ton. With a railroad through this valley that could deliver cheap fuel, hundreds of developed mines, that now stand idle, could and would be profitably worked, and hundreds of others would be opened and developed.

In the next place, with a railroad built, and only a portion of the possible mineral development and operations under way, hundreds and thousands of acres of the rich agricultural lands of the Colorado, Mojave and other valleys of the Colorado River and its tributaries would be under cultivation, for there would be a ready and profitable market close at hand. These valleys would be cultivated not only in the ordinary farm products, but in a great variety of semi-tropical fruits. At the present time some of the accessible tributaries of this river are being set out in groves of oranges and lemons.

One more item of immediate and future business remains to be considered—the coal trade. With the great advantages of this proposed road as to distance, winter travel, and especially grades, the better classes of fuel, such as anthracite coal and coke, could be profitably carried from Colorado to the Pacific coast. The greater quantity of fuel, however, would be taken from nearer points, notably from the coal fields of southwestern Utah.

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his fuel, transported at the most economical rates, over a down
e road, would first be delivered to the mining and milling opera-
all along the Colorado River. At The Needles it would reach the
tic and Pacific (the Santa Fé) system of railroads, for their own
nd for transportation into California at a much lower price than
can transport fuel from New Mexico. At Yuma the same would be
ase with the Southern Pacific Company, for their own use, and for
nent both east and west over their lines.

t San Diego the whole of Southern California could be supplied
fuel, which it now so much needs, and from there transported by
r as far north at least as San Francisco.

he following tables give the coal brought to the various ports of
ornia in 1889 and first six months of 1890:

COAL RECEIVED AT SAN FRANCISCO.

FROM WHERE BROUGHT.	Year of 1889. Tons.	First 6 mos. 1890. Tons.
Sound	372 514	195 375
	49 500	30 330
Diabalo, California	38 100	13 200
n, New York and Baltimore	18 950	22 197
Columbia	383 906	179 808
h, Scotch and Welsh	45 617	13 372
	1 340	4 850
lia	303 285	84 543
Total	1 213 212	543 675

COAL RECEIVED AT SAN DIEGO.

FROM WHERE BROUGHT.	Year of 1889. Tons.	First 6 mos. 1890. Tons.
lia	45 780	18 115
Columbia	10 738	1
New Mexico	1 859	1 052
re, California	358	141
Total	58 735	19 309

here was also received at the port of San Pedro, in 1889, a total of
0 tons, and during the first six months of 1890 about 30 500 tons.
ven though coal and coke would be delivered by this road, and sold

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at from one-half to one-third of its present market price, it is not expected that at once, or even ever, it could take possession of the whole of this trade; yet it is not impossible that with this great difference in price a very large amount of this fuel traffic would very shortly be transferred to the port of San Diego, the terminus of this proposed line. What the future development of this coal trade might be can only be hinted at. With cheap fuel, and an unlimited and certain supply, there is no question but that the consumption of such fuel on the whole Pacific coast would be largely increased, not only in manufacturing, but in many other ways. The opening of the Nicaragua Canal would create a demand farther south, not only for ocean steamer use, but for local purposes. From no other source could this so easily be procured as from this proposed road at the port of San Diego.

In conclusion, from the facts here set forth, it is believed that there is awaiting the opening of such a road as is herein described both a local and through business, in excess of what was ready for many of the lines when built that now are in operation through the western part of the United States, and that a local business would be developed far larger than can ever be expected on some of our great Western railways.

It is hoped that in this description there has not only been shown the entire practicability of the cañons of the Colorado River for railway purposes, both from an economical as well as a purely engineering point of view, but that there has also been some light cast upon the nature and possibilities of a portion of our great Western empire which, to many, has been less known than the heart of Africa.

DISCUSSION.

ALFRED FRANCIS SEARS, M. Am. Soc. C. E.—I have found this paper by Mr. Stanton a most intensely interesting study. It will prove also a much more valuable document to engineers engaged in the location and construction of cañon lines—and there are many such—than at first view may be apparent. As an engineer's story it contains the excitement of heroic adventure; it is a modest account of extraordinary pluck and nerve. When Mr. Stanton's outfit had been wrecked, his leader and two assistants drowned and the entire party exposed to the perils of starvation, an opportunity for retreat to safety with honor presented itself; but this chance was availed of, not for escape, but to refit and

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in venture on the perilous voyage down an unknown cañon, simply he name of duty to the cause of progress and science. Such persist- e in the exploration of the Colorado Cañon entitles Mr. Stanton and assistants who remained with him to the sincere admiration of his fessional brethren.

My object in entering on a discussion of this interesting paper is to ent some facts from my personal experience that should be of value orks of this character. I think, if I were called on to make a sur- of such a stream as is here described, I would take advantage of some he work I have witnessed in the west branch of the Hudson by the bermen who drive that river in the spring. Some of the members of Society who have lived or hunted in Essex and Warren counties have btless seen boats go through the horserace near Johnsburg, a deep, ow cañon with vertical walls of great height, and have seen the e men pass the rapids above and below that point. I believe I ld seek that class of skilled labor at almost any expense if I had o such work in North America. The experience of men who can t rapids in a light canoe is very different from the genius which carry a heavy boat through such peril; a kind of work with which river drivers of mountain torrents, and only they, are familiar.

I do not know if my experience is unique, but it has been my fortune, ainly not a common one, to engage in the construction of a line of oad of which I have been obliged to witness the almost entire demo- n by an extraordinary series of cataclysms. It was a line situated in ñon very similar in character to this described by Mr. Stanton, and had already graded 125 kilometers and laid track, and were running ns over one hundred and eight of them, when the catastrophe occurred which there have not been left in all that distance five consecutive kilo- ers over which so much as a hand car can be moved, save in the flat on between the coast and the precipitous cañon, where some 50 kilo- ers were repaired and restored to traffic. As teaching the dangers uch a location, the uncertainties of construction and durability of k and the class of accidents to which such work is exposed, I be- e some statements from the history of the short lived line in the ey of the Rio Santa of Peru, known as the Chimbote and Huaraz lway, must be interesting and valuable to the profession. Mr. O. Nichols, M. Am. Soc. C. E., who was associated with my predecessor, Maxwell, in that work, presented a very interesting paper to this iety, to be found at page 365 in Vol. IX of the *Transactions*, illustrat- the character of that part of the cañon in his charge. In order to ate the termini of a tunnel it was necessary to triangulate from a slope the brow of the precipice across the river, his men being let down h ropes near the tunnel approaches. I confess that I suppressed a mor when three years afterward I looked up from the tunnel at the tions, still noted by staves where Mr. Nichols had operated. That

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paper will be an interesting study in connection with this of Mr. Stanton, and gives details of the geology of the region which I omit.

The slope of the Rio Santa by the railroad line permits a gradient of from 1½ to 3½ per cent. between the 50th and 125th K. But from this point up for a few kilos it was found impossible to escape a grade of 5 per 100. From the 50th to the coast the country followed by the river is flat and the current moderate. The upper stream, like all mountain torrents, is a succession of rapids and basins of smooth water. But there are no rifts, *i. e.*, there are no rapids in which rocks of the river bed show themselves. A man falling into the Santa would never be seen again, unless his body were thrown into an eddy on the edge of one of the smooth basins. I have never known a successful attempt at sounding any part of that fearful stream. But Mr. Nichols' paper shows that it was done. Mr. Leffert L. Buck, M. Am. Soc. C. E., put several of the bridges in position, stretching wire cables between abutments for a deck on which to make the erections, falseworks in the river being impracticable.

Mr. Stanton thinks it would be necessary to build the Colorado Cañon road from both ends of the cañon. Unless there are difficulties not yet described I should think such a process unnecessarily tedious and expensive. As a wagon road will be impracticable, a mule trail may certainly be laid up the cañon. In the Rio Santa we had fifteen construction camps between K. 50 and K. 96, all our supplies and building material going in on cargo mules. It is not necessary to maintain a continuous way upon one side of the river. We never crossed the high cliffs that came down vertically into the water if there were a better way on the opposite side of the stream, which we crossed and recrossed on little suspension bridges. I am not certain, but I believe they were designed by Mr. Buck. They were exceedingly simple and proved efficient to the last, their construction being as follows: Five wire ropes of 1 inch diameter were stretched across the river parallel and equidistant (to receive a floor about 4 or 5 feet wide), and anchored securely behind the bridge seat. Across these, at distances of 2 feet, were laid floor timbers 5 inches deep by 4 inches wide, and extending on either end 6 inches beyond the outer cables. The floor boards laid longitudinally were nailed to these timbers. At each end of the bridge and on either side a small iron post of an inch and a half round iron was set securely in the rock, and another cable 4 feet above the floor system was passed through eyes in the top of the posts and anchored about 8 feet behind them. From these last cables round iron suspenders three-fourths of an inch diameter hung by open hooks; the lower end passed through the ends of the floor timbers and were secured by nuts. If I remember correctly, the longest span of this kind was a trifle over 200 feet. These bridges suffered in one singular way. The *serranos*, Indians from the mountains, who drive cattle over them from the hills to the coast for ship-

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ment, would, on their return, steal the iron suspenders for home use, and they had the wit to take out alternate rods. We then riveted the lower ends at the nuts and closed the hooks at the upper end.

I ought, before going further, to say that I am separated by the width of the continent from my books and notes of that line and may err in stating details of a work from which I have been absent for thirteen years. I will rely upon Mr. Nichols and Mr. Buck to kindly set me right in such cases.

Once the way is opened along such a cañon by mule trail, sometimes in the cañon along its flats, sometimes on iron bracketed balconies along the faces of cliffs and again over the hills back of the cañon walls, it is not so difficult to carry on the construction. This was done on the Santa by letting men down the precipitous faces with ropes. Seated in "bos'uns' chairs," they drilled and planted rods for step-ladders to go and come. Ropes stretched along the wall for the feet and another 5 feet higher for the hands made rather luxurious pathways along the sheer faces of precipices inaccessible in any other way.

When I read in Mr. Stanton's paper of the flats so favorably placed to receive the line of a railway, I am reminded of similar flats in the valley of the Santa, which after having furnished convenient lodging places for our work, have been wiped out of existence. We had every reason to believe in their permanence—they were of exceedingly hard material, such as in this country we call "hard pan"—an indurated matrix of clay enclosing pebbles of various sizes, from that of a filbert to a large apple. But another evidence of stability seemed to make positive our faith in those little pampas. The old irrigation ditches of the Inca civilization found in the valley by the conquering vagabonds of Pizarro were still intact and demonstrating by their presence a condition of safety in and near their sites which had existed three and a half centuries at the very least; and who could say for how long an age before this discovery?

But in that year of catastrophe, which saw our line destroyed—in 1878—even those old canals which had furnished the controlling datum for establishing our height above the River Santa, were swept from the face of the cañon, and the place which had known them for untold centuries will know them no more. Nor was this destruction produced by inundation. The pampas destroyed were from forty to more than a hundred feet above high water in the Santa, and while they were swept away, the Huayrachiri Camp, at K. 115, not more than 5 feet above the flood, is as secure now as when first erected. The destruction was produced by a flood wave containing a vast amount of earth and rock, moving with great velocity and cutting under the bluff faces of the conglommerate we had thought impregnable.

There was a series of these waves, the formation of only one of which, so far as I know, was witnessed by any human being. Above K. 132 is a

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lateral ravine, dry in all seasons save when a shower in the mountains may chance to vent itself by this channel, on which occasions it contains water for an hour or so after the rain is over. It would seem that a slow and continued rain had soaked into the soil overlying some steeply inclined, rocky mountain face, and thus when the mass was saturated and water had settled to the rock, the huge volume of earth had gone down, a vast avalanche, to the bottom of the ravine, where it constituted a dam across the chasm to prevent the escape of waters pouring into it from mountain slopes further up stream. When once the waters thus impounded had risen to the top of the dam and poured over its face, the mass was again swept down and out to the main valley of the Santa. On its way, it gathered new material, and with augmented force rushed across the river, damming it to the height of 40 feet or more. The Santa is a powerful stream and took but little time to rise to its terrible work. This dam must have been 140 feet long across the river at its base and perhaps 400 feet or more on top.

The watchman at La Soledad, Mr. Nichols' old camp, the location of which he has described in the paper already mentioned, heard the fearful warning of the waves' approach, for several minutes before it came in sight, but felt himself entirely safe, as that camp was certainly a hundred feet above the stream. But when he saw this hideous engine of destruction come tearing around the bend a thousand feet off, piled up to immense height in the narrow gorge, its upper volume white as snow, tumbling, foaming, leaping, seething above a tremendous black wall of rushing rocks and water and earth, all struggling together in furious roar and din, tearing at the mountain sides and cutting out great chasms from the world's eternal superstructure, he fled in terror to the hills.

There was a succession of these phenomena, all with attending disaster. At K. 96, in apprehension of danger, I moved a locomotive engine, already cut off from headquarters, to a projecting point of rock crossed by our track and housed it in. When I left the line this engine was inaccessible from any direction save by a rope ladder down the face of the cliff. Where we had blown off the nose of a rocky spur more than 300 feet high to the apex, to form an embankment, in the belief that such enormous fragments could not be moved by any flood, the entire affair has been swept away as completely as if it had been a sand bank.

In passing through the narrow gorges of the cañon, these waves were piled to an immense height, and gathering increased momentum carried forward all the loose rocky material that lay in their way, so that below the Taquilpon Hacienda at, I think, about K. 75, where the river suddenly widens, the bed of the stream, which had been more than 20 feet lower than our road-bed, was when I last saw it, but 5 feet below the rail.

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In some lateral ravines, notably the Valley of Taquilpan River, an avalanche, which carried away the bridge, cut out the bed of the stream to a depth of more than 30 feet. Within two weeks this was not only refilled, but the bed of the river was raised 3 or 4 feet above the bottom of the railroad cuts on either bank, *i. e.*, above the level of the bridge floor, being about 10 feet above the bed as it existed when the bridge was built over that little stream. In several places, the mountain has slid down across the railroad to such an extent that where a cut of 40 feet was originally demanded to reach the roadbed, it would now require 140 feet of excavation to uncover the rails.

It need hardly be added that only a traffic of great value and importance can justify the construction of a road in the Colorado Cañon in the manner recommended by Mr. Stanton's paper, *i. e.*, on the flats and low down under the cliffs (page 307, last line of "Third"). I quite agree with the opinion expressed by Mr. Nichols when this paper was first discussed that it seems the better and safer course to resort to a great deal of tunneling. I am not certain if I might not recommend, and indeed in a similar case in the cañon of the Rio Reventezon of Costa Rica I have recommended placing the road upon balconies of wood or iron, bracketed or suspended along the face of cliffs well above all possible danger.

In reference to the use of electricity for working drills I should think Mr. Stanton has no occasion to hesitate. That matter has ceased to be the subject of experiment. The falls of the Willamette, light the City of Portland, Oregon, and furnish power for electric motors. I would go further even; for in that valley, the source of power is such, that electricity generated by water power may and will be used for carrying on the traffic, instead of using steam-producing fuel, which is not easily obtained. The engineer, who is confronted by the problem engaging Mr. Stanton's attention, would gather invaluable knowledge by a visit to the Peruvian road in the valley of the Santa; when he has learned what rain in the hills and a torrent stream can do, he will feel increased confidence to advise and devise.

I am reminded here of an interesting professional experience shortly after my return to the United States and less than two years after the destruction of the Chimbote Railroad. You will have no difficulty in understanding that I approached mountain torrents in a spirit of wholesome caution. I was employed as the umpire or consulting engineer of the Oregonian Railway in the Willamette Valley of Oregon. The contractor who built the road furnished the engineering. The chief of that corps shall be nameless; he is now where contractors cease from troubling and the engineer may rest. I was dissatisfied with the manner of placing the piers in the North and South Santiam rivers, both mountain torrents of great power and volume, and once ventured to say, "I approach streams like these with a great deal of apprehen-

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sion." "Oh, no!" replied the chief, "I never fear anything in engineering. I enjoy bridging a river."

When the road was presented for acceptance, I reported against receiving those bridges until they had been proven by the floods of at least one winter. Against this advice, the representative of the Scotch company settled with the contractor, paid for the bridges and the well satisfied gentleman left Oregon for Dundee to secure a contract for completing the line. Before he reached New York both bridges were swept away. Even this was not lesson enough for that unfortunate enterprise. New plans were submitted by a California Bridge Company; and while I approved the bridge plans, I could not approve the piers or location, which should have been changed. But as contractors are "practical men," while engineers, having never used saw nor pickaxe, are only "theorists," I was again overruled. Of course this ended my connection with the company. But every engineer can imagine my intense mortification when, the third winter following their erection, both Santiam bridges were again swept away.

In concluding these remarks upon Mr. Stanton's paper, I wish to refer to the current nature of accidents to which the builders are liable and to some of which also the traffic is exposed after such a line is opened. They are essentially of a single class, the falling of small stones or larger boulders from the slopes in talus, and the falling, let us call it the sloughing off, of masses of material from the faces of cliff where there has been an injudicious use of the explosives.

In the Rio Santa Cañon the early morning sun, by expanding material on the slopes and steep mountain sides, caused small cobble stones to leave their beds and roll down to the river, detaching other stones in their path, so that the early morning traveler was exposed to a dangerous volley unless he were careful to select his time before sunrise, or, an hour or two after the heat had become well established. If excessive charges are used in blasting, a great deal of material is loosened up that fails to be removed. This neglect is extremely dangerous and leads to no end of disaster for a long time after traffic has begun. There is another cause of the falling of stones from gravelly and hard pan slopes against which I confess I know of no protection. Thus, a passenger train was descending the valley, and discovering a boulder on the track, blew the whistle for down brakes; instantly, a volley of small stones started from up the slope, and bounding toward the train, broke every window in a second-class nine-panel car, terrifying the passengers, who took refuge on the road-bed under the car. It was no uncommon occurrence for cobble stones, to break loose and roll down the slopes in response to the engine driver's whistle.

An accident occurred to the engineer's camp at Huaurachiri, quite distinct from all others, and, indeed, from any I have ever known. The camp was on a low pampa, the engineer's house being probably 200 feet

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from the river-side; in the stream and along the bank were several boulders, ranging in size from 3 to 10 feet in diameter. On the bank and near these boulders were the servants' houses and the storing department. There could be no doubt that these boulders had rolled from the upper slopes of the cliffs on the opposite side of the river, and when they reached the vertical face had acquired such momentum as to be thrown clear across the stream. But one night a boulder 5 or 6 feet in diameter was precipitated from the lofty slope and thrown at such an angle upon the inside edge of a similar boulder lying in the river, that a ricochet took place; it leaped clear over the servants' camp, and plunged directly into the middle room of the engineer's house, carrying with it a part of the roof, the eaves and wall of the little structure.

These observations have taken up a good deal of time and space. I think it will be found, however, on careful analysis, that I have indulged in no relation that is not of practical value to engineers, contractors and all persons interested in the construction of cañon lines.

In conclusion I beg you to indulge me in telling the story of a remarkable rescue of a member of a party engaged in the preliminary surveys of the Chimbote Valley. One of the men, an American noted for his agility, had been sent down from a trail high up on the mountain slope, to learn if there were a point where with an instrument he could observe a certain peak already located in the surveys. It was necessary to descend a slope of say 45 degrees, on earth overlying the mountain rock. A little distance below was the edge of the precipice, a sheer height of 600 feet above the river. When the assistant had reached half way down between the trail and the cliff, the earth on which he walked began slowly to slide upon its smooth rocky bed, falling in a cascade over the bluff, and after its start moving with accelerated speed. There was no apparent escape for the unfortunate man; he threw himself prone upon the ground, and drawing his sheath knife, thrust it into the earth and shortly found a crevice in the rock by which to hold himself where he lay. Already a Cholo axeman had forced a crow-bar into the ground at the trail, and clutching it in both hands, had thrown himself, feet down, the slope. In an instant another descended by his side, and, holding by the ankles of the first, was followed by enough men of the party to make a living rope that reached their comrade in danger, who, by this support, completed the observation and ascended the slope to a place of safety, followed by the successive members of the line. After this a light rope became a constant part of the outfit of such parties.

O. F. Nichols, M. Am. Soc. C. E.—I have been much interested in Major Sears' account of his recent experiences in the valley of the Santa in Peru, and I am glad to hear again of the work with which I was, some years ago, intimately connected. I read Mr. Stanton's paper carefully, and found it exceedingly interesting. It seems to me that this

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projected railway through the cañon of the Colorado would open up the great Southwest, of which we know so little, in a manner not otherwise practicable. The line would run diagonally Southwest from near Denver, and reaching a point near Yuma, and thence moving westerly, cross the mountains at a relatively low elevation, and reach the ocean near San Diego, California. The line of this route from the Northwest would be something more than five hundred miles shorter than any of the routes now in use. The river has a very gradual fall and the grades would be remarkably easy and much lower than those on any of the other transconti nental lines.

Mr. Stanton has necessarily introduced a great number of tunnels, so that a large portion of the line would be built in tunnel. Any criticisms I may make will be made, as they should be, in a very modest way by a person who has never been through the cañon. Mr. Stanton deserves great credit for the skill and persistency with which he overcame all obstacles and accomplished this very difficult survey, which bears the marks of energy and accuracy.

Much of the work, Mr. Stanton says, could be done with scraper, referring to those portions located on the flats or sand bars left by the river at the prominent bends. Major Sears has explained how similar work was subsequently cut out by the recent floods of the Santa, on the Chimbote line. On the Santa these flats, or pampas, are raised some 20 to 30 feet above the river, and are composed of gravel with about 10 per cent. of sand. This material had been compacted and consolidated under the water of the deep lakes of which they formed the beds. The rock dams forming the lakes were afterward cut out until the level of the River Santa was so lowered that it finally cut into the lake beds themselves, leaving this material standing high above the river, and here it had remained in a tropical climate which does not vary to exceed 15 to 20 degrees in the year. The top surface of the material became hardened and sun dried, so that picks and even blasting were required for its removal. In the middle Santa this material stands in retired cliffs, 300 to 500 feet high. I remember visiting Mr. Buck at his camp when he was erecting bridges 2 and 3 on the Chimbote line. He had located his house with flat roof of corrugated iron at the base of one of these cliffs. It was some feet from the cliff, and the face of the material was such that the gravel dropping out, under the action of the sun, would fall so as generally to clear his house. Occasionally it would fall short and strike the roof and sometimes penetrate. Fortunately for his friends as well as himself no fatality resulted, though there were narrow escapes.

These flats in the Colorado cannot be so hard and lasting as those in the Santa, and I should greatly prefer to avoid the use of them altogether, even at the expense of increasing the tunneling and rock work. On the Chimbote line the mistake was made in the lower por-

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of the line of following around too near the edges of these flats in
to lighten the work, the result being to increase distance and
ature, and expose the work too greatly to the danger of subsequent
s, then not anticipated. In the work on the upper Chimbote we
avored to keep back on these flats as near the rock as possible, for
omy of line and greater permanency.

t some points Mr. Stanton has shown alternative lines, one coming
of tunnel onto these flats, the other keeping inside entirely in
el. I believe it will be found wisest to build the inside line,
at the expense of lengthening the tunneling. The distance and
ature, both essentials in a trunk line, will thereby be lessened.
photographs of the Colorado Cañon show clearly that much of
ine can be built in rock cutting on the comparatively easy slopes,
the talus slopes can be utilized where they are composed at the
of heavy stones and are not exposed to running slides from above.
e running slides were a prominent feature of the Upper Oroza line
eru, and necessity compelled their use. Fortunately no fatal acci-
s have resulted, although a locomotive had a rock of about 1 000
ds weight crush through it, wrecking the cab and starting lever
out killing the engineer and fireman, who were men of nerve and
ty.

n the narrow gorges of the Colorado, where the walls are practically
ical and immeasurable, tunneling or gallery work is imperative.
eries are quite practicable in good solid rock. At the entrance of the
nbote Cañon, of Huyna Taquilpan, just below Tunnel Number 1,
llery was designed. The location on the vertical cliff at this point
made by arranging a system of ropes, the upper line fastened to
s placed in the rock by hanging men over the cliff, the lower line
ut five feet below it suspended by ropes strung diagonally from the
er line. The men walked on the lower rope holding fast to the upper.
two cañons are quite similar in one respect. No man ever fell into
Santa and lived. It is a rushing, roaring mountain torrent, deep
ugh to drown and rocky enough to kill; often with a 12-mile
ent; its surface, always turbulent, in daylight a picture of demo-
waters, at night waking the stillness by grinding huge boulders
ther in its bed. The Colorado is of this character at the rapids,
ough there are many places, as Mr. Stanton states, where boats can
used with ease and safety.

Mr. Stanton shows how wide the Colorado Valley often becomes at
top, reaching 15 to 20 miles in places. This would mean compara-
ly easy slopes suitable for rough construction roads, and these points
often quite near stations on the existing railways. This would
n to obviate the necessity of constructing this long railway from the
o ends, and indicate that it might well be prosecuted from many inter-
diate points, perhaps as many as thirty or forty, thus shortening the

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time necessary for construction. All the most approved methods of modern rock construction would be utilized, and all the tunnels would be driven either with compressed air or electrical machinery. At best, an immense time would be required in construction.

The Government reports show that the vast basin of the Colorado contains much land which, when the district is ultimately irrigated, will be suitable for the habitation of an immense population to be engaged in the cultivation of agricultural products. No other avenue exists for the accomplishment of the settlement and utilization of these now barren wastes, than that now presented in the skillful work of Mr. Stanton. He and his associates deserve great credit for accomplishing so much in the wildest cañon of the globe; and I sincerely trust that the author of this interesting paper may live to complete this great work which he has so well inaugurated.

SAMUEL M. ROWE, M. Am. Soc. C. E.—This report from Mr. Stanton, treating as it does of a portion of our country little known, but of which I have gained some knowledge from several years of professional acquaintance, possesses more than usual interest. In the three points, low grades, favorable climatic conditions and the facilities for water supply—such favorable conditions not obtaining in the case of either of the existing overland routes—Mr. Stanton gives strong points that will go far to counterbalance the seemingly unusual cost of construction. There is much of scientific value in this report to be added to our too limited knowledge of the region in question. His statement that "the glacial period was not icy but rainy," corresponds with my observations, and accounts for peculiar features of that region otherwise hard to reconcile with glacial action. The appearance of the Stanton party at the Colorado Bridge possessed to me all the interest of victorious veterans returning from a well fought battle. They are entitled to a greater measure of credit than can be conceived by any but those acquainted with the region traversed.

JOHN E. EARLY, M. Am. Soc. C. E.—I have read Mr. Robert B. Stanton's paper with much interest. A low grade line to the Pacific Ocean would undoubtedly be of great benefit to the country at large, and Mr. Stanton seems to have found a line which will also be a paying investment of itself. There can be but little doubt but that if this line is so "financed" and built that its capital account will not represent more, or much more, than its actual cost, it will be a paying investment to its builders. But we all know how much "water" can get into the capital stock of a railway during its construction and how this water keeps the line down during the first years of its operation.

I can give Mr. Stanton another point for traffic for this line, viz., a coal traffic for Mexico. Mexico is a country where fuel is both scarce and dear. Its only productive coal fields are near the Rio Grande, on the Texas frontier, and from present appearances no other profitable fields

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ver be discovered. At some time there will be railway connec-
with the Pacific, and it is probable that Mr. Stanton's line would
e to deliver coal at Pacific ports at such a price that it would be
med on the railways and at the towns and mines of the Pacific
of Mexico, with a chance of its use in the interior. I write the above,
ng that it is claimed there is good coal in the State of Sonora.
coal, however, is so situated that it will not be available with-
railway, and this railway would be so dependent on the coal for its
that it should be considered as part of the plant for working the
furthermore, its amount is, in the opinion of experts, not sufficient
rantee such an investment for plant. Whenever this coal comes
ket it will come by a north and south line through eastern Sonora,
t of such a line's business; but this will hardly be for many years,
robably not for a long time after some line has been built down to
ast from the interior.

ere have been many concessions given for railways to the Pacific
Mexican Government, and with subventions; but the subventions
been so small in comparison with the estimated cost of construction
e prospects for business, that no company has yet seen fit to con-
its line. It would not, however, be surprising if before long this
is undertaken, as the present policy of the Government seems to
refuse to extend concessions with subventions and to declare them
ed when they lapse; this is leading both the railway companies
e capitalists to make the most of the concessions now in hand.
s, perhaps, wandering from the subject, but may be taken as addi-
information of interest regarding the prospects for business of Mr.
on's line.

r. Stanton's paper brings out strongly the availability of cañons in
al for railway construction. I think they are usually found more
ble than they appear at first examination. A railway line is so
and narrow a thing as compared with a cañon—even a narrow one
t it can be laid on the beaches and shelves and hung on the slopes
e cañon, without disturbing the main features of the topography.
in the closest cañons, it is usually found that the really difficult
ons which first catch the eye and which remain longest in the recol-
n, comprise only a small proportion of the length. The cañons
sh the lines of low grade through the mountains, and in many cases
esistance to traffic is given by the resistance of the curve of mini-
radius rather than by the fall, so that there is a maximum grade
er than the fall of the cañon, which can be used without detriment,
h allows the position of the line to be varied on the sides of the
n within moderate limits, in seeking for the best ground. Many
variations result in a cheapening of the line below the estimates
the reconnoissance.

note the difficulties of the reconnoissance and the use of photog-

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raphy. Engineers seem able to go, as a matter of business, wherever there is need for them. A friend used to say that the entrance to the interior of Africa would be found and no fuss made or books written about it, when some engineer started out to find a line for a railway into it. The use of a reconnoissance and report on a line of railway being to give the projectors and possible investors as clear an idea as possible of it, I consider the photographs obtained as of the greatest value.

I note that the work can be carried on from many points at once and this will be of great advantage. Without doubt other ways of entering the cañon will be found if they are required. Mule or jack trails are not hard to find, and the cost of transportation of tools and supplies for construction by them does not add much to the cost of a line. Probably advantage would be taken of times of high water to send supplies down the river by flat boats, as coal is now sent down the Cumberland River, the boats not returning but being sold for other purposes.

From Mr. Stanton's paper, it seems to me that the line should be built, that it will pay for building, and that Mr. Stanton, if he should build it, would probably find, when the accounts are all in, that he has not underestimated the difficulties and cost.

J. Francis Le Baron, M. Am. Soc. C. E.—After reading Mr. Stanton's very clearly written paper, it would almost appear as if these cañons of the Colorado were made expressly for the purpose of a railroad line, as if nature, in the fulfillment of her beneficent plan, had cut these tremendous gorges through the everlasting hills to afford an easy passage for the commerce of a great country; but it required a bold and fearless mind to first grasp the value and possibilities of such a route, which at first sight might appear as a visionary or foolhardy scheme. The more the subject is studied, however, the greater its advantages appear. There is nothing in these days that the American engineer or contractor will fear to attempt, when backed by sound science and capital. The use of river valleys and gorges is and has been universal for railroad locations, but this project is unique in its fearlessness, grandeur and extent.

The location, by the writer, of the Petersborough and Shirley Railroad, through the Unconunuc and Monodnoc Mountains of New Hampshire, while not comparable with the line under discussion, afforded some details of a similar nature where the transit-man had to be lowered down to his station by ropes and the instrument taken off of the tripod and set up on the bare rock. Such instances are by no means rare on railroad surveys, even in the eastern portion of the United States. Such locations are often found more formidable in the survey than the construction. General Fremont's explorations for a railroad route over the Rocky Mountains were fully as dangerous, and the building of a transcontinental railroad appeared, at that time, far more impracticable than this line does now.

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he construction of a railroad on such a location as is proposed gh the Colorado Cañon, is infinitely easier than the construction ch a line as the Nicaragua Ship Canal Railroad, for instance; the on of which was made under the writer's direction, in a country the annual rainfall is 320 inches per year, and which was limited ery step by the line of the canal itself and its flooded basins and nkments, rendering it necessary to lay out the line, for the most transverse to the natural drainage lines of the country through the es of the Cordilleras. This is the railroad which is required to aid e construction of the ship canal.

he availability of the proposed Colorado line for a grand trunk , seems to be clearly shown by Mr. Stanton's interesting and ic description; but this availability must, of course, be governed e character of that part of the line from the terminus of the survey, Yuma, across the Sierra Nevadas to the Pacific Coast at San Diego, f this I have no information except in a general way. Many of the ls of construction through these cañons will undoubtedly be ered more feasible and less expensive if a careful location should ade. For instance, the long aggregate of river embankments, which uld consider highly dangerous and expensive in a river of this cter, can perhaps be avoided by crossing the stream, or by raising rade for some distance. The so-called half tunnels can probably asted out in great masses by drilling horizontally from platforms ended by ropes in front of the scarp wall, and it may be that some e timber and ties can be floated down the river to available points, cing the cost of transportation.

he absence of snow in winter will largely reduce the cost of opera- and maintenance of this line, which is a great point in its favor as pared with other lines; as is also the abundance of water for the nes, the freedom from caving or washing earth banks, and the great dance of stone ballast.

t would seem that the local freight traffic of this road will be largely ral, collected along the line, for distribution at the termini. The of its low level, making all its branches have a down grade to it, be an advantage, and it seems certain that it never will be vexed grade crossings and damages arising therefrom.

he question of "Will it pay?" may properly be included in a dis- ion of general availability, and the first thing that occurs to me is unique fact that, from the necessity of its location, it cannot pass ugh any sites of future large cities, although it may possibly some find itself near the suburbs of some metropolis yet to be built. might be looked upon as an advantage in many ways rather than rwise; for instance, by lessening the labors of the legal department he matter of damages and claims for injuries to persons, teams and le on the track, the costs of fences and gates, and expenses for right ay and depot grounds.

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The relative capacity and operating expenses of such a road, with no grades higher than one-half of 1 per cent., as compared with a high level road, with grades of 2 to 2½ per cent., is the most important point to be considered in the question, Will it pay? and would seem of itself to decide the question in the affirmative, especially as the curvature is probably no sharper than in many cases would be necessary on a high level road. According to Wellington's most excellent treatise on Railway Locations, the ratio of gross weight of train to tractive power required is, for a grade of one-half of 1 per cent., 111.11, but with a grade of 2½ per cent. only 34.48. The net load of train in tons that can be hauled up a one-half per cent. grade varies from 504 to 1 357 tons, according as a very light or very heavy engine is used, while on a 2½ per cent. grade only 120 to 361 tons can be hauled. This means that a larger number of trains must be used to transport a given amount of freight, and where one train will suffice on a level track, it will require 2.33 on a grade of one-half per cent. and 8.79 trains on a grade of 2½ per cent. The cost per year of 1 000 ton miles daily over a one-half per cent. grade is stated to be $18.75, while over a 2½ per cent. grade it will be $26.72.

J. Foster Crowell, M. Am. Soc. C. E.—For those of us who were present on the occasion which Mr. Stanton refers to, at Cresson, viewed his remarkable photographic ensemble of the cañon and listened with eager interest to his narration of the difficulties and perils of the survey through those stupendous gorges—this paper, giving its results, possesses an added interest. To those who were not so fortunate it may be mentioned, that Mr. Stanton's reference to the photographic features fails to give an adequate idea of the very comprehensive and satisfactory showing that these photographs make of the characteristics of the various cañons, of their formation and of the topographical details of the cañon walls, throughout the many miles which the series embraced, as Mr. Stanton says, almost continuously. The lantern exhibition of these views was not only demonstrative as to the object, but also of the great value of the camera as a surveying instrument. To it is largely due the fact that this examination, which otherwise would have been but a reconnoissance, possesses the value of a careful preliminary survey.

The character of the paper is such that intelligent discussion in detail is not within the resources of those who are not fully acquainted with the region of the cañons; as I am of that number, I shall confine my brief comment to a word upon the considerations which to me seem to recommend Stanton's route, assuming its entire availability to be demonstrated. First of all and more than all, is its low grade feature. Railroad projectors have become so inured to high gradients that 4, 5 and even 6 per cent. are not deemed obstacles to a location. The projectors ascertain the latest performance of locomotives as grade climbers and govern themselves accordingly. Afterward, when the roads

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ompleted and the operating officers begin their never-ending and
us struggle against the laws of gravity, it is generally too late to
ct the evil and the road is handicapped for all time. The enormous
ntage of a line with a maximum grade in either direction of not
than one-half of 1 per cent. as against the present high grade lines
west of Denver, is too apparent to be dwelt upon. Mr. Stanton
that even a lighter ruling is practicable. But, of course, the
ate success of a railroad depends upon the amount of traffic, and
probably true that in this year the traffic in suitable volume to
cate the commercial success of this proposed road is not within
It by no means follows that it will not appear speedily. We in
East are apt to regard all such projects from the transcontinental
of view; the applicability of a railroad through the Colorado
as has this point of view, but only in a minor sense; its true
ion would be to aid in developing the Southern Pacific coast.
that development will call for upon the completion of the
ragua Canal, or even without it, can only be conjectured, but we
safely presume that the requirements will be vast. A second im-
nt recommendation of the cañon route is its comparative freedom
snow; a very valuable feature from the standpoint of both the
ting and traffic departments, besides saving the enormous outlay in
cost and maintenance of snow sheds and chutes, where necessary on
nountain lines.

Mr. Stanton has put in evidence the indications which refer to the
bility of local development, which would seem to confirm the
re success of the proposed line as far west as Yuma, independently of
commerce to be reached hereafter at the port of San Diego. What-
may be the outcome, we as engineers should congratulate our
w-member and his associates upon the thorough and admirable man-
n which this exploration and survey have been carried out in the
of almost insuperable difficulties and amid disaster and death; that
achievement in itself of which they may well be proud.

s a voyager of some little experience in exploring and navigating
s beset with rapids, I would add a word as to the best form of boats
e used; this at first sight appears to be an unimportant detail, but
e sad sequel proved, in this expedition it was the most important.
Stanton describes the improved cedar boats built for the second
npt in the light of the first disastrous experience and they appear to
rendered good service. But a canoe-shaped boat built of steel, on
leboat lines, with air compartments fore and aft and propelled with
dles instead of oars, can be made much lighter, is non-sinkable, and
stand the roughest usage and most violent concussions without detri-
t. Such boats are used by the Nicaragua Canal people and were
outcome of many years of experience with wooden ship's boats and
ve canoes. Their cost is not great; they can be transported any-

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where without injury, have no seams to open, and can be quickly repaired. As an auxiliary transport for provisions, records, ammunition and other perishable articles, I would recommend air-tight metallic balsas, in pairs, fitted either for being paddled or lowered, or dragged up through rapids in cases where open boats could not be kept dry. As speed is no object in the balsas, they can be made very roomy and strong at both ends; and they should be provided with two or more packing holes, secured with air-tight covers for getting at the cargo.

Mr. Stanton has referred to the possibility of making use of the water power in the cañons for generating electricity to be applied to rock drilling. This seems entirely feasible and the power would appear to be inexhaustible, so that not rock drills alone, but devices for removing débris, laying up rock slopes and setting masonry, and contractor's locomotives, could also be operated. Indeed it is not altogether visionary to point out that by the time this railroad shall be constructed, it may become mechanically and commercially practicable to operate the entire railroad from end to end with the power of the cañon in electrical application.

Mr. Stanton shows a commendable conservatism in locating his line generally well back from the river in exposed localities. I think he will be entirely justified in adopting the alternative lines he has indicated and even in pursuing this policy to a still greater extent; thus, while requiring greater length of tunnel and increasing the first cost, he may avoid dependence on the talus where any doubt exists as to its permanent stability. It is quite true that important lines and structures are not infrequently founded on talus, but it is wiser not to allow it to constitute a sole dependence where other and safer possibilities exist. This remark applies with still greater force to the flats, whose permanence must always be problematical. This is not offered as adverse criticism, but only by way of emphasizing a consideration which has already been placed before us in the paper. The safer line is, in the case of the more difficult portions, the shorter, and the condensed classification of the Glen Cañon Division shows that in the alternative location "B," there is a saving of 18½ miles of line gained at the expense of 6¾ miles of rock tunnel. Figures of cost are not given, but if we assume average prices and capitalize at 5 per cent. annually the cost of maintenance, renewals and operating the 18½ miles of distance saved, computed on a basis of moderate traffic,—we will find that the excess in first cost will be very nearly balanced, not taking into account the value of insurance against risk and the saving in time to passengers, both tending to augment travel.

But even in cases where the financial showing would not be so favorable, I believe this course should be followed irrespective of first cost, and the line located, as the author of the paper has well stated elsewhere, "on as permanent and safe ground as the nature of the cañon walls will

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permit, believing as I do that this 'railroad of the future' should be built for ultimate value and profit even at a much greater cost."

LATHAM ANDERSON, M. Am. Soc. C. E.—Mr. Stanton's paper is a valuable contribution to our knowledge of the geography of this continent, and also to his daring enterprise in prosecuting the survey. It is interesting to know that a railroad can be constructed through this mighty chasm. It is always pleasing, especially to members of our profession, to see any new instance of the subjugation of matter to mind in overcoming apparently insurmountable obstacles. Apart from these considerations, the benefit of this survey is not apparent. In the opinion of the writer there is no equal area of the earth's surface in any civilized country in which a railroad would be of so little practicable benefit. If the sole object be to transport the coal of Utah to the Pacific Coast, a much shorter and better line could certainly be found; *e. g.*, beginning, say, at Frisco (the present terminus of a line of railroad), running thence southwestwardly in the vicinity of Pioche, through Vegas Valley, and south of the Amargosa Desert, up the Valley of the Mojave to Barstow, on the Atlantic and Pacific Railroad. Most of the country traversed by the line just described is about as inhospitable a desert as can be found on the face of the earth. But it has one crowning advantage over the line in the chasm; that is, the people living on either side of the road could get to the cars. The desert just described is bad enough, but it has some oases scattered through it. There are rich mining districts of the precious metals, and there is so much open country to traverse, that there is every reason to presume the line would be less costly than the line in the Cañon. As you approach this abyss, the plains on either side are rent by chasms similar in character to the Grand Cañon itself, and these have side cañons equally precipitous. This great table land, interspersed with mountain summits, becomes more and more sterile and unproductive as you approach the main river. Water is at least as scarce here as on any part of the desert. Even where gold or silver mines exist, fuel and water are so scarce and access to the country so difficult, that the mining of even the highest grade of ores must prove unremunerative. Imagine a man working a mine near the brink of such a precipice. He may be within sight or even within hearing of the cars a mile below him, but it would require a half day's journey down precipitous roads to get to the nearest station, and, of course, still more of an undertaking to climb back again.

The fact that the expedition found some evidence of gold in the river or mesa gravel at several points on the route, cuts no figure at all in overcoming such insuperable obstacles to the profitable operation of a railroad in the bottom of such an inaccessible hole as this.

J. L. BARLOW, M. Am. Soc. C. E.—Mr. Stanton divides his paper into two questions, first as to the feasibility of the Colorado for a railway line, and second as to its advisability. The feasibility of the

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Colorado for railroad purposes becomes a matter of little interest, unless its construction can be shown to be a business proposition and one which gives promise of answering the question of its advisability in the affirmative.

Mr. Stanton's tables of classification show a very heavy line; to construct probably $40 000 per mile for its entire distance would be a moderate estimate, or perhaps $40 000 000 or $50 000 000 to build from Grand Junction to San Diego, its one advantage being that it is a very low grade line for the greater portion of its distance.

The Union Pacific now has its grade completed to a point 30 miles south of Pioche, Nevada, to tap the mines in that district, and complete surveys southwest into California (the ruling maximum grade being 1 per cent.) show, that at such time as it may be deemed advisable, the connecting link can be built at from 10 to 12 per cent. of the cost of the Colorado Cañon route, thus completing a substantially parallel line to the Colorado River, answering all the commercial purposes of the latter, and a shorter through line to the East, going via Salt Lake and Ogden instead of via Denver. The same comparative portions of each route, taken by adding the construction cost and capitalized value of operating any probable amount of traffic they may have, will show unfavorably to the Colorado Cañon route.

The writer is also familiar with a considerable portion of the route surveyed by a new company about two years ago, from the vicinity of Grand Junction to Southern California, averaging to run parallel to the Colorado River on the north and west side, at a distance varying from 25 to 150 miles, which, when compared to the Colorado Cañon route, will also show the same result as above, though perhaps in a lesser degree.

The winter of 1889-90, in regard to snowfall and railway blockades, was the most serious in many years in the western portion of the United States; yet the snow was never deep enough in southern Utah to seriously impede the writer's progress, and he traveled many hundred miles by buckboard along the routes of survey in making the reconnoissance, and often on no wagon roads.

The question of any given route's feasibility is very largely of a comparative nature, and while the writer has no personal knowledge of the Colorado River and its cañons, except below the mouth of the Rio Virgin, yet from Mr. Stanton's description of the route above that point and his own observation below, as compared with the completion of another route already partly built, or the construction of a new and totally different one as indicated above,—he is inclined from such a standpoint to doubt the feasibility of the cañon route.

As regards its advisability or the question of "Will it pay?" Mr. Stanton indicates its three main sources of revenue, viz.: 1. Its share in the transcontinental business. 2. Colorado and Utah coal traffic to

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South California. 3. Local business. In regard to the first, there are already two transcontinental roads competing for Southern California business direct, while a third (the Union Pacific) indirectly forms an active competitor via the Central Pacific and San Francisco—all three roads maintaining traffic agencies in Southern California, with through freight lines and passenger coaches. With three competing rail lines and the probability of another formidable competitor in the Nicaragua Canal, it will be seen that the Colorado Cañon route can command but a small proportion of the transcontinental business, and that, in turn, practically confined to the district of Southern California.

Concerning the second, or coal traffic, the British Columbia and Puget Sound coal, mined at or near tide water, is carried by steam colliers to San Francisco for $2 per ton, and Southern California for $3 per ton. Mr. Stanton cites even San Francisco as within the scope of the coal business of the new road. In order to successfuly compete can this proposed route carry Colorado coal to Southern California for one-quarter of a cent per ton mile? Or from Utah at one-third of a cent per ton mile? And to compete in San Francisco the rate would have to be about one-eighth and one-sixth cents per ton mile respectively.

In regard to the third source of revenue—the local business—it can only be a matter of conjecture, but the writer believes that, taken as a whole, there is no other stretch of a thousand miles of country in America which is more barren or gives less prospect of developing local business for a railroad than does this. And the advantage claimed with regard to local business by reason of being in the bottom of these comparatively narrow valleys and cañons, can, with equal force, be urged against it, by reason of inaccessibility from a comparatively wide belt of tributary country. Aside from these questions, the boldness and nerve required to successfully carry out such a hazardous expedition, and the value of this research as an exploration and engineering reconnoissance, will be appreciated by every engineer who reads Mr. Stanton's paper.

Lewis Kingman, M. Am. Soc. C. E.—I have read with much pleasure and interest Mr. Stanton's paper on his Colorado Cañon survey. The difficulties and dangers overcome have been great; the results are valuable and highly commendable; it has been demonstrated that a railway can be constructed through the Grand Cañon and that it can be done within reasonable limits of cost for cañon work. I have spent two decades on railway location and construction in the Southwest, and two years in charge of the location of the Atlantic and Pacific Railroad, through northern Arizona, from the Little Colorado to the Needles on the Colorado River; a distance of nearly 300 miles in the country south of the Grand Cañon.

I would congratulate Mr. Stanton on being alive and well. It is not often that an engineer is accompanied on an exploring expedition by the President of the company, he is usually sent with the best wishes of the

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higher officials. We have heard of proposed laws requiring the President to ride on the cow-catcher to insure the safety of his trains; but in this instance a railroad president volunteers to go to the front on a most hazardous expedition and loses his life. I think President Brown should have a monument as high as the walls of the Grand Cañon to commemorate, among men, an act so rare.

I do not question the practicability of building a road through the cañon; engineers can do almost anything if there is really an inducement for capital to support them, and a reasonable assurance that it will pay. A line from Colorado to the Pacific passes over a country of magnificent distances. From Grand River to the Rio Virgin it is 668 miles; the line for the greater part of the distance is inaccessible, and there is but little inducement to build branch lines. The distance from Grand Junction to the Needles is 793 miles; the distance from the Needles via the Atlantic and Pacific Railroad to San Diego is 374 miles, an air line between the two points being about 215 miles. It is possible a line might be located saving 100 miles of the Atlantic and Pacific distance; we would then have 1 067 miles from Grand Junction to tide water at San Diego. The coal traffic would be one of the principal items of business for the new line through the cañon. Taking the cost of coal F. O. B. at Grand Junction to be $1 per ton and freight charges at three-fourths of a cent per ton-mile, it would cost delivered at San Diego $9 per ton. A railroad might be located from the southwest Utah coal fields via Rio Virgin and the Colorado River, and shorten the distance from the coal fields to San Diego to about 600 miles; then with coal costing $1 and freight three-fourths of a cent per ton-mile, coal would cost $5.50 per ton at San Diego, and if the quality was first-class they might control the market. On January 4th, 1892, Wellington, Scotch, English and Santa Fé coal was quoted worth from $8.25 to $8.75 F. O. B. at the wharf at San Diego. At Los Angeles, on the same date, Gallup, New Mexico, coal was selling for $8.50, and Wellington coal for $10 per ton. At San Francisco, on the 2d of January, 1892, the prices were as follows on 2 240 pounds to the ton, F. O. B. at the wharf:

Wellington	$7 25	Australian	$7 00
New Wellington	8 00	Pennsylvania anthracite	10 50
Naniamo	6 75	English coke	8 00
Seattle	5 00	Colorado anthracite, 2 000 pounds to the ton	12 50
Coos Bay	5 00		
English	7 00		

From Mr. Stanton's tables showing the amount of coal used at San Diego in the first six months of 1890, only 19 309 tons were received; this would be 38 618 tons a year, or 2 575 carloads; about 8 carloads a day; the whole would not make a very large item for any one railroad. One reason for the great difference in the prices of coal at San Diego

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and San Francisco is the amount handled, another reason is that ships coming to San Diego cannot get any return cargoes. Mr. Stanton's tables indicate that there is about twenty-eight times the amount of coal received at San Francisco that there is at San Diego.

The distance from Gallup on the Atlantic and Pacific Railroad to San Diego is 791 miles. This is 276 miles nearer to San Diego than Grand Junction would be; yet the distance and cost of Gallup coal explains why only 2 000 tons of this coal were sent to San Diego in the years mentioned. At Los Angeles, where other coal has to be reloaded and shipped by rail, Gallup coal is sold for $8 50, a price which allows it to be used in competition with other coal. Colorado anthracite is sold now for $3 per ton of 2 000 pounds F. O. B. at Crested Butte. The distance to San Francisco is 1 394 miles, the freight to San Francisco is $9 50, which makes this coal cost $12 50 per ton of 2 000 pounds; while a ton of 2 240 pounds of Pennsylvania anthracite is sold F. O. B. at the wharf at San Francisco for $10 50. By the cañon line it would be 1 220 miles from Crested Butte to San Diego; then reckoning as before freight at three-quarters cent per ton, we would have the first cost of anthracite coal $3, freight $9 76, cost at San Diego $12 76, which would not allow it to compete with Pennsylvania anthracite. That coal and coke could be delivered and sold at San Diego from any place on the D. C. C. and Pacific Railroad, or its proposed branches, for from one-half to one-third the present market prices, does not seem apparent; they could not deliver coal at San Diego as cheap as the Puget Sound or Oregon coal is delivered at San Francisco; for that reason they could not expect to ship any from there along the coast, and, if not, they would have to rely upon the San Diego consumption, which is very small.

As to the passenger business, distance is always an important factor. Then again, the movement of passengers is somewhat in proportion to the population and the business. San Diego has a population of 16 153, Los Angeles 50 304, and San Francisco 298 000. We should expect three passengers to go to Los Angelos to one to San Diego; and nineteen to go to San Francisco to one to San Diego.

Comparing the coal business of the two places, we should expect twenty-eight passengers to go to San Francisco to one to San Diego. The distance from Denver to the Needles via Grand Junction and the cañon would be 1 218 miles. The distance from Denver to the Needles via the Santa Fé, La Junta and Albuquerque is only 1 102 miles. The distance from Denver to San Diego via Grand Junction and the cañon, with a new line from the Needles to San Diego, 100 miles shorter than the Atlantic and Pacific, would be 1 492 miles. The distance from Denver via the Santa Fé, La Junta and Albuquerque to San Diego is only 1 476 miles, and passengers going to or from points east of Denver would find the distance much more in favor of the Santa Fé. There would be some passengers who would go to see the cañon, but 700 miles of cañon would become rather tedious and monotonous.

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At the time the Atlantic and Pacific Railroad was built there were parties who talked of building a hotel at or near the mouth of Diamond Creek, 25 miles north of Peach Spring, constructing several steamboats and improving several riffles in the cañon, so as to take passengers far enough up to get a good view; but the project was never carried out, as no one was ever convinced that it would pay. I visited the salt cliffs along the Rio Virgin. It is a wonderful sight, cliffs and hills of salt standing high above the valley, usually capped with a few feet of mud rock; the salt had been mined and used by smelting works, but distance and bad roads were too much to contend against. Occasionally some of the salt is clear and transparent, but the most of it would have to be refined for domestic uses.

The agricultural resources of the country adjacent to the cañon are overestimated by most every one, I think. Its value for grazing purposes is limited by the absence of water. The valley of the Colorado from the Rio Virgin down is greatly damaged by salt. There is no good grass growing from the Rio Virgin down. The earlier engineering expeditions often had to subsist their mules on mesquite beans gathered by the Indians. Wheat does fairly well on selected soil; but cottonwood trees usually grow to be from 4 to 6 inches in diameter and then die as soon as their roots reach the salty subsoil. One great trouble with the whole country is that it is too well drained. Water is not often found and when found it is in small quantities. On the Atlantic and Pacific Railroad at Winslow, while I was in charge, we sank a well 1 800 feet deep. The surface water was cased off, and when the well was tested there were but six barrels of salt, bitter water pumped every twenty-four hours.

At Ash Fork a well was sunk 900 feet deep, water was hauled from Hardy, 126 miles, to drill with, and often drilling was performed with difficulty, as the water ran out through crevices and left the tools almost dry. Finally the tools became fast in a fissure and work was abandoned.

The mining interests in the country are considerable, but many mining companies have failed on account of the cost of fuel and supplies. They do not realize that distance regulates cost even on railroads, and many of the mines are away from railroads, where the wagon freight over sandy roads is greater than the cost of the long haul on the railroads. The mines in northern Arizona were expected to develop when the Atlantic and Pacific Railroad was completed, but the results are not flattering. In conclusion, it seems to me that the Colorado Cañon line, with its terminus at San Diego, is not in the direction of travel or traffic. Passengers would desert the line at the first opportunity to go to San Francisco, San Bernardino or Los Angeles. A short line from the Needles to San Diego would not reach the productive part of Southern California. The tendency would be for freight to follow passengers,

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but distance would operate against this and the line would be practically shut off from the California business.

ROBERT B. STANTON, M. Am. Soc. C. E.—It is gratifying that the interest manifested in my paper has brought out so extended a discussion. Some points seem to call for explanation, though I shall endeavor not to extend my remarks to any great length.

I am fully aware of the value of "skilled labor" in any undertaking, under ordinary circumstances. Yet in selecting my men for the second expedition, although strongly advised to secure the lumbermen Mr. Sears refers to, I rejected all such labor with but one exception, an experienced boatman in such waters, and he deserted the expedition in the Grand Cañon when we most needed him. It has been my experience many times during the past twenty years that for such work, whether on land or water, where prolonged hardships and privations, and especially scanty food, and perhaps starvation, are to be faced day after day, the man who has an object to gain, with of course the qualities of body and mind to quickly acquire "experience," is far more efficient, and the better man to tie to in a wilderness, than the laborer, however skilled he may be, who is simply hired for so much a day.

No one could have rendered more faithful service or have more skillfully saved our lives by his quick-witted work just in the nick of time, than my First Assistant, Mr. John Hislop, C. E., and while all the men who made the entire trip with me did their whole duty, yet the one who in less than a week developed the most perfect skill in navigating that rushing torrent with a heavy boat, was Mr. Reginald Travers, whose only preparation for such work, from the time he was seventeen till he was twenty-eight, when he joined the expedition, was that of a stock broker on one of the New York exchanges, and an amateur oarsman on Flushing Bay.

Mr. Sears and Mr. Nichols are each led into one error when they quote me as saying the road could only be built from the two ends. This is fully set out on pages 318 and 319, where I say that "work in the Cañon Division could be carried on simultaneously at at least forty different places," by existing roads and trails into the cañons on both sides of the river.

I certainly appreciate what these same gentlemen, and others, say in regard to the effects of great storms on work built on earth and gravel flats in cañons, having had some experience with such destructive forces in Clear Creek Cañon while engaged on the Union Pacific Railway. If called upon to build this line, I would use as my principle of location the quotation made by Mr. Crowell from one of my letters, that of placing the road-bed "on as permanent and safe ground as the nature of the cañon walls will permit, believing as I do that this 'railway of the future' should be built for ultimate value and profit even at a much greater cost."

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As to the flats, which I suggested in my paper as available for use, and which would constitute plow and scraper work in the Cañon Division, the much larger number are in Glen Cañon, where for 150 miles the river has an average fall of 2.17 feet per mile, and where the flats exist, of about five-tenths of a foot per mile, and is from 1 000 to 1 800 feet in width. With these features, and the heavy rock work necessary in almost every instance at the upper approach to the earthwork, it is believed that these portions of the line can be made perfectly secure, except from a cloud burst directly over the roadway, which contingency cannot be provided for even on the dry plains of Nebraska. Of the remaining flats in the steeper portions of the cañons, and the 51 miles of "hillside slopes" in the Grand Cañon, almost every mile is situated on top of the first rock bench, with a vertical or sloping wall of from 10 to 80 feet of solid rock next to the water, protecting the earthwork from being undermined by the river.

These features are clearly shown at every point by the series of photographs, and also the necessity, and it is believed the perfect security, of the short stretches of "embankment in the river" in Glen Cañon, to which these gentlemen and Mr. Le Baron call attention. These embankments would be but newly formed talus slopes built entirely of solid rock, which Mr. Nichols says "can be utilized where they are composed at the base of heavy stones, and are not exposed to running slides from above," conditions which would exist in every instance. The experience of these gentlemen in Peru is most valuable, and I only hope that I shall have the opportunity of profiting by it in the cañons of the Colorado.

No one engaged in the investigation of a subject like that under consideration, can more highly appreciate adverse criticism than I, when it throws any light upon one's errors of judgment, want of experience, or facts. Mr. Latham Anderson does me the honor to say that my paper "is a valuable contribution to our knowledge of the geography of this continent." It is to be regretted that Mr. Anderson did not take advantage of the geographical information and other facts therein contained, when he wrote his part of the discussion. Nowhere have I stated that the "sole object" of this proposed railway is to transport the coal of Utah to the Pacific Coast. But assuming this to be the case, Mr. Anderson proceeds to point out a line from Frisco to Barstow, and says, "it has one crowning advantage over the line in the chasm." This is simply begging the question. A slight use of the valuable geographical information will show that no earthly comparison can possibly exist between his suggested line and my line "in the chasm," for the portion of my line from the Utah coal fields has not one foot of its length "in the chasm," or "in the cañon." Where it is, and what it is, certainly is clearly set out on pages 315, 329, 330 and 331.

I accept, without a question, Mr. Anderson's description of the

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country through which his proposed line would run. Nor do I doubt for one moment that there exist along it "rich mining districts of the precious metals." Yet Mr. Anderson wipes out with one stroke of his pen all the evidence in existence of the mineral deposits of Lost and Gold Basins, Mineral Park and Eldorado Cañon, and the whole country tributary to the Colorado, from the Virgin to San Diego. The evidence is given in brief on page 330. A little use of the geological as well as other facts given in the paper, would have saved Mr. Anderson the trouble of perching his imaginary mining man a mile high on the brink of the precipice, and his sympathy for the tired legs of his friend, even though he were in the position assumed, would all vanish if Mr. Anderson would make a visit to Ouray, Colorado, and examine the toll road built from the town to the mines above, around a precipice as formidable for wagon road purposes as any in the Grand Cañon, and witness the ore wagons, each loaded with three tons of ore, rolling down that smooth and narrow way. Transfer that picture to the great ore bodies in the Grand and Side Cañons from "100 to 1 000 feet above the river, and from 4 000 to 5 000 feet below the plateau country" (page 329), and I think my case is made.

Every one is entitled to his own opinion; but before one accepts the sweeping assertions of Mr. Anderson and Mr. Barlow in the face of actual facts given, it might be well for one to consider some of the "inhospitable and barren" regions of the State of Colorado, and note their past and present local railroad business, and carefully read a folder issued by the Northern Pacific Railroad containing the famous speech of the Hon. Proctor Knott, of Kentucky, on the City of Duluth, delivered in 1871.

Mr. Barlow falls into the same unpardonable error with Mr. Anderson, when he compares the cost of the "entire distance" of the Cañon route with the cost of the Union Pacific line from Milford west. The only possible just comparison must be made, as Mr. Barlow suggests (but does not follow), of the "same portions of each route." Within the last year I have made some quite extensive examinations of various crossings of the Sierra Nevadas from Tuolumne County, California, south to the Mexican line, and I confess myself considerably surprised to learn, as Mr. Barlow states, that a railroad "can be built" across these mountains at a cost of from $4 000 to $4 800 per mile. The comparisons that Mr. Barlow assumes, but does not make, say from San Diego east to even points Cheyenne and Denver, would make a most interesting paper from him, and I shall be glad to furnish him with all information in regard to the Cañon route. Will he kindly accept the offer? It was distinctly stated that the question in its full bearing, "Will it pay?" was "outside the scope of this paper." However, the remarks of Mr. Barlow and Mr. Kingman are pertinent.

Mr. Barlow's answers to the three sources of revenue are somewhat

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surprising. *First.*—His statements clearly imply that no one road can ever have any advantage over others in through traffic except in the ability of its traffic agencies to secure the business. I need only suggest a reading of the clear statements of Mr. Rowe and Mr. Crowell on this point. He also assumes that all transcontinental business for this line must be "confined to the district of Southern California," an assumption completely answered by himself when he says that the Union and Central (and I may add the Northern and Canadian) Pacific roads compete for this California business.

Second.—His estimates of cost of transportation are based on two errors, the selling price of coal at San Diego and San Francisco, and the contemplated methods of final distribution. I need not occupy the space to correct these. Mr. Kingman brings them out much more clearly.

Third.—Mr. Barlow's answer to the question of local business, is simply an opinion of an extensive mineral country—of gold, silver, lead, copper, iron and coal, of which he says he has no personal knowledge.

Mr. Kingman, it seems to me, occupies two or three very untenable positions. His figures and facts are interesting, but not complete. Analyzing his statements, the first position taken is that no road can have any advantage over another in the transportation of coal (and I suppose other freight) except in the matter of distance. If this were the case we should give up all attempts to better the grades, etc , of existing roads and stop building new ones except in absolutely straight lines.

I claim for the proposed road the advantages so well put by Mr. Rowe and others, over existing railways through the same section of country, not only for coal, but for all through and local business of whatever kind. If the cost of coal freight over the Atlantic and Pacific Railroad is, as Mr. Kingman states, three-quarters of a cent per ton-mile, I claim for the cañon route one-half of a cent per ton-mile. See profiles and statements of grades, etc., etc.

There are some slight discrepancies in the various distances given by us over the Santa Fé system. Mine were taken from their folder and may be wrong. The distance from Gallup to Needles is given as 420 miles. Using Mr. Kingman's figures, this Gallup coal costs at Needles $4 15 per ton. From the Utah coal fields to Needles by the Virgin and Colorado River lines (taking Mr. Kingman's distance to San Diego—600 miles) is 236 miles. Coal as before at $1, and freight at one-half cent per ton-mile, would cost $2 18 at the Needles. Which would allow it to go nearly 150 miles east over the Atlantic and Pacific till it met the Gallup coal in price, and would forever drive Gallup coal out of any California market, even over their own road. Of course I only take into account Mr. Kingman's position of cost and carriage and make no note of unbalanced grades or freights East and West. If quality be considered, even Colorado coal at one-half cent per ton-mile could be delivered at Needles

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and supply the Atlantic and Pacific with all they use at that point and in California as against Gallup coal. To a much greater extent would these results obtain at Yuma with the Southern Pacific road. It is not necessary to give the figures.

The prices of foreign coal at San Francisco, Los Angeles and San Diego given by Mr. Kingman are of course correct. But coal of a quality equal to the Wellington by the Colorado River line at one-half cent per ton-mile, would cost at San Diego $4 per ton of 2 000 pounds, and by water to San Francisco per ton of 2 240 pounds only $5 43, as against $8 25 to $8 75 in San Diego, and $7 to $8 (of like quality) in San Francisco. The same coal could be carried by the Atlantic and Pacific road from Needles to Los Angeles and on the same basis sold for $5 05, and even from San Diego by rail for $5 55 (allowing three-quarter cent freight from Needles to San Diego), as against $8 50 and $10 for Gallup and Wellington; again using Mr. Kingman's figures, and the long ton. Hence, I fail to see why the market for the Colorado River road's coal should be confined exclusively to the present demand of San Diego. My statement that coal could be sold for one-half to one-third the present market price, was intended to be confined to the port of San Diego and the coal and coke of Utah.

The last position taken by Mr. Kingman strikes me as the most remarkable of all, that were this line built, its passenger and freight business would be, and forever remain, as compared with lines terminating at other points in California, in the exact proportion with the present coal trade and the present population of the cities of San Diego, Los Angeles and San Francisco.

With good coal at San Diego at $4 per ton (and this could be done F. O. B. of vessel, per ton of 2 240 pounds), the whole situation would be changed. There would then not only be 38 000 tons to be delivered at San Diego, but the whole supply for the two railroads at Needles and Yuma, and the mining, milling and smelting industries of the whole lower Colorado valley, the whole supply of San Diego, Los Angeles, and all Southern and Lower California, and a portion at least of the consumption of San Francisco. To these would be added the whole south coast coal trade, Central and part of South America, and Mr. Earley's suggested market in Mexico, and, as I am informed by the largest coal dealers on the coast, many of the coaling stations on the Pacific.

There would then not be, as Mr. Kingman says, only eight cars per day of coal business for the new road, but nearer one hundred and fifty to two hundred cars per day within a short time after completion.

With such conditions and prices, San Diego, becoming an exporting instead of an importing coal station, the whole traffic of the South Pacific would be adjusted on new lines. An ocean traffic would be attracted to this port much larger than at first sight could seem possible,

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but all depending upon an unlimited supply of cheap fuel. It is not impossible that many of the now existing manufactories on the coast would move south to cheaper fuel and iron. Witness the departure of so many such industries from around Pittsburg to Alabama, Illinois and Indiana.

I do not for one moment recall anything said as to the extensive local interests of the upper cañons, or the great advantages of this line as a transcontinental route, but shall leave those to the simple statements of the paper itself, and add one point as to the prospective development of the far Southwest.

Mr. Kingman admits that "the mining interests of the country are considerable," and also shows just why the business is at a standstill, and then draws a dismal picture of the country through which the Atlantic and Pacific Railroad runs, and its failure to develop the mines of Northern Arizona. Ample reason for this is given in my paper. Allow me a moment to suppose a case. Consider the cañon road completed, with its many short and some long branches; at or near the mouth of the Virgin establish on the bank of the river, say, perhaps, at Old Callville, a milling and smelting plant for the reduction of gold, silver, lead, copper and iron ores. Into its mills and furnaces would pour, as into a funnel, by down hill railroads, from every point of the compass save one, the various ores from Southern Nevada, the precious metals, the iron and coal and coke from Southwestern Utah; and from Northwestern Arizona the ores and fluxes for hundreds of miles up the great cañons. Cheaply constructed branch lines would bring also the ores from those sections lying south and east of the river, and on account of the low grades, from far down the lower river valley. Does such a business as would certainly thus be developed, not only handling these raw materials, but the commercial necessities of so large a population as would thus be gathered together, depend in the least upon the depth of a dry well at Winslow or Ash Fork? The facts to show that this supposition is not beyond strict business principles can easily be had. Is there another point on any other present or prospective railroad in the section we are considering where such conditions can be brought together?

What, then, may yet be the local and through business of such a road in this far Southwest? Mr. Kingman is mistaken when he says no good grass grows in the lower Colorado Valley. I have walked through miles of it, and slept on acres of it, and have seen grapes, peaches, plums, pears, figs, pomegranates, nectarines, almonds, limes, oranges and lemons grow upon the banks of the Colorado and its tributaries at different points from the Virgin to the mouth of the river, and more are being planted every year. San Diego County, California, which is larger than the States of Massachusetts, Connecticut and Rhode Island combined, has, west of the Colorado basin, in its Spanish grants and

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ntain ranches, over 1 000 000 acres of good fruit and grain land. in the last two years new irrigation districts have been organized r the State law, covering over 250 000 acres, while there are com- d or under construction private irrigation plants capable of water- 80 000 acres. In the year 1891 there were set out in this county 00 new fruit trees. What freight will this give a railroad? In 1890 less than 3 500 acres—about 200 000 trees, besides smaller plants— ingle station of Riverside shipped 1 800 car loads of fruit to the ern market. While one ranch near San Diego has the capacity, the te, the soil and the water to produce more than ten times this nt of oranges, lemons and raisins.

me one will say the Santa Fé already reaches this section with a line to Chicago. Yes, and I have seen its trains standing in the at National City unable to depart for want of a tender of coal at rice.

ut I must desist. Until it is better known the far Southwest will urse be misjudged. In conclusion, with a road such as I have sug- d, with a long division possibly operated by electricity generated e same power that cut those mighty cañons, and transcontinental portation readjusted by the necessity of quicker time, cheaper hts, and the completion of the great canal, this line, with its low es, favorable climatic conditions, its perfect water supply, its rapidly loped local business, and the attractions of its wonderful scenery, d in my humble opinion be able to earn dividends on a very high of construction—if that were necessary—while the upper trans- ntain lines could not carry on their business at the same prices and its competition.

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PLATE XXVI.
TRANS. AM. SOC. CIV. ENGS.
VOL. XXVI, No. 523.
STANTON ON CANONS OF THE COLORADO

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AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

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524.
(Vol. XXV.—April, 1892.)

BRICK MANUFACTURE AND BRICK PAVEMENT.

BY F. A. CALKINS, M. Am. Soc. C. E.

WITH DISCUSSION.

PART I.—ELEMENTS AND METHODS.

The art of brick making is supposed to have originated in ancient times with the powerful races found in Egypt, Chaldea and China in the Old World, and in Mexico and Peru in the New. The ruins of buildings and tombs left in these countries testify that they were skillful builders. Rawlinson states that the dimensions of bricks baked by the Chaldeans were 11½ inches square and 2½ inches thick, and those of a later date 13 inches square and 3 inches thick. All are familiar with the Egyptian method of tempering clay with a stick and drying in the sun three thousand years ago. Compare the ancient method with the practice at the present time in any small town where bricks are made, or even at the brick yards on the Hudson; there pug mills will be found in operation which are only a combination of several sticks and a mule; and

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green bricks are still dried in the sun the same as was practiced by the ancients, showing little advance in three thousand years.

The first bricks manufactured and burned in this country were at New Haven, Conn., in the year 1650; and the next city to claim early manufacture is Philadelphia, where they were made thirty-five years later, in 1685. The first brick machine of which we have any record was made in Philadelphia in 1840. It was a soft clay machine operated by horse-power. This machine was destroyed in 1844 during a riot, caused by the brickmakers fearing it would take bread out of their mouths. In 1847 a dry clay machine was made in the latter city. That the moulding of bricks by hand can be done rapidly was clearly demonstrated at Cincinnati in 1832. The moulding of nine thousand bricks was then considered a regular day's work. In a contest at this date between two expert moulders, working from sunrise to sunset, one moulded 24 700 bricks, and the other 25 470. A similar number were moulded at a contest in Philadelphia at a later date.

The clays considered in this paper, treated by improved methods, are located in New Jersey and Illinois. The clays of New Jersey were deposited by a glacial drift in the Tertiary Period, and compose a portion of the belt designated by geologists as the plastic belt, which extends from Long Island through New Jersey to Georgia. Near Philadelphia this belt is about 6 miles wide, and of increasing width to the southern portion of Virginia, where it is more than 80 miles wide. It is called plastic, as it is susceptible of being moulded without the admixture of any foreign substance. Most clays outside of the plastic belt originated in the recent or Quaternary Period. The clays of the central and eastern section of Illinois are included in the upper coal measure formation, and the western portion of the State in the lower coal measure.

Constituent Elements.—The properties of clays are modified by composition. The physical structure has weight to determine the quality and density, yet fineness and division of particles are also controlling points. The theory advanced by the late Professor George H. Cook, formerly State Geologist of New Jersey, is the best received at present. He states "that the microscope reveals a crystalline structure which the eye does not detect, and that this structure varies greatly in different samples; some are composed of masses of hexagonal plates or scales piled up in long bundles and masses of unattached scales nearly

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perfect. Such clays are always but little plastic, but may become so on mechanical treatment, such as grinding and kneading. On examination after receiving that treatment the clay shows the same elements of structure, but broken and confused, no bundles left intact, scales broken, and a homogeneous mixture of cracked material, still crystalline. Clay beds are found with the clay of this broken up form, still a crystalline mass, and on the degree in which the crystalline structure is retained its plasticity depends." As this examination was made only with plastic clay it does not apply to flint clays.

Professor Edward Orton, in his report on the Economic Geology of Ohio, Vol. V, states that "the mineralogical structure is worthy of consideration as well as the chemical one with reference to their value. The minerals going to compose clay are kaolinite, quartz, mica, feldspar, oxide of iron, lime and magnesia probably as silicates of the alkalies, sulphate of iron, titanate of iron and various rare constituents such as cobalt, copper, zinc, etc.; all of them are not found in each clay. There is no theory worth consideration at present concerning the difference between flint and plastic clays. It is a point in chemical composition, or water hydration, and is, it seems, at most dependent on the physical structure and may be studied with best effect through the microscope." "The impurities most to be dreaded in refractory clays are iron and potash, and it is hard to state which is most to be feared. Iron is not so powerful a flux as potash, which is the worst of all common elements, but the iron is present in larger amounts than potash in most clays, and consequently does as much harm, if not more. If iron is present as the sesquioxide it takes more heat than when in the protoxide state to combine with clay, for iron will only combine with silica in the protoxide state, and if that state is already developed it is easier to combine the sand and iron than if in the oxide. Sulphate of iron has a bad effect on clay, since its decomposition gives rise to a lower oxide of iron. Silicate of iron is also detrimental, since it melts at a comparatively low temperature. Lime and magnesia act as fluxes on clay."

Testing Clay.—In the report of Professor Orton can be found a method of making a chemical analysis of clay that recognizes its physical structure, classifying together bodies that are alike, which will enable a correct comparison to be made. The chemical test should be followed by a microscopic examination of its mineralogical structure. The data thus obtained of the properties of the clay will result in a correct com-

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parison of the nature of clays and their value for any use possible. When the tests described are completed they should be supplemented by a fire test to develop points indicated by the analysis to be weak ones. Fire tests are of two kinds; one is to subject the clay to a high degree of heat (without the admixture of any foreign substance), which will develop the quality of the clay to endure heat. The other test is to put the clay through a course of treatment for its intended use, which will decide the fitness of the clay for the purposes intended. If manufacturers would submit their clay to a series of tests of the character described, it would reveal to them the best methods of treatment, and avoid the loss of thousands of dollars caused by the thumb-rule of experiments in general use.

TEMPERING CLAY.—The existing prejudice against bricks made by machinery the author believes to be due to the fact that clay is often taken direct to the machine imperfectly tempered. As a brick machine usually possesses the necessary power to make brick without tempering the clay, some manufacturers neglect it entirely or do it imperfectly. When a broken brick fails to show a homogeneous structure it clearly indicates that a clay has not been well tempered before pressing. There is no necessity for this condition in a machine-made brick, as tempering machines in great variety can be obtained, adapted to every condition. Machine-made bricks from well tempered clay are more dense in their interior than on the outer surface, but when poorly tempered it is the reverse.

There are three distinct processes utilized for treating clay for machine-made brick: the soft mud, the stiff mud, and the dry process. A more uniform nomenclature is preferable; therefore we will designate them as the soft clay process, stiff clay process, and the dry clay process. The tempering for a soft clay and the stiff clay process is identical, differing only in the quantity of water added to bring the clay to the proper consistency, which is variable and controlled by the nature of the clay. Ordinary clay when treated by the soft and stiff clay process is improved by being thrown up and exposed to the rain and frost, or placed in a clay-house and wet, to allow dry lumps to absorb moisture, although Mr. Eudaly says this is not always so. A better plan is to just pug the clay and allow it to lay a few days before using. If not practicable to prepare clay in advance, a granulator and crusher is necessary to thoroughly crush and pulverize all tough and dry lumps. When clay is free from

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stone the smooth roll crusher with differential motion is useful, but if stones are present a compound four-roll crusher with top rolls corrugated to remove the stones should be used.

When clay is deposited in a pit it can be handled economically to the machine by means of a conveyor. For the dry clay process it is essential that the clay be of considerable depth, and enough should be hauled under cover to last several months to permit its moisture to become uniform throughout, but it should be dry enough to allow the bricks to be set in the kiln at once. Some clays are found that do not require previous exposure, which will be explained in the description of plants, Part 2.

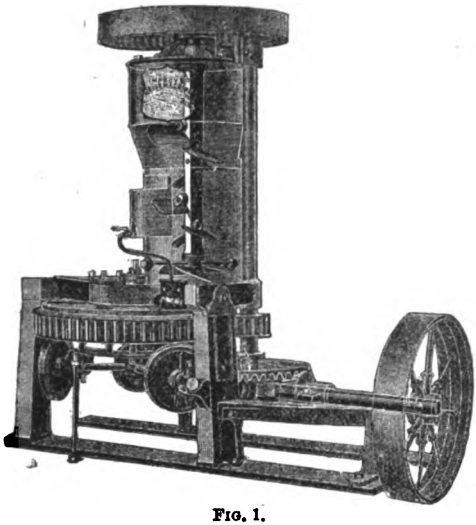

FIG. 1.

MACHINES IN BRICK MAKING.—A large portion of the brick plants operated by modern methods date to a period within seven years. When

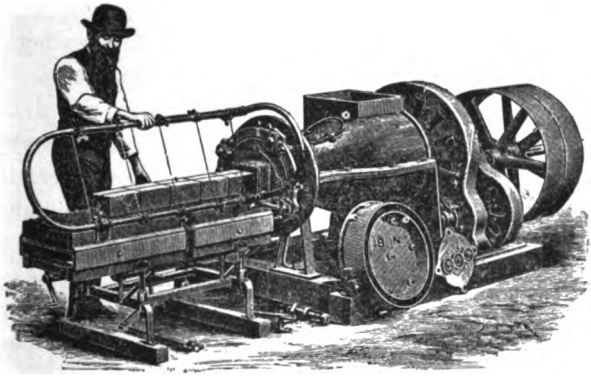

FIG. 2.

we consider the accuracy with which a machine works, what delays their exclusive use in brick making? First, it is largely due to cost of modern machine methods. Second, it can be traced to the violation of the first

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principles of treating clay, either in the use or adaptability of the machine. It is the use of clay direct from the bank, untempered, that has produced inferior bricks, and not the machines.

Machines for treating clay by the stiff clay process are of two types, the mould and the die press. In the former the clay is pressed into steel lined moulds (set in a revolving horizontal wheel) by the action of spirally arranged knives on a vertical shaft (Fig. 1). A follower closely fitting the mould elevates the bricks above the surface of the wheel. The die machines are divided into two classes, the auger and the plunger. The

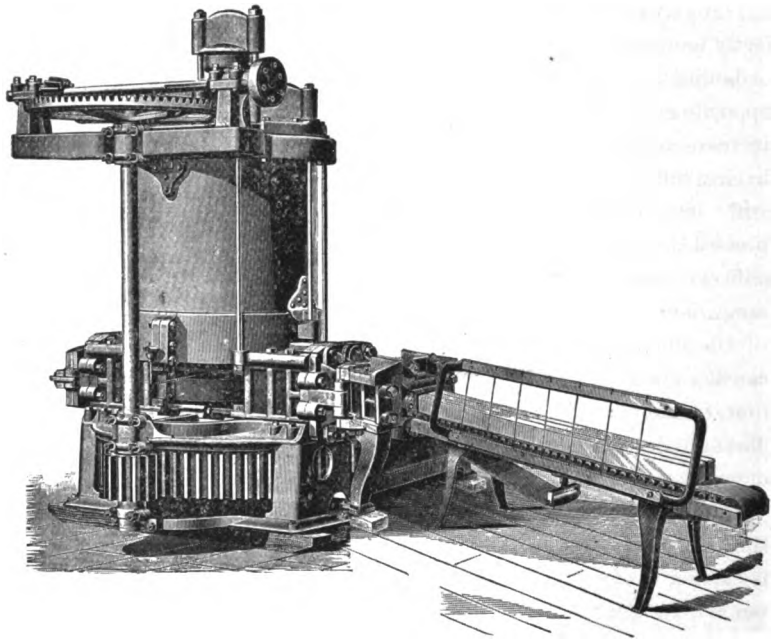

FIG. 3.

clay in the auger type (Fig. 2) is forced through a die by a series of knives arranged spirally on a horizontal shaft. The machine discharges the clay in a continuous bar or bars, permitting it to be cut in sections by steel wires, operated by hand or automatically, cutting the width of a brick and designated as side cut, or, the length of a brick and known as end cut.

In the plunger type (Fig. 3) the clay is forced by knives and a mud ring downward into a chamber from which it passes through the die by the action of a reciprocating piston or plunger. The clay passes from the die

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a cutting board, where it is cut into bricks by the methods explained
he auger type. The moulded clay on leaving the machine is strong
gh to be removed safely with ordinary care. The objections to this
hod are that when clay is forced through a die it has a grain in the
ction in which the clay is forced. If side cut, the grain runs vertical;
nd cut, it is in longitudinal layers or laminations, the latter splitting
ly, but being difficult to cut crosswise when required in building
onry. When side cut, it is easily broken, but cannot be split. Defects
ıd in bricks by either method have doubtless been caused from
erfectly tempered material, for it hardly appears possible that per-
ly tempered material forced into a die would be deposited in layers
aminations sufficiently to diminish the quality, but rather that the
osite effect would be produced, as the great pressure applied would
ease the strength and make it more homogeneous. Side cut bricks
ing finished sides and ends are often utilized for face bricks. The
clay process is adapted to prepare bricks and tiles to be re-
ssed for ornamental as well as for plain work. Bricks made by a
clay machine are 40 per cent. stronger than hand-made from the
e clay.

Die moulding machines have been made of a faulty construction,
sing a tendency of the die in some cases to produce seams or lamina-
s in the brick, but this defect has been overcome by lubricating the
and also by layer forcing propellers, and this has also overcome the
ved form in the brick.

Dry Clay Machines.—By this method the clay is tempered by passing
ough a pulverizer, which reduces it to fine powder. This powder
arried by conveyors to a hopper above the press. Under the hop-
is a charger open at top and bottom, which travels forward, receiving
harge of clay from a hopper, and then moves backward, dropping the
into molds. During each alternate motion plungers descend and
s the clay into bricks. After the compression the upper and lower
ngers are both lifted upward; the upper enough to permit the charge
ass forward as before; the lower plungers move upward until the
surface is level with a table in which the charger slides. The lower
ngers here remain stationary for a short period, until the charger
ing forward pushes the bricks off. When the bricks are started off,
plungers drop and the moulds receive another charge of clay. The
nents of a dry clay brick machine are the parts for compression, for

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charging, the table and the mould box; the plungers, the parts for discharging, the master wheel and automatic cams.

There are two types of machines for pressing dry clay bricks. In one the lower set of plungers remains motionless and the upper does all the pressing; in the other both top and bottom plungers begin to move toward each other at the same time, and although the amount of travel differs, yet the last small space of compression is performed half by each plunger. The latter method tends to preserve the corners of the bricks in a more perfect condition. There is no practical difference in solidity of bricks made by the two types of machines. Fig. 4 shows a dry press utilizing the latter method.

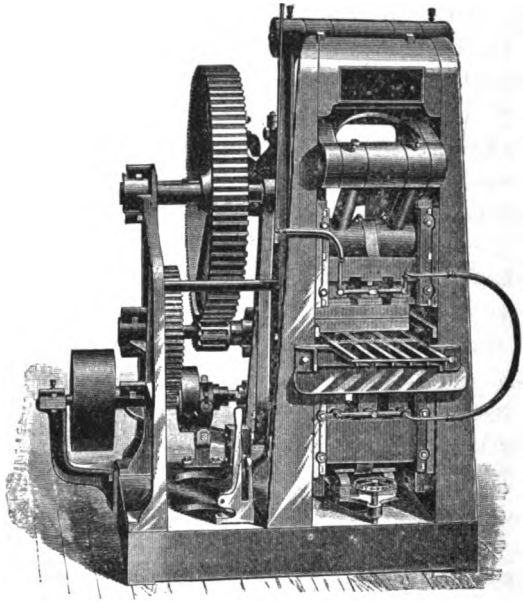

FIG. 4.

DRYING BRICK.—Clay moulded in a dry press does not require any artificial drying, but is removed direct from the machine to the kiln; but a better grade of brick can be made by drying. All clays treated by the soft and stiff clay process are dried after moulding from twelve to forty-eight hours' time, as determined by the quantity of moisture contained in the clay, the quality of the material and the method utilized for drying; and the bricks are then transferred to the kiln.

Only a brief outline of the methods in use for drying will be given.

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The hot floor method is in more general use than any other, which is, doubtless, due to the fact that it is less expensive to construct. A hot floor is constructed of a series of heated parallel brick flues; the arches are connected and covered with brick, producing a uniform flat surface on which is deposited a layer of earth 4 to 12 inches in thickness, which is surfaced with one layer of bricks laid flat. The flues are connected with smoke stacks, the number controlled by the dimension of the drying floor. The green bricks are hacked direct from the press to the drying floor, where they remain until fit for the kiln. Coal is in general use for fuel, yet gas and crude oil are utilized also for heating.

By another method plates of iron are used to form the floor, which is heated with hot steam from the engine while running, or from a boiler when at rest. Long flues are preferable, as they produce more uniformity in heat, and there is less danger of checking the green bricks. Some varieties of clay will endure forcing to any extent, while others must be dried slowly at first, at a temperature not to exceed 90 degrees, followed by increased heat. For other varieties the heat must be retained at a uniform low temperature.

In a third method the floors are built in stories and heated by either live or exaust steam. In a fourth method the bricks are hacked on a slated floor which is heated by steam pipes placed underneath, live and exhaust steam being utilized. The fifth consists of a series of brick tunnels permitting cars loaded with green bricks to enter and remain until the bricks are dry, the loaded car being then taken direct to the kiln. The tunnels are heated by steam pipes located either above or below the tunnels. The sixth method is a combination of the drying floor and tunnel system, constructed of iron and heated by steam.

The weight of evidence from successful manufacturers confirms the mechanical and financial success of utilizing artificial heat. When bricks are sun dried the loss during one season is from 20 to 50 per cent., while the manufacturer must cease work at the beginning of winter. But when artificial drying is used, work may continue during the whole season, provided clay has been housed, and no loss or delay at any time occurs from stormy weather. It will cost about five cents per thousand to hack bricks from the press to the drying floor, and one ton of coal will dry five thousand bricks. A green stiff clay brick contains more than a pound of water to be evaporated, and a kiln of bricks would, therefore, require from 16 000 to 300 000 pounds of water to be evaporated; hence

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the important fact is developed that much care and attention should be given to drying methods.

SETTING BRICK.—The method of setting bricks in the kiln materially affects the results of burning. The methods described will apply particularly to kilns provided with permanent walls. In an up draft kiln the bricks are set with a wider space between those at the bottom than at the top, but in a down draft kiln the reverse is the rule. By setting them in this manner the draft is increased, thus facilitating the distribution of the heat. Bricks should be set not only to obtain a perfect draft, but also to utilize all the heat possible from the combustion of the fuel, and to leave the least number of marks on them when burned. Ordinarily the space between bricks is 1 inch, but to facilitate the distribution of heat throughout the kiln, the space is increased or diminished to suit the condition of the bricks. The methods of setting are three over three, four over four and five over five. The cost of setting is variable in different localities, ranging from thirty to sixty-four cents per thousand. Three men will set from twenty-five thousand to thirty thousand per day.

BURNING BRICK.—Where improved methods of burning are in use the old style of kiln built of green bricks and plastered with clay on the outer surface to retain the heat, is discarded. Stationary kilns constructed of brick with permanent walls about 2 feet in thickness, are utilized. Another type is the continuous kiln, which is divided into sections, permitting the waste heat in one compartment to be utilized to heat the green bricks in adjacent ones. The principle of the continuous kilns is to utilize the heat contained in the burned bricks during cooling, in partially heating the green bricks set in adjacent compartments.

The initial process in burning brick is the removal of the excess of moisture in the green bricks, which requires a temperature of 212 degrees. This is called water smoking, the time required to accomplish which is from four to six days (being controlled by the amount of moisture the bricks contain), and from three to four days more to complete the burning and cooling. Slow water smoking is considered preferable, as a rapid change in temperature tends to check the bricks. This checking is produced by too rapid drying of the outer surface, before the moisture contained in the center of the bricks has been evaporated. The extraneous water should be driven off before the heat is increased sufficiently to eliminate the combined water. This applies with greater force when much lime is present in the clay.

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The draft in common kilns built entirely of green bricks is upward; is is the same largely in kilns constructed with permanent walls. The rmanent up draft kilns are constructed with side and end walls 2 feet more in thickness, but open at top. The kiln is filled with green icks, and will hold thirty to forty courses in height, of brick set on edge. retain the heat, the top course is covered with brick bats and imper- t bricks, then plastered with clay. The down draft kilns are provided th a brick roof, and this type may be constructed to permit the up and wn draft method to be combined in one kiln; but there is no special vantage gained by this method.

A brick may look well on the surface, but when broken across the ter show only a crust half an inch in thickness that is well burned, licating that the heat necessary to burn a perfect brick was not con- ued long enough to produce uniform hardness. As nearly all clays l shrink under certain degrees of heat, the surface of a brick, when rder than the interior, permits unequal contraction to take place, and en subject to uneven pressure it is in a condition to break easily.

Bricks moulded by the dry clay process require a longer time to water oke than if moulded by any other. As the material is subject to ater pressure it is more dense, consequently contains more clay, and l require a longer time to evaporate moisture. A full and free draft ecessary during water smoking. Down draft kilns are preferred for rning this class of brick.

All clays that shrink much in burning possess alkaline salts, and heated too rapidly before the moisture contained in the center of the ck has been removed, the salts will effervesce; and if the kiln is sed before the salts are oxidized or burnt off, the bricks will become colored.

Chemical Change in Burning.—The elements of chemical change in rning moulded bricks are: first, complete removal of moisture, which accomplished by maintaining a temperature of 212 degrees Fahr. veral days after the temperature of the kiln has reached this degree of at. Next, elimination of the water of crystallization, during which the y becomes rigid, shrinking in volume. After and during expulsion combined water, the organic matter burns out. Shrinking ceases en all water of crystallization has been expelled up to red heat, yet th increased heat shrinking will continue up to the moment of fusion. od flint clay containing some sand will lose all shrinkage on being

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once calcined at white heat. Some clays possess a very high percentage of silicate of alumina, containing little, if any, foreign matter. Carbonate of magnesia will lose its water of crystallization at as low a temperature as 60 degrees Fahr.; then cold water decomposes it and the result is an insoluble white hydrate in powder, seen on the cracked bricks in the kiln. The temperature required to complete the burning of a kiln of bricks is from 2 200 to 2 600 degrees Fahr., but burners usually determine the degree by the appearance or color of bricks. The degree of temperature applied during water smoking is ascertained by means of a Fahrenheit thermometer; for higher temperatures the pyrometer is the only reliable instrument, yet the latter has only been brought into recent use and by a very limited number of manufacturers.

The present prevailing practice and the tendency to a large increase in the use of improved machinery and methods, is very encouraging to any having an interest in the development of a good brick; this tendency particularly applies to the manufacture as carried on near cities located in the central States. That the thumb-rule and the stick and mule "combine" will soon be relegated to the past, is the devout wish of the writer. This "combine" has promoted the brick industry in the past, therefore deserves a fitting eulogy, then to be silently buried in clay.

Part II.—Plants.

The Lorillard Brick Works, located about two miles south of Keyport, N. J., are among the largest in the United States.

The soil overlaying the bed of clay utilized here has a depth of five feet or more. The company controls 300 acres of land. After the removal of the surface soil the clay is excavated with steam dredgers and deposited on dump cars which are hauled to an incline and there transferred up an inclined trestle (by means of a cable) into the clayhouse. The cars being side dumping, can discharge the clay on either side of the trestle into a pit below, from which two belts elevate the clay to the tempering machine. These belts are 2 feet in width, and the clay is deposited on them by two men stationed at the lowest point, provided with long handled shovels, while a third man sprinkles sand over the clay as it passes him by the forward motion of the belts. All material after leaving the clay pit is manipulated with mechanical power until delivered in the brick kilns to be set for burning, except the cutting the bricks at the press and their transfer thence to the cars. Fig. 5

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show the arrangement of the granulator, crusher, pug mill and k press.

One man is stationed at the granulator and crusher to remove obstruc-s, should any occur in feeding. The granulator is similar in con-ction to the pug mill (Fig. 7), yet is adapted to receive the clay ct from the clayhouse. The belt from the clay pit discharges erial into the granulator, which is provided with radiating knives to k up the lumps of crude clay. The granulator discharges the clay, uniform rate and in a uniform condition into the crusher (Fig. 6).

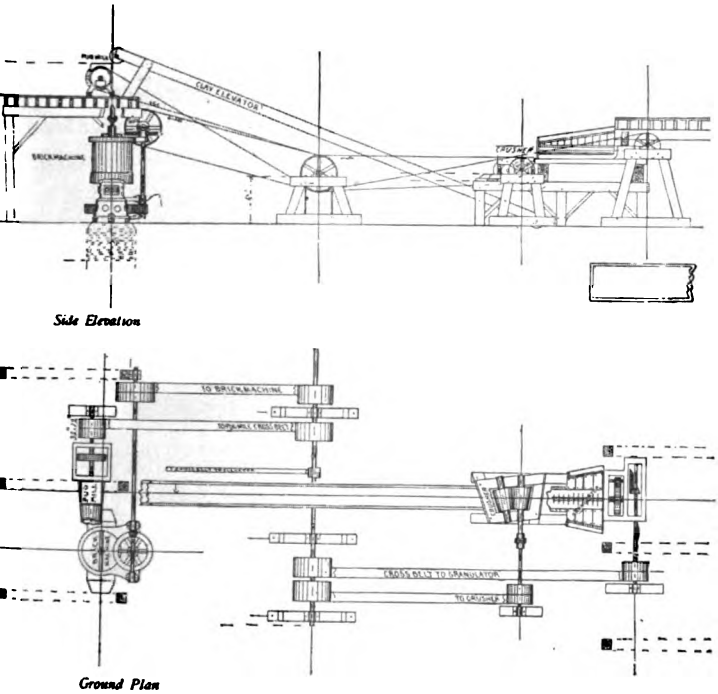

FIG. 5.

the point of discharge the granulator has reduced all lumps to about e size of eggs, and capable of manipulating enough clay to make fifty eighty thousand bricks per day.

The crusher is provided with rollers and reduces the clay to thin eets, which are deposited on a second endless belt utilized to elevate e clay to a height of 14 feet above the floor, there discharging it into e pug mill, located adjacent to the brick machine shown in Figs. 5 and The duty of the pug mill is to complete the requisite tempering of

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the clay, reducing it to a plastic mass before it enters the brick press. A pug mill similar in design is now considered an important factor of the

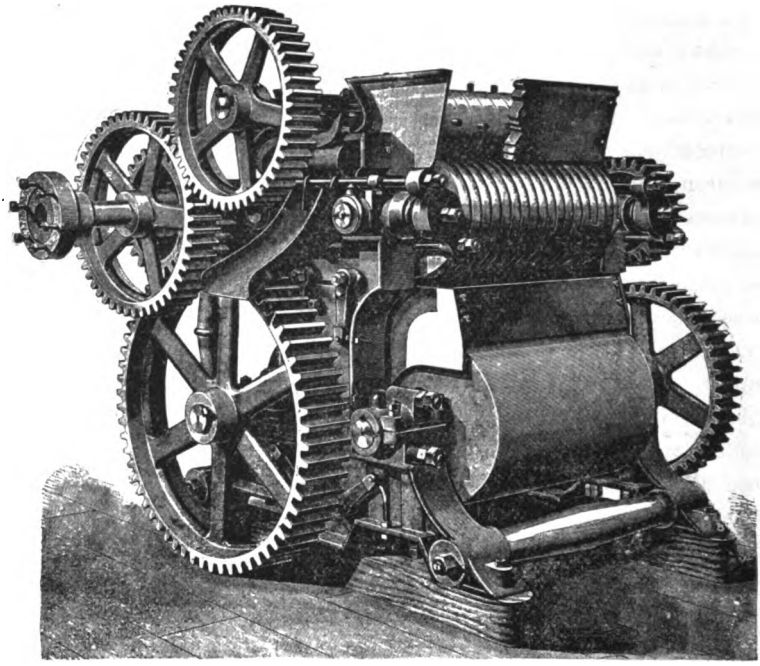

FIG. 6.

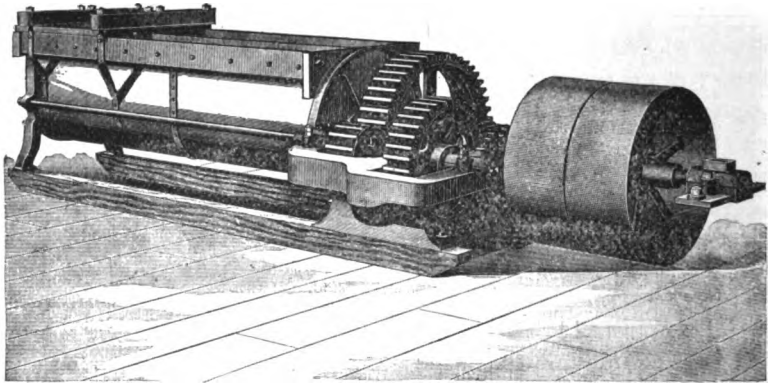

FIG. 7.

success of any well regulated plant. The construction of the pug mill and granulator is similar, both being provided with cutting knives;

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but those in the latter are arranged to reduce the large lumps, and in the former to manipulate the material which has been partially tempered by the granulator and crusher, reducing the rolled clay now in sheets to a plastic mass, in which condition it enters the brick machine.

The brick press illustrated in Fig. 3 is of the plunger type in construction. The clay enters the drums or tempering cylinder, in the center of which is a shaft filled with blades that grind the clay and force it through a porthole into the pressing chamber. A plunger driver then presses the clay through the die and on the cut off table, as shown. The mechanical device used to propel the plunger is a steel cam placed on the main shaft between the upper and lower bed plates. It operates the rollers at the front and rear ends of the sliding frame, to which the plunger is attached, providing an alternate forward and backward motion at each revolution of the shaft. Nearly any conceivable size and form of brick can be made by change of die. This press is capable of turning out sixty to eighty thousand bricks in ten hours. The grain of the bricks made by this machine is vertical.

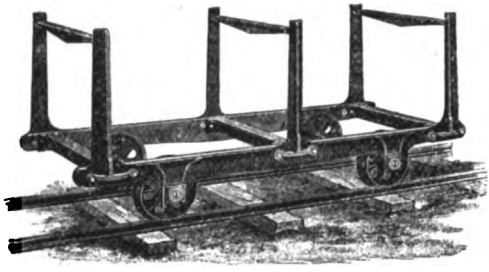

FIG. 8.

At each revolution of the machine the plunger moves forward and forces the bar of clay through the die and upon the apron of the cut off table. As soon as the bar of clay has come to rest upon the belt and the plunger begins to move back, the operator cuts the bricks and removes them with brick forks (which are similar in action to ice tongs), lifting four bricks at a time. Two men operate the cut off device and remove the bricks to pallets resting on the car shown in Fig. 8. The car stands on the side track parallel with the cutting table. Each man is provided with a brick fork, therefore only one motion of each is required to clear the cutting table. Two pallets fill the length of the car, and when covered with brick, a second layer is put in position, resting on the first. This process is repeated until the necessary height is attained, usually six layers. The pallets are placed in position by two boys. When filled, the car of green brick is moved by a man to the main track (who returns an empty car), thence transferring the full car to the drying tunnel.

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Fig. 9 shows the side track and transfer car resting on the main track, which supports the drying car shown in Fig. 8, the former illustrating the method of transfer.

The drying tunnel will admit several cars filled with green bricks, which are allowed to remain from twelve to twenty-four hours. When the bricks are dry the loaded car is transferred through the tunnel into the kiln near by, where the bricks are set for burning, which is the second handling after the material has left the clayhouse. The drying tunnels have a clear opening of 3 feet 2 inches x 4 feet, and are about 84 feet long. They are arranged in sections, each section containing from eleven to twenty-five tunnels. This arrangement is made to facilitate communication between the pressroom and the brick kilns, as the latter are located in the rear of the tunnels. Open spaces are left between the sections to permit the passage of carts. The tunnels are heated with

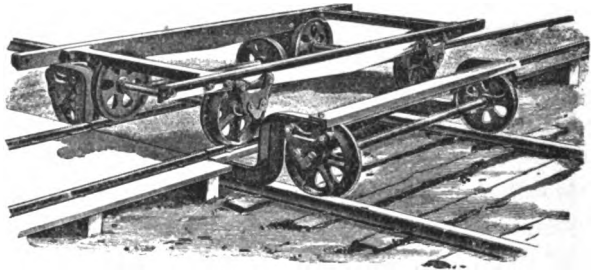

FIG. 9.

steam by means of pipes on the roof, which extend the entire length of a tunnel section, but cover less than half the length of a tunnel. The roof of the tunnels beneath is coated with perforated plate iron, over which the pipes are located and partially covered by a shield.

In order to make the relative location clear, the series of sections comprising the drying tunnels will be represented as occupying a city block, in the form of a rectangle, cut up transversely with alleys between the sections. The tunnel sections fronting on A street, and located on the right side, and the rear or exit of tunnels fronting on B street. On the left side of A street is located the clay manipulator and the brick presses, and the power plant is adjacent for operating them. In A street a track is located from which connection can be easily made to all side tracks leading from the presses and to the drying tunnels as shown in Fig. 9. B street is provided with longitudinal tracks, and tracks connecting with

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e drying tunnels and brick kilns. The exit of the drying tunnels is on e left side of B street, and the entrance to the brick kilns is on the right le of B street. The longitudinal tracks on B street extend to the out- le of the buildings, and connect with a track leading down to a dock on e bay about one-fourth of a mile distant. At this dock the brick can be ipped for transportation by water. A small locomotive is utilized here transfer loaded cars to the dock. The works are also connected by rail th the New Jersey Central Railroad. The inner dimensions of the kiln e 25 x 100 feet, with walls 2 feet in thickness, which are open at the top, signated as up draft kilns. They are provided with furnaces on the tside for firing. This plant at the date of the writer's visit consisted eight kilns, having a capacity of one million bricks each, and four at would contain one-half a million each. The bricks are set in the n forty-four courses high, on edge.

Anthracite coal is used for the drying tunnels, but soft coal for rning. The plant has a capacity for producing more than fifty mil- n bricks per annum. The dimensions of the green bricks are 2½ x 3¾ 8½ inches and of burned bricks 2¼ x 3½ x 8¼ inches, shrinking one- urth an inch in every direction. Both soft and stiff clay bricks are made these works. The brick machines utilized here were supplied by J. . Penfield & Sons, Willoughby, O., to whom the author is indebted for ustrations.

The plant of the Galesburg Pressed Brick and Tile Company is located Galesburg, Ill. The clay bed is located about two miles east of the y, on the side of a ravine through which a brook flows. A test boring ade to a depth of 110 feet failed to reach the base of deposit, the boring ving passed through a vein of coal which is utilized for fuel in burning. he common designation of the grade of clay used is "shale," and the posit belongs to the lower coal formation. The clay bank (covered a few feet only of soil) consists of a top surface layer, gray in color, d 20 feet thick. Directly beneath this is a bed 45 feet thick of blue ay, which is finer in texture than the gray. Both grades of clay are ratified, the layers being from 5 inches to 3 feet in thickness, and roken by vertical seams. Such seams prevail to a greater extent with e gray than the blue.

The machinery is operated by a 60 horse-power Frost engine. The lant consists of two dry pan crushers, one Andrus dry clay press, one cme auger press for stiff clay bricks, which include common and

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paving bricks. The kilns are six in number, and possess capacity to burn seven hundred and seventy thousand bricks. One kiln with a capacity of burning one hundred thousand bricks is an old style Philadelphia kiln, open at top. There are two square kilns of the Eudaly type, with a combined capacity of five hundred and thirty-two thousand bricks; and three round Eudaly kilns, with combined capacity of one hundred and thirty-eight thousand bricks. The stiff clay bricks are hacked to a drying floor, where they remain one day and are then taken to the kiln. The dry pressed brick go direct from the press to the kiln. The clay being very hard, is excavated with dynamite; and when too large in size for crushers, the pieces are broken up in the same way.

No track is utilized for moving the clay cars from the bank to the inclined plane. The dump cars are provided with wheels having broad tires that can be run easily by one man over the hard surface of the clay pit. The clay cars are 2½ x 3½ feet, with the rear end board fitted for self-dumping by a simple device, as the car arrives at the top of the inclined plane. The inclined plane consists of a double track constructed of wood, with a guard strip 16 inches wide in the center between the tracks, and one outside of each exterior rail, which retain the cars in position.

The cars are elevated by means of a winding drum operated from the engine, and deposit the clay into a hopper, which conducts it into the crusher, as shown in plan, Fig. 10. The length of the incline is about 80 feet, and it terminates 30 feet above the clay pit. One man attends two dry pan crushers, removing lumps from the hopper that obstruct free passage to the dry pan. The dry pan crushers (Fig. 11) consist of a heavy upright iron shaft, supported at bottom as shown, and secured in the center of a rigid frame which is about 12 feet wide, with supporting timbers to which the upper bearing of the center shaft is secured. To the upper end of the vertical shaft a beveled wheel 5 feet in diameter is keyed, and over it a driving pulley shaft. The dry pan

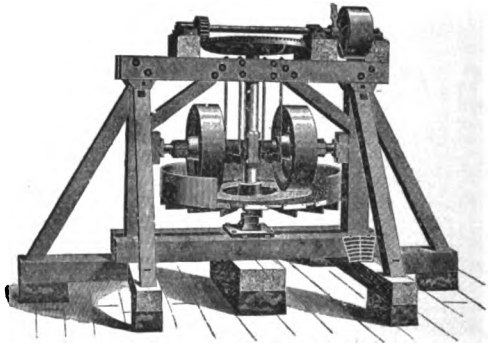

FIG. 11.

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is 8 feet in diameter and secured to a central shaft which is utilized to produce a revolving motion to the pan. Six feet of the center of this pan is solid, around which are set perforated plates 1 foot wide through which the green clay is sifted and falls into a hopper-shaped receptacle below, not shown in the sketch. The cross shaft supporting the rollers is suspended from a cross-head above, thereby removing the entire weight from the pan and central shaft. The space between the pan and the lower face of the rollers is one-fourth of an inch. The pan and crushing rollers are independent. The rollers revolve around a horizontal shaft and not around the pan. This shaft possesses an upward motion permitting large lumps of clay to enter the pan, as the rollers are self-adjusting. When the pan is filled with clay and revolved it produces an opposite motion of the rollers, crushing the clay by a grinding pressure of its weight, which is about 6 000 pounds. Two plows, not shown in the sketch, are bolted to the timber and set in front of the rollers and across the screen plates, in a manner to replace lumps of clay under the rollers again, retaining all lumps in position to be crushed. A vertical rim of boiler plate, 12 to 16 inches wide, retains the clay in the pan. The rollers are 4 feet in diameter and have a 12-inch face. A pair of rollers have the capacity to grind clay to make fifty thousand to sixty thousand common and paving bricks and from twenty-five thousand to thirty thousand paving bricks in ten hours.

Cup elevators convey the ground clay from the receptacle below the dry pan up to hoppers on the second floor, that have the capacity of 4 tons each, from which it descends to a pug mill, which is attended by a boy provided with a hose to add water in sufficient quantity to make the clay powder plastic as it enters the pug mill. This method is the stiff clay process. This pug mill is a trough about 30 inches in diameter and 6 feet long, provided with a central shaft to which the mixing knives are attached. The knives also aid in forcing the clay forward to the opposite end, from whence it drops into the brick press, in which the common and paving bricks are moulded.

A second set of dry pan crushers prepares the clay for the dry press in the same manner as described, and similar carriers transfer the clay to the hoppers and floor above, from which it descends to the dry press without any admixture of water. The "Acme" brick press for treating stiff clay is located directly below the pug mill previously described, permitting clay to drop directly into it through a chute.

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The motion of clay in the pug mill is controlled by means o: placed in a hopper attached to the pug mill, which is simi side of this pivoted to act like a jaw, to force the clay aga: grinding knives in the brick press, and when drawn back to pe: clay from above to pass freely. The Acme brick press (Fig. 2) of a heavy conical-shaped cylinder in which is set a horizontal st provided with mixing knives, and screw propellers on the end to the clay from slipping back toward the hopper. The ribs (Fig. ning around the machine correspond to the threads of a right-han while the auger acts as a left-hand screw. The spiral ribs prevent the clay from turning with the propellers, moving it forward to the orifice, to which a brick die with two openings is attached, through which two continuous slabs of clay are forced out on a cutting table (which moves on anti-friction rollers) with an end motion of about 8 inches. The cutting frame when depressed cuts six bricks, which are then removed to pallets while the frame is returned to again receive slabs of clay. The green bricks are hacked from the press to the drying floor on sprin where they are set on edge and remain one day, then remove in kilns for burning. The capacity of the Acme press is th sand to fifty thousand bricks per day.

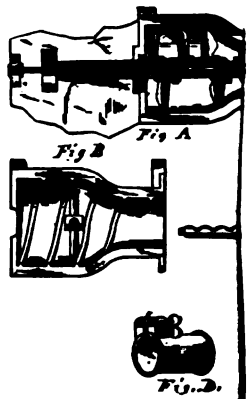

FIG. 12.

The Andrus dry clay brick press, for making dry pressed shown in Fig. 4. This machine is of a double pressure type with the plungers having co-ordinate upward and downward with the pressure strains transmitted through connecting rod upon the cast iron frame, and with the joints and working par the way of damage from grit. The machine exerts a pressure 50 tons on each brick, evenly distributed on both bottom of the brick. The pressure plates are adjustable and can raised or lowered if it is desired to change the thickness of th Clay should be pulverized to the consistency of coarse corn me entering the press. The bricks are delivered automatically on in front of the machine at each operation. Two men provi

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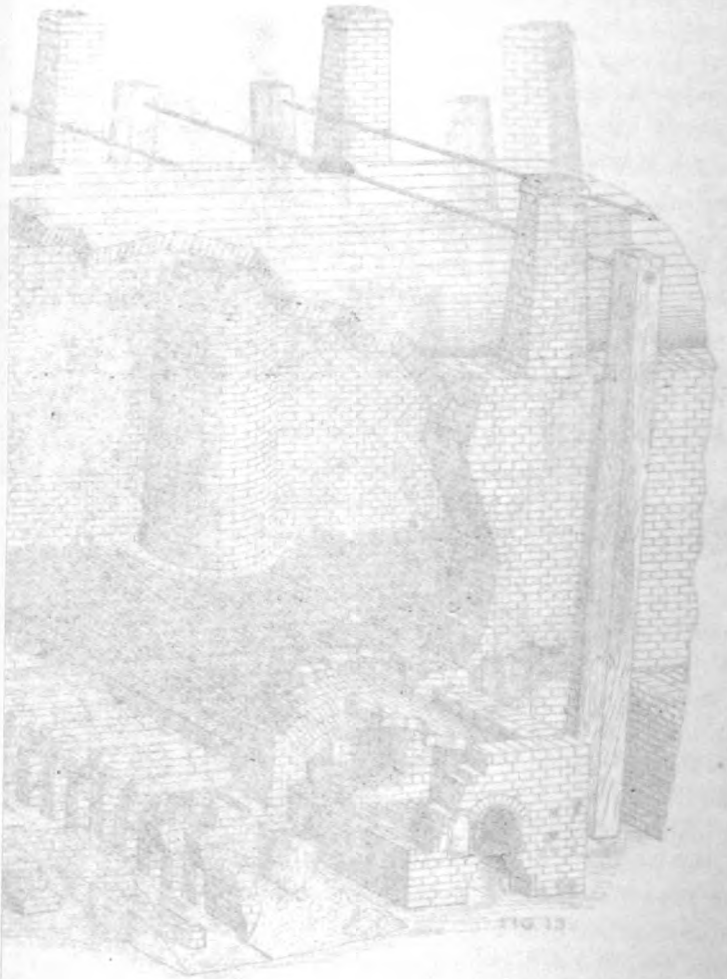
FIG. 13

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suitable trucks will remove to the kiln twenty thousand bricks in ten hours. Six horse-power is required for operating the machine.

Drying Floor.—The drying floor is located in a brick building adjacent to the brick pressroom and is 44 x 98 feet in size (Fig. 10). The drying floor is on the same level as the pressroom, yet the bottoms of the flues are nearly four feet lower. The heating flues are 36 feet long, leaving a space of 7½ feet for feeding the furnaces. There are nineteen flues in the length of the building, connected with five chimneys by means of transverse channels. The heating flues are higher in rear than in front. The entire floor is covered with earth 12 inches thick in front and diminishing to 3 inches in the rear, and this earth surface is paved with one layer of brick laid flatwise. The capacity is thirteen thousand to eighteen thousand bricks in ten hours. The green bricks are hacked on this floor and usually allowed to remain twenty-four hours, and are then transferred to the kiln. Coal mined on the premises 60 feet below the surface of the clay is utilized for fuel. The coal seam is 30 inches thick. Clay deposit is found below the coal to an unknown depth. A platform 9 feet high extends from the coal shaft to the furnace flues of each brick kiln and also in front of the power-house and drying floor, on which loaded coal cars are propelled by hand to supply fuel.

Brick Kilns.—The location of all the kilns utilized for burning brick in this plant is shown on plan (Fig. 10). All kilns except No. 6 are of the Eudaly type. The rectangular form is shown in Fig. 13. The circular kilns are similar in action, but have a central flue not required in the form illustrated, and are provided with iron bands about 4 inches wide to prevent outward expansion from heat, but rectangular kilns have posts and rods for a similar purpose. The round kilns are better adapted to burn brick in small quantities than the rectangular form. The principles of construction are identical, all walls being two feet or more in thickness and about 14 feet high. The kilns are divided into sections in the bottom only. In each of the sections a large flue *F* runs from the walls *S* to side walls, and there connects with chimneys built in the wall of the kiln. Each chimney has a certain part of the kiln to control, and cannot take heat from any other part, except dampers are closed to throw heat into other sections. These sections being located in the bottom of the kiln do not interfere with setting. A space is left over the wall *S*, when bricks are set, to permit heat to pass quickly down to the bottom of the kiln.

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Fig. 14 shows a detail of the furnace. The rear of the furnace is contracted to form a double bell mouthed orifice or throat *M*, flaring at both ends. On each side of the throat *M* are air chambers, *O*, *O*, supplied with air through air flues, *m*, *m*. On each side of the firebox *G* are air chambers, *V*, *V*, supplied with air through flues, *n*, *n*. The air from chambers *V*, *V* and *O*, *O* enters the throat *M* and the firebox through small openings between the ends of the firebrick linings. When the furnace is charged with a fresh supply of fuel the gases set free would, if not prevented, pass out through the kiln unburned, but the currents of air from the air chamber *V*, *V* come in contact with the carbon and hydrogen gases as they rise and combustion begins at once. The light gases not consumed in the furnace *G* come in contact with the bridge wall in the throat *M* in curved lines that first contract and then expand the flame; this increases the draft and at the same time the escaping gases are supplied with oxygen heated to a temperature insuring perfect combustion.

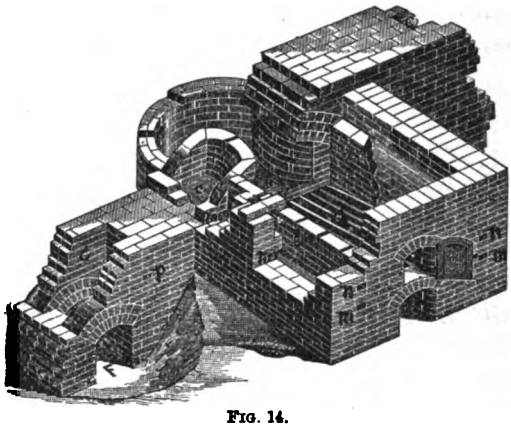

FIG. 14.

The heated air after leaving the furnace flue *M* passes up through an air flue (as shown by arrows in Fig. 10) to the top of the kiln, where it separates, a portion going down through the green bricks to the flues *P* and *F* at the bottom, thence out through the chimneys, while a portion is drawn down through the open space over the wall *S* between the stacks of brick, and distributed directly to the bottom of the kiln. If desired, a portion of the heat from the kiln may be drawn down to the blind flue *F* and allowed to escape from the kiln.

The outer end of the blind flue *F* is closed when burning. The chimney leading from the blind flue can be closed by means of a damper from the outside of the kiln. The center of the kiln being already hot, the heat will continue to pass down the center stack to the bottom, but as the draft is cut off from *F* when the damper is closed, the heat is forced through the openings in the floor to the brick in some other part of the

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kiln to escape. This arrangement of draft and flues permits the burner to distribute the heat and thereby prevent an excess at the top of the kiln. Peep holes are provided in each section to enable the condition of the burn to be ascertained from outside. If heat should be found to be excessive in one section, it can be transferred to another by simply closing the damper in the chimney and stopping the draft. This device provides a method of keeping the heat in different portions of the kiln under proper control and preventing excessive burns. When the kiln is cool enough to admit cool air, the flue *F* and holes in the crown are opened, but the dampers in the chimney are closed. The cool air will then enter the flue *F* and pass up through the bricks and out at top. The burner at this plant is one of the best in the country, and the proprietors after several years' experience with these kilns speak in high terms of its burning qualities.

Dry pressed and common brick made here, shrink in burning about one-half inch. Paving brick shrink in length 1 inch, in width one-half inch, in thickness one-fourth inch.

The following named manufacturers supplied the machinery for this plant, and the author is indebted to them for the illustrations and descriptions: Frost Manufacturing Company, Galesburg, Ill., supplied boiler, engine and dry pan crushers. H. O. Whitney, Keokuk, Iowa, the Andrus dry press. Henry Sheckler & Hover, Bucyrus, O., the Acme press. W. A. Eudaly, Cincinnati, the brick kilns.

Part III.—Brick Pavements.

The impression has been general that brick is too soft and destructible a material to endure the rough usage it would be subjected to when utilized for paving a roadway, but tests in a large number of cities in this country, continuing through a period of more than fifteen years, have demonstrated the fact that when paving bricks are composed of suitable material, properly made, burned and laid, they will make a durable pavement.

Quality of Clay.—The material required to make a good paving brick should be a tough, stiff and tenacious clay, which should be thoroughly tempered and moulded by machinery capable of producing heavy pressure to make the material compact, close in texture and homogeneous.

A machine that will make a good building brick is not necessarily

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adapted to produce a good quality of brick for paving. therefore that only selected clay should be utilized, a ascertained by thorough testing before being accepted; machinery adapted for treating this grade of clay only s The best available information obtained by the writer is suitable for making stiff clay bricks and capable of necessary pressure is suitable to mould paving bricks. E determine that a dry press machine will mould good pave sent it has not been a success, except in one locality, so f the writer. The objection is raised that the great den bricks will have a tendency to retard burning, therefore cost of manufacture, diminishing uniformity of burning a center of the brick imperfect.

It is not essential that the clay bear a very high degr it is essential that its composition be of a nature to retai shape, and support the superincumbent bricks in the k semi-vitreous condition. The more refractory a clay is, t increase the margin between the semi-vitreous condition a of the clay. A clay that will endure heat and vitrify with running down will make a good paving brick. The adm to some shale clays will improve their quality for making

In the plastic clay belt of New Jersey is found a to clay, between the red brick clay and fire clay. Clays most satisfactory paving brick contain very little sand in them. They are generally deep below the surface and shale clays, overlying the coal formations, yet good pavin found in a plastic form. Clays composed of suitabl making pavers are usually very hard and difficult to grin Clay that will effervesce when tested with acid indicates t carbonate of lime, which will prevent vitrifaction; hence clay will be unfit to make pavers.

Quality of Pavers.—A good paver is very dense an a small percentage of water. It should be burned to a ness, and the surface should be vitrified. If water is exclu be also. The clay should be of a quality to permit a hard clay is unfit for pavers. As paving bricks are subject t much greater degree than to compression, they should b and substances that will readily vitrify. Much iron, alu

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should be present, the first and second to produce strength and toughness, and the latter to facilitate vitrifaction. The green bricks should be dried slowly, with an even temperature, thereby facilitating more perfect crystallization. A good paver should be hard but not brittle nor granular, which will cause it to be chipped and broken easily. After receiving a hard burn they should be annealed to make them tough. Dr. I. S. Hopkins, President of the Georgia School of Technology, states that there are some vitrified bricks which are so brittle that when the wheels of loaded vehicles come in contact with them they wear off very rapidly. These bricks are not good pavers. They must combine toughness with vitrifaction. If a brick is thoroughly vitrified the whole body of clay is melted homogeneously into one mass. Toughness means the tempering which is given to the clay while in the firing or burning process, whereby the life, grain and tenacity of the bricks are not destroyed. The fact that a brick is vitrified does not of itself make a good paver, as there are a large number of clays which will burn to a state of vitrifaction, and yet will not make bricks suitable for street paving because they lack the necessary "toughness."

Shape of Pavers.—It has been proposed that the corners of bricks be slightly rounded instead of square, to prevent chipping, as the sharp corners when broken do not chip evenly, and produce a slight roughness in appearance of the surface. A pavement examined by the writer, which had been in use four years, did not indicate anything objectionable in its appearance or condition. A brick with two round corners would be objectionable on account of increased labor in laying to maintain the round corners on the surface; therefore if round corners were adopted, the four corners should be uniform.

Large bricks, 4 x 5 x 14 inches, have been proposed, and their advocates claim that, if perfectly burned, they will resist greater crushing strain and wear longer under abrasion. But the weight of evidence is largely in favor of the dimension of common bricks. The advantages in using the common size are, the ease with which they are made and the readiness with which they can be obtained, since all brickmakers possess moulds for making this size. Also when pavers are required for repairs they can always be found. All soft bricks unfit for paving can be sold for building, hence the danger of poor bricks being supplied will be diminished. Bricks larger in size will be more difficult to manufacture, slow to dry and more expensive to burn. The drying and burning will

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not be as uniform, hence the brick produced will not be proportionately as strong.

A brick slightly warped in burning can be used in the lower course, but only bricks uniform in size and perfect in form and quality should be allowed in the wearing surface. The lower course need not be so hard as those in the surface course.

TEMPERING.—When clay of a plastic nature is refractory it will not easily absorb water in tempering, and if ground before drying, it will be liable to pass through a crusher or rollers in flakes which are not easily broken in the pug mill and therefore may be found in the bricks untempered. If the clay be a shale it is necessary to pass it through a dry pan or other heavy crusher to thoroughly pulverize it, before tempering in the pug mill. When the clay has been well tempered the particles unite closely, and when burned will make stronger pavers, and if broken will present a smooth and glassy fracture. There is a class of clays that will become brittle with much pugging, hence there is a point beyond which they cannot be tempered with safety. They may be dense, but partake more of the quality of glass, breaking easily. The Galesburg clay can be thoroughly vitrified without becoming brittle, and yet be hard enough to resist cutting with a chisel.

VITRIFYING AND ANNEALING.—Experienced manufacturers of paving bricks prefer a kiln in which the heat can be controlled perfectly; therefore permanent kilns only should be used. The burn should be complete when the last fires are built, then the kiln is closed and the heat held from twenty-four to thirty hours to produce a uniform distribution. The cooling of a kiln of brick should then be done by drawing the fires very slowly to toughen or anneal them. Much is claimed for this method, especially for common clays. Some refractory clays vitrify freely by the admixture of an inferior quality of clay, but when used alone become too brittle. Tempering with an inferior grade of clay tends to toughen them. There doubtless may be much good paving material obtained from common clays when the method of treating them is discovered. Some grades of clay, when burned to the vitrifying point, become a solid mass. It is useless to attempt making paving bricks from clays that will not retain their shape under a degree of heat sufficient to vitrify. A hard burned and a vitrified brick differ, as the former may only contain the necessary properties to burn hard, and none to vitrify; then if too hard, it becomes brittle, and chips

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sily under travel. Surface clays consisting of decomposed shale will laze when they contain sufficient iron and silica, but when too large percentage of lime is present no glaze can be obtained. The term trified is sometimes applied to pavers made of fire clay, but fire clay non-fusible, and fusion is the essential element in vitrifaction.

A few manufacturers are salt glazing their paving bricks, but this ethod should be condemned, as a salt glaze adds nothing to the rength of bricks, and affords an opportunity to hide defects by filling l cracks, covering blemishes, and also producing an appearance of a ard burn, when the opposite is the condition. A first-class paving lay will contain all elements necessary to produce a glassy appearance ithout any artificial glaze. The objection will naturally be raised that brick coated with glass will be too brittle, and unable to endure the oncussion incident to heavy traffic; but if the bricks are well annealed fter being vitrified, instead of being brittle they will be toughened, and ndure concussion far better than bricks that are not vitrified. A per-ect paving brick should be hard burned, and then annealed to resist ompression and abrasion, and vitrified to resist abrasion, acids and oisture. Vitrified bricks are proof against caustic soda, one of the ost destructive chemical agents, as demonstrated by recent experi-ents, and are therefore capable of resisting all acids affecting a pave-ent. It is essential that bricks should be impervious to acids as well as o moisture, as the street refuse in cities contains a high percentage of isintegrating elements that often have as much effect in producing dis-ntegration in pavements as either moisture or abrasion; however, all ther conditions being equal, brick will outlast all other paving material, nd is preferable from either an economic, mechanical or hygienic stand-oint. The field for the use of vitrified bricks is not confined to street avements alone, but they can be utilized to advantage for foundations, ewers, culverts, etc.

Tests and Specifications.—A crushing test of paving bricks made t the United State Arsenal, Rock Island, for Mr. William Steyh, City Engineer of Burlington, Ia., developed the following results, six bricks nly being tested, which were selected from those made at three differ-ent places. Spawling occurred at from 30 000 to 40 000 pounds pressure on the whole brick. The crushing strength per square inch varied from 4 890 to 9 214 pounds. The weakest sample, that spawled at 30 000 pounds, had an area of little more than 9 square inches. The entire

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surface contact of the wheels of a loaded wagon would not exceed 6 square inches, and the weight would be distributed over four different bricks; hence the test for crushing strength appears to be worthless, or useful only for comparing the quality of the bricks.

As the cohesive properties of a paver are the first to demand attention, tests should be made for tensile strength. The maximum loss in paving material doubtless comes from abrasion, due to contact of horses' feet and wagon wheels; therefore tests for abrasion are of equal importance to tensile strain. A test for absorption made by Mr. Patton, of the University of Wisconsin, indicates the following average result: Thirty bricks were selected for testing, obtained from six different makers. The bricks were not subjected to any drying operation before testing, but were kept in a warm room for several days previous to testing. The bricks were first weighed and then laid in a tank of water, the weight being taken at intervals of one hour, one day and one week. The following average results were obtained: Absorption of water in one hour, 0.1016 per cent.; one day, 0.3 per cent.; one week, 0.5177 per cent. Condition of surface not given. An approximate test for absorption can quickly be made by setting a brick endwise, one-half its length in water, and capillary attraction will indicate porosity. Tests for tensile strain, abrasion and absorption appear to be of equal value, but an acid test should not be neglected.

The specifications will necessarily be controlled to a great extent by the method of laying. The quality of brick should be clearly defined, in a manner which will enable the bidder to correctly interpret its meaning. A common clause in specifications is, "the bricks must be of the best quality of hard burned brick." A hard burned brick in one locality may be known as soft in another, therefore this is misleading, as it does not clearly specify quality. The following is a preferable form, the writer unknown:

"The paving brick shall consist of the best quality of sound, hard burned paving brick, made and burned specially for street paving purposes; and should stand all reasonable tests as to durability and fitness to which paving material is usually subjected. All bricks shall be square edged and straight, free from cracks and other defects, of a uniform size and of a quality to be approved by the engineer.

"Three or more bricks of the quality proposed to be used in the pavement shall be furnished with each proposal, the bricks to be labeled

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with both the bidder's and maker's name and address, and to be deposited in the office of the Board of Public Works."

A sufficient number of laboratory tests have not been made to establish a standard for quality, but the writer would suggest the following:

First Quality.—The modulus of rupture in compression to be 1 700 pounds per square inch; absorption to be 1.6 per cent.; abrasion under test to be equal to granite.

Second Quality.—Modulus of rupture to be 1 500 pounds per square inch; absorption, 5 per cent.; abrasion not to exceed twice that of granite.

All brick with a variation of more than 15 per cent. below these requirements to be rejected.

An incident occurred at the Galesburg Pressed Brick and Tile Company yard which will show the strength of paving brick. A side track of the Atchison Railroad enters this yard, and several cars standing thereon were loaded with brick. The weight of each car was 32 000 pounds, and the load of bricks 40 000 pounds more, making a total weight of 72 000 pounds for each car. Half a brick was lying on top of one rail as the cars started on a down grade. Two trucks of the first car passed over the brick safely, but as the second car had obtained greater momentum, the first pair of trucks of the second car crushed it.

Methods of Laying.—The tabulated statement showing the detail of methods for laying brick pavements includes all data the writer has been able to obtain as reliable. After excavation has been made for a pavement the ground is generally made compact with rollers weighing about 2 tons, but at Decatur the weight of roller is given at 6 tons. A foundation course of sand or broken stone is spread and compressed by passing the roller over it three times, when the material is made as wet as possible without licking up in rolling, and then rolled until it is thoroughly compressed. The exact form and crown of the finished surface of the pavement is obtained by means of templates for testing the convexity of the surface when grading, and previous to laying the first course of brick. On this foundation surface a layer of brick is laid flatwise, with the longest dimension longitudinal with the roadway. On the surface of this layer sufficient sand is applied to thoroughly fill all joints; then it is rammed or rolled. After rolling, one to two inches of sand is spread to provide a cushion for the finishing layer. On this cushion a layer of brick is set on edge, with the longest dimension across the roadway, which layer is either rolled or rammed.

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METHODS AND COST OF LAYING BRICK PAVEMENTS.

LOCATION.	METHOD OF PREPARING FOUNDATION.						WEARING SURFACE.			COST.		REMARKS.
	Depth of			Brick laid flat lengthwise of street. Broken joints.	Testing.		Depth of Sand.	Brick laid on edge transversely. Joints broken.	Testing.		Per Square Yard.	Price of Brick.	
	Gravel.	Broken Stone.	Sand.		Rolled.	Rammed.			Rammed.	Rolled.			
Galesburg, Ill.	.....	.....	3 inches	do	.....	on 2 inch plank, 10 feet long.	1 inch.	do	do *or*	do	$1 45 to 1 55	$9 00	All labor except curbing.
Quincy, Ill.	4 inches	*or* do	6 inches	do	do	.....	1 inch.	do	.....	do	1 76	7 to 9	Includes grading and curbing.
Bloomington, Ill.	cinders 2 inches	.....	2 inches	do	do	.....	1 inch.	do	.....	do	1 15 to 1 50	10 00	Includes grading and curbing.
Decatur, Ill.	.....	.....	4 inches	do	.....	do	2 inches	do	do *or*	do	1 32, 1 37, 1 40	.....	Includes grading and curbing.
Burlington, Iowa	6 inches	*or* do	6 inches	do	do	*or* do	1 inch.	do	do *or*	do	.....	.....	
Atkinson, Kans.	.....	.....	4 inches	do	do	.....	1 inch.	do	.....	do	.....	.....	
Charleston, W. Va.	.....	.....	3½ in's tarred board 1 inch thick.		.....	.....	1½ in's	Herring bone style.	.....	.....	1 25 to 1 75	6 to 10	
Peoria, Ill.	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	1 42 to 1 50	9 50	All labor except curbing.
Jacksonville, Ill.	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	1 92	.....	
Youngstown, Ohio.	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	1 73	.....	
Allegheny City, Pa.	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	1 25 to 1 75	.....	
Nashville, Tenn.	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	1 80	.....	

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After rolling is completed, any broken bricks found are removed and replaced with whole ones. This is done also in the first layer. In some localities in Ohio a foundation is preferred composed of 8 to 15 inches of broken stone, made compact with rollers weighing from 12 to 16 tons, and upon this is spread 3 inches of sand to act as a cushion.

At Nashville, Tenn., pavements are constructed by first spreading a layer of broken stone on which about 2 inches of sand or gravel is spread. Upon this sand is laid the wearing surface of bricks on edge, only one course of bricks being laid. The bricks are common ones, medium in hardness, but prepared by saturating them with a distillation of coal tar applied very hot. A foundation made of concrete will be more rigid than the methods described, but will necessarily be less elastic. It may, however, render better service under heavy traffic. As concrete has not been utilized except to a limited extent, its effect is not known to the writer. Any pavement, whatever material it may consist of, will be more stable when laid on dry soil, brick being no exception; therefore the soil should be underdrained, if necessary, to produce a condition which will materially aid in preserving a uniform surface.

At Galesburg and Decatur the paving bricks are thrown from wagons when delivered on the line of a work the same as paving stone, and only a few are broken.

Crowns.—Gillespie, in his "Manual of Roads," advocates a cross-section of 1 in 40 to 50, and Gillmore, in "Roads and Street Pavements," a section of 1 in 40. Both authors doubtless had in view a street paved with stone, which would permit a crown of 6 inches in a roadway 40 feet wide, and this is ample to turn all storm water into the gutters. As the surface of a stone pavement is rough compared with the surface of a roadway paved with brick, a crown having the same elevation would be too great for the latter. At Decatur, Ill., a crown of 4 inches is allowed in a roadway 50 feet wide, and 3 inches in a roadway 40 feet wide. At Galesburg, the crown of the first pavement laid was one-third inch per foot, but for recent practice the crown is reduced to one-fourth inch per foot, and the section is a flattened segment. The specification states that "the transverse shape of the finished pavement will be a segment of a circle so large that the center of the street shall be 4 inches higher than the lowest point in the gutter, which shall be 1 foot from the outer edge of the pavement, and this outer edge shall be one

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inch higher than the lowest line of the gutter." Recent practice, where curbing is provided, is to extend the section to the curb line, omitting any gutter depression. It will be noticed that the experience of several years' traffic developed the fact that a crown of 6 inches exceeded the requirements for a pavement of this character; it therefore was reduced to about 4 inches. As the surface of a brick pavement is comparatively smooth and uniform, without the projecting points always present in a stone pavement, the conditions necessitating a high crown are not present; therefore the writer believes that the practice at Decatur is preferable to that at Galesburg. It may be stated that a perfect surface in a pavement cannot be maintained (therefore the crown should be high to insure removal of storm water), which is true if the pavement has a defective foundation and is imperfectly laid and no attention given to keeping it in repair. Any pavement, no matter of what kind, should have constant supervision, and be repaired as soon as possible when any depression appears, and not left until it becomes a nuisance. The writer did not notice any depression in brick pavements examined either at Galesburg or Burlington, Iowa, that perceptibly affected the condition of the surface. If material removed from trenches is replaced in a dry condition, instead of saturated with water, and thoroughly rammed, there would be no complaint of depression caused from the refilling of trenches. The maintenance millennium for a street pavement will doubtless not arrive until the average plumber is only allowed on a street, when traveling at a speed of 8 miles an hour.

In a climate where frequent storms prevail in winter, any pavement is liable to be coated with mud and ice, and a high crown would have a tendency to contract the width of roadway used on account of the slippery condition of the section near the gutter, and so concentrate travel on the flattened portion adjacent to the crown; but a low crown sufficient to remove all storm water would permit the whole width of a paved roadway to be utilized.

Condition of Pavements.—A brick pavement will present a smooth surface over which heavy loads can be drawn without a jolt, consequently the wear of rolling stock will be diminished, and horses be able to draw from one-fourth to one-third larger loads than over a stone pavement. Brick pavements are easily and quickly repaired. If a soft brick is found, it can be easily replaced. They are less expensive than stone, when durability, wear and tear of teams and vehicles, as well as comfort,

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are taken into account. They are as noiseless as a wood pavement, and furnish a sure foothold for horses. The writer has received data regarding this feature in brick pavement from numerous sources where this class of pavement has been in use for many years, and such testimony as to its value is uniform without a single exception, and also that it does not become slippery when wet. Ice does not form as readily as on a stone pavement. The surface of a good paving brick will wear smooth, but does not become slippery. The writer examined a brick pavement in Galesburg which had been in use on a main thoroughfare for four years, subject to heavy traffic, and it remained in first-class condition. The surface of the brick was worn smooth, yet there was no slipping noticed during a visit covering a period of two weeks. The corners were slightly chipped, but not to an extent to affect the pavement. The annexed statement of Prof. Churchill, the City Engineer, relative to its condition is fully confirmed.

Mr. William Steyh, City Engineer of Burlington, Ia., states that if "tamping is done carefully with a rammer weighing 75 to 100 pounds, brought in contact with a plank 2 inches thick, 8 inches wide, and the blows repeated until the rammer rebounds, it gives a satisfactory result, and the bricks will remain in a vertical position, and present an even surface; while, if rolled, they have a tendency to lean in the direction in which the roller is moving, especially if the weight of roller exceeds 5 tons in a width of 4 feet." If a heavy roller has a tendency to incline bricks from a vertical position, a roller weighing only 2 or 3 tons may be preferable. Heavy grades prevail in Burlington, therefore this inclination may be practically due to steep gradients. He recommends that when grades exceed 6 feet in a hundred, a groove three-fourths of an inch in width and about 1 inch deep be made in the bricks, to furnish better foothold for horses.

As the bricks composing the lower course, laid flat, do not require to be as good in quality as the upper or wearing surface, a better selection for both can be made by permitting some rejected brick to be used in the lower course when suitable.

When pavers are the size of common brick, the thickness being narrow and uniform tends to increase the smoothness. The joints between the bricks being one-eighth to one-half an inch, the latter the maximum, little chipping occurs, and when it does the depth is too slight to perceptibly roughen the surface, hence heavy and long continued traffic

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does not produce a rounded surface to each brick as is often seen in a stone block pavement. The bricks are laid as close together as possible, therefore it is necessary to use fine sand to brush into the joints.

Mr. E. R. Chatten, City Engineer of Quincy, Ill., states: "I have but one opinion, and that is that when properly constructed with burned brick, or proper materials, it will prove the coming pavement for the West, and equal to anything but granite for heavy hauling." Mr. A. H. Bell, City Engineer of Bloomington, Ill., writes: "I am very much in favor of brick as a paving material when the proper class of brick can be obtained at reasonable rates and the work is well executed. Brick pavements are giving entire satisfaction in our city, and we are using nothing else. It stands the heavy traffic of our city of 25 000 population very well when the bricks are well selected, and better than Joliet limestone or wooden blocks, which are in use on some of our streets."

The economic problem to be solved in a pavement should include maintenance as well as construction, and maintenance is controlled by the amount of traffic; hence the latter is an important element to be considered in determining the economic value of a pavement.

In addition to the gentlemen whose names are given in this paper, the writer would express his obligation to the following: Prof. R. T. Whitfield, Curator of Geology, American Museum of Natural History, New York; T. S. Randall & Co., Indianapolis, Ind.; Galesburg Pressed Brick and Tile Co.; J. P. Hale, Charleston, W. Va.; E. R. Chatten, City Engineer, Quincy, Ill.; A. H. Bell, City Engineer, Bloomington, Ill.; Prof. George Churchill, City Engineer, Galesburg, Ill.; William Steyh, City Engineer, Burlington, Ia.

APPENDIX.

GALESBURG, Ill.,
September, 1888.

F. A. CALKINS.

DEAR SIR,—Our first piece of brick pavement was put in about four years ago. It was very near our railroad depot and a large lumber-yard, over which the heaviest traffic passes constantly. The wear is very slight, hardly more noticeable to-day than when it had been down only six months. There are, unfortunately, some depressions in the pavement caused by a deficiency in making a good foundation. I have this summer taken out bricks from the center of the street to find how much

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wear could be seen, and have found the bricks of full width, with the single exception of worn or chipped corners, and this only to a limited extent. One may walk for rods over this pavement and find scarcely a broken brick, and usually that one would show from its light color that it should not have been placed there.

We have quite a long section laid three years ago, which is to-day in as good condition as the day it was finished, only that in the center, where horses travel most, there is a slight chipping of the corners, which hardly ever is so great as to reach the middle line of the brick surface. It is a question whether this chipping of the corners is not a good thing, in that it may afford a better hold for horses' feet.

Our soil is a soft prairie loam some 2 feet deep, lying upon a porous yellow clay. It is absolutely essential that the roadbed be made solid and free from any soft places occasioned by excavations new or old, and when the grading has brought the bed to the exact contour of what the pavement is to be when finished, it should be rolled repeatedly with a heavy roller. When the roadbed has been thus prepared, we spread a layer of good sand over the entire surface from curb to curb to the thickness of 3 inches, and in my opinion the sand should not be dry, but should be moist enough not to run; upon this sand layer a pavement of bricks on their largest surface and their lengths longitudinal with the street is laid, and then the pavement is rolled with a roller heavy enough to settle the bricks thoroughly into the sand beneath them. This lower layer of bricks ought to be about as good as the surface bricks, though many allow a grade slightly inferior to be used. The surface bricks are laid upon their edges, their length transversely of the street; they should be laid as compactly as possible, care being taken to break joints and to keep the courses in nearly a true line across the street. Our bricks are not perfect enough in shape to prevent a sufficiency of sand from entering the crevices to make a solid bed. The pavement thus formed is again rolled, and then dressed with a layer of dry sharp sand, which is worked into any and all crevices by sweeping.

We have used the ordinary yellow clay for bricks, and when these have been well made and properly burned they seem to do well, but they are not equal to the bricks made from a clay we have which looks when in its natural bed like layers of stone. We demand that the bricks be burned to the point of vitrifaction, but prefer that they shall not be much vitrified. Our bricks when thus burned, when broken into small pieces are so hard that they will scratch glass about as a diamond will. So you will see that we have a superior clay for bricks. These bricks are very compact and heavy and take almost no water, and so when wet from rain are not more slippery than other pavements.

We have no stone pavements with which to compare the brick, but we have some stone crossings, and I have never heard any objection to the brick pavement from its slipperiness when wetted by rain or when

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frozen in winter. We occasionally have sleet storms here when everything is slippery, but I have never noticed that the brick pavement differs from other surfaces under similar conditions. The street in front of my house has been paved three years, and I have never noticed any difficulty from the surface becoming icy so as to interfere with traffic, and the travel is as heavy and constant as upon any street in the city—a place of about 15 000 inhabitants.

We have had no occasion as yet to make repairs, except when, after a time, a depression in the pavement shows that some old ditch or trench for water pipe, gas pipe or sewer had not been well tamped; such a depression is very easily repaired. From present appearance our brick pavements will not need repairs for a dozen years, if for a score of years. Still, our traffic is not to be compared with that of large cities, where trucks and drays are loaded with many tons.

Our curbing is made in several ways; sometimes ordinary stone curb, sometimes a strip of scantling to retain the bricks in place until the dirt has been thoroughly solidified at the pavement edge, sometimes by bricks 12 x 4 x 5 inches, laid so as to form a gutter, with the blocks sloping from the edge of the gutter up to the grass area and to the ordinary brick pavement. This is for residence property only, where grass areas are made between the street roadway and the sidewalk.

I think I may safely say that our business men unanimously agree that for all our Western cities, where stone and cedar blocks are not abundant, the brick pavement is the thing; and I may say further that the opinion is almost unanimous, that if stone and cedar blocks were abundant, the brick pavement would still be the thing, because of its merits when compared with other materials.

Yours truly,

GEO. CHURCHILL,
City Engineer.

F. A. CALKINS, C. E.,
Ogden, Utah.

DEAR SIR,—From my present experience with brick pavements, I would lay down the following rules for the construction of the same: In the first place, the clay of which the bricks are made should be compact and solid, having enough iron in it to make the bricks tough in addition to being hard. The clay should be pulverized to a fine powder, so that it will make a homogeneous mass when being moulded into bricks. I do not think that pure fire clay is a good material for paving brick, as it does not fuse or melt readily, and for this reason does not become homogeneous, which is the main point of a suitable paving brick. Having secured a first-class brick, we will now consider the different methods of constructing the pavements. According to various published reports, Charleston, W. Va., was the first or one among the first cities in this

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country to experiment with brick pavements. This pavement was laid under the following specifications: After grading the street to the proper contour, about 3 inches of sand was spread over the surface, on which were laid inch boards lengthwise with the street, which again were covered with about 1 inch of sand, after which the bricks were laid herringbone fashion. This method has some good points, but also serious objections. Its good points are an even and fairly solid foundation, not too rigid, but elastic enough to make it pleasant to ride or drive over. Its objections are the perishable nature of the boards, and the inconvenience and greater expense of digging up the street for necessary water, gas, steam heating and sewer repairs. It also involves a greater expense in laying the bricks, requiring better skill and more time than if laid at right angles with the street. To use a concrete foundation adds to the expense and is open to the same objection as the board foundation; as regards the necessary repairs of the underground systems, therefore, I should not adopt this method unless there was no other way of securing an unyielding foundation.

As far as I am able to judge at the present time, the specifications used in western Illinois and Iowa have advantages over those just described. In the first place, the use of the lower course of brick instead of boards insures a better quality of bricks for the top course. Experience teaches us, that no matter how carefully the bricks are selected in the yards, before delivery to the work there is always a large percentage of the selected bricks which have to be rejected as top brick, but can be used to advantage as a bottom or lower course brick. If these bricks could not be used in this way, there would be a strong desire on the part of the contractor to put them in, when not too closely watched; but aside from this, the lower course of bricks is far superior to the boards, as the bricks do not decay, while the boards in the course of time will. When compelled to dig into the street for repairs of the underground systems, you do not have to take up a whole square equal to the length of the boards, but only a trench large enough to work in, and should the earth in the trench settle afterward, all that is required is to remove the brick, refill with sand and replace the pavement; this can be done in a very short time at a small expense, which should be borne by the party who caused the digging up of the street. In 1888, when Jefferson street, in this city, was paved, the Water Company put in lead pipe connections at every 20 feet; they put all the earth back in the trenches, rammed them as well as this class of work is generally done; but nevertheless about half of the trenches sunk down during the following winter and spring and had to be raised. This was done by the Water Company and paving contractor jointly, the expense amounting to about fifty cents per trench for both sand and labor, since which time the street has shown no more sinks. In some cities, notably Nashville, Tenn., they have been creosoting the brick before putting them in the pavement, to

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make them impervious to water. This would perhaps be well enough, if a paving brick had to contend with water only and not with traffic. If a brick will absorb an appreciable amount of fluid it is not fit for paving purposes, hence the expense of making a soft or porous brick suitable for paving purposes will prove a bad investment. As to filling the joints with pitch or paving cement, to make the pavement water tight, it is a very good feature; but I doubt whether the advantage sought for, equals the extra expense of the first as well as subsequent costs in taking up and relaying the pavement, where necessary repairs on the underground system have to be made. The joints on a brick pavement are so small that it takes rather fine sand to run into them, and after being thoroughly filled with sand, very little moisture will find its way to the foundation. You can take up a section of the pavement after it has been down about six months, and examine the soil under the foundation, and you will find the same perfectly dry even at places that have been soft and muddy before the pavement was put down. The advantages of brick pavements over all others are, cleanliness, moderate first cost, easy and cheap repairs, healthfulness, comfort and durability, always provided you have the proper material. There are lots of brick pavements laid now which will not last very long on account of using an inferior article. Yours truly,

WILLIAM STEYH, C. E.

DISCUSSION.

CALVIN TOMKINS, Assoc. Am. Soc. C. E.—Some time ago I read a paper before the Society upon this subject, treating especially of the methods pursued in the manufacture of brick in the vicinity of New York City. The paper to-night describes methods pursued in the western part of the country with much success. Regarding the relative merits of both methods I would say that those described by Mr. Calkins require more capital and also greater intelligence to operate satisfactorily. Frequent attempts have been made about New York City to manufacture brick as described, but with indifferent success. In the more primitive process as conducted about New York City, the clay and sand are shoveled in approximately correct proportions directly into the brick machine, where they are subjected to pugging, and are then pressed into individual moulds from which they are dumped on the surface of the yard and exposed to the drying action of the sun and wind. When sufficiently stiff to allow handling the brick are hacked up in rows, and after remaining in this condition for a few days are set in the kiln to be burnt, the kiln itself being built up with the bricks that are to be burnt, and plastered on the outside with clay. Regarding the drying of brick artificially, it is my opinion that in this locality not much could be saved by adopting that process, as the loss due to storms and

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interruption of labor amounts to less than the increased expenditure for plant, attendance and fuel under the method described by Mr. Calkins.

Regarding the tests which Mr. Calkins suggests could be applied to bricks for paving purposes, I would say that I hardly think that we have as yet arrived at a point in this new use of brick to warrant us in placing much dependence upon any tests other than those resulting from actual use. The very different wearing properties of bricks, which in all respects appear much the same in quality, would indicate to my mind that the only safe test at present is the test of experience. Later on, I think, we may be in possession of more data and a wider knowledge on which to base practical tests for this use of brick.

S. WHINERY, M. Am. Soc. C. E.—A great deal has been said and written in the last few years on the subject of brick paving for streets. And yet it is a subject about which there is very little information of a reliable character from the standpoint of the Engineer. Many of the writers on the subject seem to have been actuated by enthusiasm and a brilliant imagination rather than by the cool teachings of experience. Even the writer of this paper is not free from the extravagant statements that have been so plentifully showered upon the public; as witness his assertion, that "All other conditions being equal, brick will outlast all other paving material, and is preferable from either an economic, mechanical or hygienic standpoint." On the other hand, some of the writers who have decried brick as a paving material, have not adhered closely to the facts, or have based their arguments on the failure of the earlier pavements constructed with hard burned common brick, overlooking the fact that the vitrified paving brick of to-day is as different from its predecessor, as granite is from soft limestone.

The one thing most needed in order to discuss brick pavements fairly and intelligently, is a respectable array of facts gathered from experience and scientific observation. But as the modern vitrified brick pavement has only been in existence for a few years, there has not been time for the necessary facts to be ascertained and accumulated. It may, therefore, be truly said that these pavements are, at the present time, in the condition of interesting and promising experiments. It has been sufficiently proven that the use of hard burned common brick, as a material for street pavements on streets having any considerable amount of travel, will result in failure, and we can safely exclude them from further consideration. The vitrified or semi-vitrified brick made from selected clay of a peculiar quality, carefully prepared and burned, is an entirely different thing, having little in common with the ordinary brick except in name. Considerable areas of pavement made from these vitrified bricks have been laid in many cities of the smaller class, or on suburban streets around the larger cities, but they have not been in use long enough to demonstrate their measure of durability, even under the comparatively light travel to which they have been subjected. A number of such pavements have already failed, but fairness requires the

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statement that in some cases, at least, the failure was due to faulty construction rather than to the inability of the brick to sustain the travel to which it was subjected.

In judging of the relative durability of different pavements, it must be kept in mind that the durability of any pavement, the material of which is not subject to natural decay, is measured by the amount of travel it will carry before it is worn out. In other words, it is a question of tons and not of years. The statement that a brick pavement has been in use for a given number of years on a back street in a small city, and is still in good condition, is, therefore, no evidence of the practical durability of the pavement. It is very unfortunate that we have so little data relating to the tonnage of city streets. Captain Green did some valuable work in this direction, the results of which were published in a paper read before the Society some years since. The company with which the writer is connected has for some years past given a good deal of attention to this subject, and has had taken censuses of travel on a large number of streets paved with various kinds of material, in different cities. It is hoped in this way to determine the average life of the various kinds of street pavement, measured in tons of travel per square foot of surface. Until some such standard is adopted, we cannot arrive at just conclusions as to the relative durability of the various kinds of pavement in use, and must expect the confusion of evidence that now exists on the subject.

In reference to the endurance of brick pavements under travel, we have a large amount of fragmentary and conflicting testimony, most of which is entirely without value, because no reliable standard of comparison with other pavements has been adopted. It is not, perhaps, unnatural that the citizens and officials of cities having populations of from 10 000 to 30 000, should speak of the travel on their principal streets as excessively heavy; or that they should believe that any pavement that has stood up under such travel for a few years, has fairly earned its title to great durability. The reputation for durability of brick pavements rests largely on experience derived from its use on streets of the character named above, as the following table will show. It embraces some of the best known brick pavements, about which much has been said and written, and is made from an actual counting of the travel on each street, upon the same basis:

Name of Street.	Kind of pavement.	Tons carried per square foot per day.
Chapline, Wheeling, W. Va.	Brick.	23
Sixteenth, " "	"	18
Franklin, Peoria, Ill.	"	39
Second, Cincinnati, O.	Granite.	196
Race, "	Asphalt.	203
Main, "	"	103
Pennsylvania Ave., Washington	"	65
State, Columbus, O.	Brick.	14
Fourth, "	"	23

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A glance at this table shows how insignificant is the so-called heavy ravel on these brick streets, as compared with that on the principal reets of a large city, where the pavements are subjected to more tons f travel in one year than the brick streets carry in from five to ten years. o far as the writer is informed, brick pavement has not been tried, ex-ept in small sample areas, like those referred to below, on streets having high as 100 tons of travel per square foot per day. In the few in-ances where it has been laid in these small sample areas on such reets, it has uniformly failed. Such a sample was constructed on Pearl reet, between Race and Elm, in 1885. The best West Virginia paving rick was used, and they were laid on a broken stone and gravel founda-on 10 inches in depth. As it was put down as a sample by the con-actor, upon condition that if it did not prove durable it was not to be aid for, it is to be presumed that both the material and workmanship ere of the best. Under the moderately heavy travel to which the street subjected, the pavement failed in less than two years, and was replaced y granite. In 1886, the intersection of Washington and Dearborn reets, Chicago, was paved with "Hayden blocks," put down as a sam-le to prove their capacity to endure heavy travel. In about two years was entirely worn out, and was taken up and replaced by other mate-al. It is proper to say that the Hayden block is believed to have no uperior among the many kinds of vitrified paving brick now upon the arket. In 1887 a sample of brick pavement was laid on Winter street, Boston, Mass. In less than three years it became worn out and was emoved. Other facts of the same character might be given, but it eems unnecessary to extend the list.

When a brick pavement shall have endured for five years on a street aving an average travel of 200 tons per square foot per day, there will e some justification for the oft repeated claim that brick pavements are s durable as are those of granite or sheet asphalt. At present there is o substantial basis for such a claim. A paragraph in Mr. Calkins' paper lustrates admirably the character of much of the literature of the sub-ect of brick pavements. He states that, "At Nashville, Tenn., pave-ients are constructed by first spreading a layer of broken stone on hich about 2 inches of sand or gravel is spread. Upon this sand is laid he wearing surface of brick on edge, only one course of brick being laid. The bricks are common ones, medium in hardness, but prepared by satu-ating them with a distillation of coal tar applied very hot." From this he reader would fairly infer that the character of pavement described as a standard one in that city, and presumably used to a considerable xtent. The facts are, that in 1885 a pavement of the character described as laid in the intersection of Union and College streets, and was thought o give promise of great success. Two years afterward, two blocks on Union street were paved on the same plan, but, as it soon appeared, in n inferior manner. Both pieces of pavement were quickly worn out and

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removed, the first named being taken up in 1888, and the second in 1890; since which time no more pavement of that kind has been laid in the city. Yet the pavement still lives and is doing duty in the literature of brick pavements. While it is preposterous, in the light of present experience, to place brick in the front rank of paving materials as regards durability and other desirable qualities, there can be no doubt that a pavement made with the best quality of vitrified paving brick, laid in a proper manner on a concrete foundation, with the joints between the bricks properly filled with pitch or other water-tight cementing material, will prove reasonably durable and satisfactory on streets having a volume of travel not exceeding, say, 25 tons per square foot per day. It is to be observed, however, that a brick pavement constructed in the manner just stated, is not very much less expensive than one of other materials constructed in like good manner.

In regard to tests for paving brick, I agree with the writer of the paper, that the present methods are not entirely satisfactory. A street pavement is subjected to destructive action by forces which combine the effects of crushing, abrasion and percussion. Of these three, percussion is undoubtedly the most destructive element. Compared ton with ton, the writer is satisfied that the destructive effect of horses' feet on any pavement is at least three times as great as that due to the action of the wheels of vehicles. If this be true it must be at once recognized that a test for resistance to percussion is of the greatest importance; but so far as the published tests show, this element has been neglected, except in so far as the usual "tumbling" test embraces it. In this connection it is somewhat singular that both the English engineers and Captain Green, in their censuses of street travel, excluded the weight of horses in computing the tonnage of travel.

T. C. Clarke, M. Am. Soc. C. E.—I am pleased to hear that Mr. Whinery confirms my own idea—that the wear of brick pavements comes from the chipping of the edges of the bricks.

The proper test would be to strike such bricks as the inspector would select on the edges and corners with a light steel hammer. They should be able to bear the same blow as a granite block.

Edward P. North, M. Am. Soc. C. E.—Mr. Clarke's proposition for testing paving materials by striking them with an edged tool, seems to me an admissible one, particularly when applied to granite, sandstone and brick, as it is nearer the test given to those materials in every day wear than many now proposed. All blocks laid in pavements are injured by the percussion of the horses' hoofs and of the wheels when falling, from the roughness of the pavement, and by the grinding of the heavily loaded wheels. Both of these tend to cleave the blocks; a tendency greatly increased by the unnecessary widths of the joints allowed in this country. Mr. Clark's proposed test would enable an engineer to judge quickly and easily of the cleavage resisting qualities of the stone or brick offered.

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Mr. Whinery's mode of keeping accounts of traffic may not be quite as good, as he would only count the collars, *i. e.*, the number of animals employed in draft; apparently thinking that a rapidly moving, lightly loaded vehicle is as destructive to a road surface as a heavy load moving slowly. In the case of the Macadam roads this is undoubtedly correct, as the springing of light wheels is effective in dislodging stones.

The value of hard paving brick as a substitute for stone has not been noticed. In many instances they might make an economical and efficient substitute when either concrete or the stone of this country is too friable for the wear they are likely to meet. At the Manchester Ship Canal the dock walls were of concrete for about 25 feet, or to the water line, where a band of granite about 8 inches deep was laid, projecting about 4 inches from the face. From this fillet the concrete was faced up to the granite coping with Staffordshire "blue" bricks. From breaking one of these I doubt if they are as strong and tough as our paving bricks. Mr. Knott, the manager for the contractor, told me they were abundantly strong and tough for the position they occupied.

S. C. THOMPSON, M. Am. Soc. C. E.—I have had considerable experience with brick in sewer work. On one contract, in the Twenty-third Ward, I was obliged to condemn large quantities of brick on account of the impossibility of cutting them either across or splitting. The bricks were tough and of good shape and hardness, but appeared to have had the center forced from the machine in advance of the sides, so that when an attempt was made to cut them they would break like an inverted U. These bricks were made in New Jersey, and were of a special brand. This brand was machine made, and appeared to have been made from clay that was not uniformly tempered. Another brand made by the same company, said to contain 25 per cent. of sand, did not exhibit these peculiarities, but was easily cut in any direction.

ALFRED FRANCIS SEARS, M. Am. Soc. C. E.—This paper of Mr. Calkins treats very fully of the modern methods of manufacturing bricks, of the improvement in their quality and the increased diversity of uses to which they are now applied. He has touched briefly on the ancient history of the brick, which he finds to have existed, like the craft of the smith,

> "Since the birth of time,
> Throughout all ages and nations."

It seems to me the time is opportune, as well as the occasion, for placing on record a historical fact or two concerning not so much the manufacture as the use of brick among a people whose civilization was destroyed by the ruthless conquest of Pizarro. Mr. Calkins' paper mentions Peru as one of the countries making and using brick in ancient times; how ancient we are unable to say, but certainly before the advent of the Incas, for we find among the ruins of those, who recognized the God Pachacamac as the supreme Deity in the pre-

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Inca days, large towns surrounded by walls of unburned brick of symmetrical figure and uniform size, and built up in houses of the same material. Mr. Squire's last volume on Peru gives some interesting, and, as I can testify, very accurate sketches of these walls.

I propose, however, to confine what I have to say to the construction of an important historical road built by the Incas for the comfort of their journeys from the mountains, where they had their capital, to the sea-coast near the mouth of the Santa River, which was, I believe, the northern border of their conquests on the coast, and was a favorite place for the royal baths in the warm seasons. You have no difficulty in tracing this road in some parts of the Valley of Samanco and along the plains of Chimbote to the pavilion at the river. It has been described by some writers, and I believe Squire is one of them, as a "double road," *i. e.*, two roads side by side of unequal widths, and both walled in by adobes. This, however, is a mistake, as several members of the Society can testify. There are really three roads running side by side, of which the middle one seems to have been the principal and is nearly 100 feet wide, the edges of which are marked by walls of unburned (adobe) brick, built to a height, so far as one may judge from the ruins, of 3 or 4 feet, and 1½ to 2 feet thick. This road was paved with adobes of similar form and dimensions, and was flanked on either side by an unpaved road of about half its width, which was also protected from the open pampa by a wall similar to those already mentioned. The pavement in the lower pampa has been generally worn away, but up in the Samanco Valley I found considerable sections in quite perfect condition. This is accounted for by the use made of the lower road for the modern passage of cargo mules, horses and donkeys, while up in the valley it seemed essential to select easier grades for these animals than the Inca road afforded, which has thus escaped destruction. I suppose this middle road to have been for the exclusive use of the Inca's palanquin and his suite, and that the peons who carried him and his nobles were barefooted or wore a simple undressed sandal. The llama of the mountains evidently did not travel this road. The flanking roads are generally reputed by oral tradition to have been used by the armed body guard attending the Inca monarch in his journeys.

I wish to especially invite your attention, as constructing engineers, to a point that was to me the most interesting in these and other similar walls left by the Incas and their predecessors, and that is the fact, that although the brick were made of uniform dimensions, being about 18x 10x2 inches, and laid up carefully with reference to horizontal and vertical lines on faces and quoins, I have never been able to detect a system of breaking joints. Of course, by the accidents of laying, joints were often broken, but you found nothing like a system as it is now known. You will often see joints running from the bottom to the top of the wall and never an attempt at a coping. But after the wall was built the faces

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d top were covered smoothly with a stucco of mud. Generally the urses were continuous in horizontal lines, and laid in about 2 inches mud or wet adobe. At the coast end of these roads there are some teresting works. First, there is a ruin testifying to the former existce of a small town, built, as all the ancient Peruvian houses were, on nd hills or other eminences too high to be reached by the irrigation sters, which gave value among that laborious people to every square rd of land accessible to their vivifying influence. Then, on a lofty ll close at hand, the topmost peak is covered by an elaborate fortificaon of dry wall in stone and adobe; and third, more interesting than , is the ruin of what was evidently a pavilion, and contains the only mains of round columns I have ever seen or read or heard of in Peru. It is a remarkable fact that all their columns were either pilasters to ttress walls, or if detached were of rectangular section. But here is adobe platform nearly or quite 200 feet long and 25 feet wide. Along ch side at the uniform distance of perhaps 15 feet are the bases, I may ther say the stumps, of what were once round columns. These mps are weather worn; rather by the wind than any other element, as in is known in that region only once in seven years. They now show ottom diameter of less than 18 inches and in a height of 3 feet end in lunt point; generally they are not more than 1 or 2 feet high. But at interested me was the fact of their being built up of adobe bricks d like the walls on beds of mud. You detect this construction by eaking them off and thus exposing the interior, for the effect of eather wear has been to so dress off the outside as to have them smooth d apparently homogeneous or monolithic, if I may say that of asonry in adobe. The Incas, I judge from these columns, must have d a method of moulding round brick.

While these statements are of no practical use in the modern manucture of brick, I trust to their historic value and interest, to justify e in laying them before you and asking for them a place in the archives this association of constructors.

R. H. THURSTON, M. Am. Soc. C. E.—I have read Mr. Calkins' paper ith much pleasure, and am particularly interested in what he has to y about the use of brick for pavement. It would seem to be important at some further experience be had in the larger cities, under the avier traffic that is usually met with there. I was in Rochester a few ys ago and observed that East Avenue, one of the principal and finest venues in that city, had been laid with brick, while the principal porn of the city, where the work was not very heavy usually, however, d been laid with asphalt. I am told that the asphalt interest is strong ere, while that promoting the brick is weak; but I presume it will be sy to get the facts relating to both, after a little while, when the rvice shall have revealed their wearing power as compared with the ore common pavements. A careful inspection of the two pavements

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gave me a very favorable impression of both for light traffic; but I could form no idea of their actual or relative value under heavy loads and a dense traffic. I hear excellent reports of the brick pavement in Western cities; but I have received, and get from Mr. Calkins' paper, no very definite quantitative measure of the value or endurance of the material, and no knowledge at all of the precise qualities, as to strength and toughness, which make the best brick for this work.

This seems to me to be the essential information. No engineer can intelligently advise for or against any given manufacture of brick until he can have a definite specification for the quality that is desirable. Presumably a brick may be burned so hard as to be too brittle for the purpose; and, on the other hand, it may be too soft to have any commercial value at all for this use. Just what are the best combinations of strength and "*resilience*" seems to be still unknown. This is the very first thing for the makers to ascertain, and to present to the engineer, before offering brick for paving purposes. I imagine it will be found that some clays are capable of making admirable paving brick, while others, excellent for building, may have no value at all and may be entirely incapable of exhibiting that wearing power which is required. It may fairly be presumed also, that a brick which may prove best for a country village may be entirely unsatisfactory in a large city or under very heavy traffic. The figures given on page 390 of the paper are good so far as they go; but we need vastly more. The compression specification proposed seems to me to admit so weak a material as to make it useless for the purposes of a contract. I think it will be found that, at many kilns, the weakest "sammel" brick turned out would pass that test, so far as strength is concerned. The absorption test may prove all sufficient and the abrasion test seems to me extraordinarily high and inconsistent with the compression test. It would seem to me that a double test, by crushing under pressure in the testing machine, and crushing by the blow of a stated weight and height of drop, would cover the requirement. The former might, it would seem, be easily established, the latter would require careful experimental investigation to get the proper figures of weight and height.

Modern machine-made brick are, I think, usually much stronger than the hand-made brick of years ago, when the specifications assumed that their maximum strength would range from 3 000 to 5 000 pounds per square inch with the best makes. The latest figures that I have obtained for the best makes have been astounding, when compared with these. Years ago, I tested a brick for Mr. Haswell on which we were able to place about 10 000 pounds per square inch, and I have since tested still stronger specimens. We have recently tested brick, made near Ithaca, New York, of two varieties; of which the common samples bore from 8 000 to 11 000, and the "repressed," about 15 000 pounds per square inch before final crushing. They cracked at from 3 000 to 5 000,

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splintered at from 6 000 to 8 500, in the first class, and the two critical periods were reached with the "repressed" article at 12 230, and 14 800 pounds, respectively. They weighed about 125 pounds per foot, the repressed running up to 137. They are usually of a reddish cream color, and very smooth and regular in form.

But this is insignificant beside figures recently given me by a maker of terra cotta in Central New York, who has shown me samples which have stood, in 2-inch cubes, about 40 000 pounds per square inch. I imagine these are ideal materials for paving, and there is no reason why we may not expect to find at various points about the country, equally good clays. The costs of manufacture are not, in any important degree, greater with good than with poor clays, and it should be possible to make brick of these clays possessing enormously high powers of resistance to dead loads and to the blows of heavy traffic.

William Steyh, Burlington, Ia.—The tests for crushing strength of paving brick, referred to by Mr. Calkins, were made at my request, at the United States Arsenal, Rock Island, mostly on account of determining their resistance to traffic, or rather to see what weight would be required to crush them, as it was claimed by some people that no brick could be burned hard enough to prevent its crushing under a heavy load. The bricks in question were tested on end, by a misunderstanding, instead of on their edges as they are laid in the pavement, and therefore did not show their true crushing strength by at least one-half; however, it was shown conclusively that it would require an enormously heavy load to spawl or crack them. I agree with Mr. Calkins as to this test being insufficient to determine the necessary qualities of a good paving brick, and that tests for abrasion, absorption and tensile strength should be made, as well as an acid test; but the facilities for such tests are rather scarce in most localities, and even where samples have been subjected to all these tests, it must be remembered, that the samples are generally selected with care, and that their qualities are in the majority of cases, above the average. It therefore depends very largely on the ability, good judgment and honesty of the inspector, whether the specifications are complied with or not. Any one who has had practical experience in this class of pavements will agree with me, that it is not so very easy to distinguish a good brick from a poor one, as an overburned brick will appear all right outside, while it is brittle and porous inside, and consequently worthless as a paving brick. On the other hand, a well burned and tough brick may appear soft, on account of its pale color, and probably be rejected, although it might stand all the tests quoted above. As it is out of the question to subject all the bricks, or even a small portion of them to the various tests, some way should be found by which an inspector may be guided to detect the poor ones. This can readily be done in the following manner:

First, have the bricks dumped or thrown from the wagon when

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hauling them to the line of work. Second, if a brick should be rather pale in color, or not as heavy as the average (which is the case with an overburned brick), drop it flatwise on another brick set on edge, from a height of 4½ to 5 feet. Third, immerse it in water and notice if air bubbles appear on the surface, if such is the case the brick is porous, and should it absorb an appreciable amount of water it should be rejected. Any brick that will stand the above simple tests is acceptable, and a pavement laid with such material will stand the heaviest traffic if supported by a suitable foundation.

At the time I wrote to Mr. Calkins about the requisite qualities of clay for paving brick I had no knowledge of the true composition of the clay used in the manufacture of paving bricks for use in this city, and on a superficial examination of the same was led to the belief that it contained iron in sufficient quantities to make the brick tough. This conclusion was erroneous in so far as it is not iron but alumina which produces this result. The following is an analysis, made by students of the State University at Iowa City, Iowa, under the personal supervision of C. D. Jameson, Professor of Engineering:

Silica	77.40
Alumina	11.74
Ferric Oxide	3.29
Potash	3.76
Magnesia	1.91
Lime	1.60
Soda	0.47
Mn. (trace with Fe O.)	
	100.17

LEWIS KINGMAN, M. Am. Soc. C. E.—I have read carefully Mr. Calkins' paper on "Brick and Brick Pavement." There are so many methods, machines and kinds of material that it is not strange that the bricks made, vary greatly as to quality and finish. It is true that great improvement has been made in the methods and machines used during the last seven years. It is also true that as the uses of brick have been widely extended, people buying them have been educated and taught by experience the difference between good and bad brick, and now demand better results than the present methods and machines give them. So long as there is a demand for a poor quality, so long will careless, indifferent manufacture continue.

Here, in Topeka, the making of vitrified brick for street paving was commenced in the fall of 1889. A Penfield plunger, with a capacity of 40 000 a day, and five Eudaly down draft kilns with steam dry houses were set up. They have made 7 540 000, which have been accepted and laid in the pavement of our streets, under my personal direction; during this time, about one-half of the brick delivered for paving have been re-

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jected. The plant was supposed to have a capacity of 40 000 bricks in twenty-four hours, but it has not been worked up to more than 25 000 or 30 000, with the average somewhat below these figures, and then one-half of the product was not fit to be used for the purpose intended. The machine broke down more frequently than was expected. Contracts for paving were delayed more than a year on account of defects and weakness of the machinery and the inability of the company to make good brick. The machinery is now worn out; the rollers are concave, so that the material is not ground fine, and the company have had it all taken out, and propose to try another Penfield machine of the auger style with dry pan crusher.

It is not very encouraging for a paving brick company to make brick and have one-half of them unfit for use. Of those made here during the last two years probably 5 per cent. were rejected and broken in two on account of a core or air hole, due to the faulty plunger movement of the Penfield machine. Some of the cores do not appear on the outside, and doubtless many of them have been received and put into the pavement. The brick made by the auger machines do not have this defect. A small per cent. of the brick have been rejected on account of their becoming warped or twisted after leaving the machine—some few more due to the inattention of the cutter leaving a broken end; but the principal cause for rejection is that they have not been properly burned.

The Eudaly kiln used here will receive 160 000 brick. They are well made kilns, tight and about perfect. When the kiln is filled the two ends are bricked up and plastered. Wood is used for from two to four days to heat them up gradually, then bituminous lump coal is used, and the whole kiln kept at a high heat and held at this heat for four or five days, or until the top of the brick in the kiln has settled about 2 feet. It has been found that the application of the heat is the all-important detail in making vitrified brick. The toughness and temper of a brick requires that the dried clay be heated gradually and never allowed to chill until it attain a point past the cherry red, tending to white heat. It must be held at that heat three or four days and then gradually cooled, which takes five or six days. There have been more bricks spoiled from changes in temperature after the fires were started than from all other causes together. I think cold air has too easy access to the flues when the fires are out or low. Some kilns have been spoiled so thoroughly that the bricks were like dried clay, without any ring, and of a dull, dirty color, and not fit for any use. There have been but few bricks spoiled from overheating, when it has happened some of the bricks would run or melt and stick together, but otherwise would be of good quality.

I have known this plant to deliver three kilns, one after another, with over 90 per cent. of them first-class, good brick; then following these good brick would be one or two very bad kilns. This uncertain

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risk is one of the details of brick burning that ought to be obviated. It is something that with the present methods seems almost impossible to prevent. That intelligent men, with their money at stake, cannot remedy this seems improbable; yet it goes on from month to month, and people buy and use for other purposes those brick which will not do for paving brick.

Brick pavement has been tested here since May, 1890. Since then there has been laid 73 000 square yards. I think it has met with the approval of our people, but there are many things about it that are not altogether satisfactory to an engineer—so long as the brick are each and every one first-class, well tempered, tough and well vitrified, the pavement is without fault. When a kiln has been properly handled and 90 or 95 per cent. of all the brick are first-class, then it is an easy task to inspect and have good pavement; but when one-half of the kilns burned have been chilled or otherwise spoiled (and it has happened that whole kilns have been), then it is that the inspector has trouble. After they have been laid and traveled over and the "skin" worn off, and a large number of worthless brick found in the pavement that ought not to have been there, this is annoying at least. Until the science of brick-making is advanced so that each and every kiln can be burned and tempered exactly right and all the brick come out good and true, we have not much to boast of. I have used St. Joseph, Mo., Atchison Soldiers' Home, and Osage City Brick here during the last two years, and I am satisfied that all have the same or about the same trouble in burning vitrified brick.

HORACE M. MARSHALL, M. Am. Soc. C. E.—Inasmuch as the author concluded that, "the test for crushing strength appears to be worthless, etc.," it is surprising to see the requirement. "The modulus of rupture in compression to be 1 700 pounds per square inch;" more especially as the said bricks would stand several thousand pounds before beginning to crush. Transverse strain or cross breaking is probably intended, but if so, why not prescribe whether laid flatwise or on edge when tested? No method for abrasion test is suggested, though there seems to be great diversity and want of accuracy in the usual tests, that is, Prof. Ira O. Baker so thinks. His pamphlet, "Brick Pavements," covers much of the subject in very close agreement with Mr. Calkins. If bricks undergoing the abrasion test were clamped at one end of a "rattler" box by some device like a printer's form, so they would receive blows from the shot on one face or edge, while they were themselves supported at back and sides, the conditions would more nearly approach the actual facts in a pavement. From the size and weight of the shot, the length and the number of turns of the rattler, a fair comparison could be made between the impact in the test and in a roadway. With bricks that resist abrasion as well as granite, a pavement should be possible which would outwear granite, for it could be laid smoother. Judging from the table given, the builders of brick pavements never read what Mr. Clemens

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Herschel, M. Am. Soc. C. E., wrote on road foundations. On page 20 of his prize paper, "The Science of Road-making," he says: "The foundation to be solid, unyielding, porous and of large material." Again on the next page: "The point never to be lost sight of, is that this foundation course must remain porous, must be pervious to water so that all rain water that soaks through the top covering will find, through it, means of escape to the ground underneath; thence, according to the nature of the subsoil, it is either left to soak into the ground, or must be further led away by appropriate drains." Brick pavements without foundation may be good, but with a foundation they should be better.

Why a roadway should ever have its surface conform to a circle is one of the things past finding out. If the slope near the middle is sufficient to cause the water falling there to flow off, why then should the slope be increased towards the sides? The rational form for drainage would be two planes declining from the median line, say 1 in 300, but as the angle along the intersection of the planes would be objectionable, it should be rounded off with a curve whose radius is practically 900 feet, so as to be tangent to the planes about 3 feet from the median line. If a gutter is desired, make it an invert and round off the edge where it joins the plane of the roadway.

WILLIAM C. JEWETT, M. Am. Soc. C. E.—Mr. Calkins' paper on brick is certainly very interesting and instructive to the members who are using large quantities of brick.

The work of arching the tunnels on the Cincinnati Southern Railway has been in progress for a number of years under my supervision as Resident Engineer. I have made it a rule to insist on the use of the very best brick to be found in the market independent of the usual tests for strength; and the difficulty of securing suitable brick for this work is apparently increasing within the limits of a reasonable haul, including Cincinnati and Chattanooga. A short tunnel is the most trying place in which brick can be used, and unless the bricks are perfect they will ultimately fail through the action of frost. Brick-makers do not like to assort brick and it is rarely that a kiln can be found that will furnish brick for this work without thorough culling. I attribute the difficulty of securing perfect brick in this section to the recent introduction of machines described by Mr. Calkins as the auger type. The bricks from these machines are usually perfect in form and pleasing to the eye, hence they find a ready sale among builders, but the internal mechanical structure is not adapted to durability, at least such is the case with those that have come under my observation. I have no doubt but that the cause of this trouble is as Mr. Calkins says, the improper tempering of the clay, but even when apparently perfectly made I find that they are not equal to those entirely free from a laminated structure. I have used large quantities of brick made with these machines, but the most rigid inspection has been necessary, resulting in constant contention between

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the inspector and the contractor, the latter knowing that the particular bricks rejected would pass inspection for any other work.

STEPHEN L. HAIGHT, M. Am. Soc. C. E.—In connection with the subject discussed by Mr. Calkins, I would like to call attention anew to the use of the bricks made of furnace slag for paving streets, as there is a superabundance of such material. I saw them in use in Toronto some time since, where they were laid between the rails of a street car line. There was a conspicuous absence of grooving by the horses' feet, such as could be seen where stone blocks had been set in other parts of the same line.

The City Engineer stated, in reply to the objection that they would be slippery and cause frequent falls of horses, that no trouble had been experienced from this cause. A remedy for this danger could be readily obtained by forming grooves in them when they are manufactured.

The weight of such blocks might be an objection to their use at long distances from the place of manufacture, but if properly set they would be much more durable than either brick or stone.

ARTHUR N. TALBOT, M. Am. Soc. C. E.—The subject of paving brick is one of growing importance, especially in the prairie portions of the country where stone of any kind is not to be found. The improvements in the manufacture of brick within the past ten years, and experience with brick pavement extending in two cases over fifteen years with results that are certainly satisfactory for what may be termed the experimental stage, show that where suitable clay may be found, brick constitutes a durable, economical and desirable paving material for light and medium traffic, and also for moderately heavy traffic when best "pavers" are used. But it must be borne in mind that satisfactory results will be obtained only with brick of suitable clay, properly burned, and that such bricks are entirely different from the usual building brick. Some discredit has been thrown upon brick pavement by the use of improper material. The effort to utilize and stimulate a local industry, however desirable in itself, may lead to the acceptance of unsuitable brick.

The absolute necessity of a high quality of brick makes the need for definite specifications more urgent. "The best quality of hard-burned brick," or even the added "shall stand all reasonable tests to which paving material is usually subjected," does not express any definite requirement. Any structural material subject to wear and needing strength, toughness and other qualities should have careful and precise specifications. Imagine structural iron or even cement, especially if manufactured by small local companies, having such vague requirements. Nor does the furnishing of six samples and the requirement that no brick shall be inferior in quality to the samples, obviate leaving the acceptance to the arbitrary judgment of an individual.

How, then, to specify the requisite toughness, hardness and imper-

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viousness is an important question. Of the various tests, those of specific gravity and crushing strength are unnecessary. A mixture of rock with clay may increase the specific gravity without adding to the value of the paving brick. Experiments have shown that crushing or pure compressive strength is not conclusive evidence of toughness; and, besides, this test involves greater care and labor and more extensive appliances than the others. High transverse strength has been found to be an attribute of the paving brick which have stood the test of time. This test may be easily made, even if a testing machine is not at hand, as follows: Caliper the plunger of a hydraulic press, such as may be found in the machine shops of any city, and attach a pressure gauge. Place the end of the plunger against a knife edge (easily cast), resting on the middle of the brick. Then the brick may rest on two knife edges placed for uniformity 6 inches apart and the stress of the plunger may be applied. After finding the pressure of the plunger, the modulus of rupture is easily calculated. For ordinary climatic conditions with the pavement subject to repeated freezing and thawing when wet, the absorption test is an important one. Even where porous brick have been used for sidewalks, subject to comparatively little wear, the surface has rapidly disintegrated from the effects of frost, and for pavement, brick must be impervious enough to prevent this action. These two tests of transverse strength and absorption are simple, and quickly and easily made. The value of the rattler test has been questioned, but, while it does not give the same wear as the pavement is subject to, it does give a comparison between the brick and some standard stone, and so adds to the knowledge obtained by the two preceding tests. The grinding test seems less desirable.

The specifications for first and second quality given by Mr. Calkins, are practically the same as those first proposed by Professor I. O. Baker. It would seem to the writer that the requirement for absorption in the first class are more severe than necessary, though if it can be obtained the life of the pavement will be increased. Pavement made from the second quality has given excellent service in our smaller cities. I should, however, suggest the selection of a certain number of brick with every given quantity delivered, say two to every 10 000, which should be tested as above. In the event of failure to fulfill the conditions a further selection and test should be made before rejecting the lot. In addition, any suspicious lots should be tested. The various tests will train the judgment of the inspector, so that inferior bricks are quickly detected. The above tests refer to top brick. Those for the lower course need not be so strong and tough, but should pass a high absorptive test. The application of systematic tests to paving brick will tend to raise the standard of the product and methods of manufacture, as it has that of cement, iron and steel.

By GEORGE W. RAFTER, M. Am. Soc. C. E.—Mr. Calkins' paper is

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of interest and value, not only for the information in relation to recent processes of brick manufacture, but, more especially, because of the discussion on the use of brick for street pavements, as such discussion is likely to be productive of great good. The demand for a satisfactory paving material is great in those regions of the West where stone suitable for either block or macadam paving is in many localities entirely wanting. The use of wood has nowhere proved to be economical.

The absence of stone of all kinds for long distances in Illinois and neighboring States is a thing which, to be appreciated, must be seen. For example, on the Cairo Branch of the Illinois Central Railroad, built in 1879, suitable stone for ballasting can be obtained at only two points in a distance of 364 miles between Chicago and Cairo. At that time brick pavements were just coming into use in Bloomington, Decatur and some of the other interior towns of that State. The early brick pavements were mostly poor affairs, but the improvements since made in processes of manufacture, burning and selection of bricks and in methods of laying have greatly increased their wearing qualities.

In regions where what may be termed normal paving material is difficult to obtain, necessity has thus developed in brick a material fairly suitable considering the special conditions there existing. It is not creditable, however, to the sagacity of many of our urban communities that the commercial spirit has led to the use of brick in places differently situated, where better material could be had, and where, if ultimate durability is taken into account, such use can only lead, so far as we now know, to a relatively speedy removal and a greater ultimate expense.

However, in many Eastern cities, where pavements and other street improvements are paid for by local taxation, the struggle of the pavements is fully on. Such taxation carries with it in the cities in New York the right of selection by the taxpayers of the pavement to be laid, and as a result the paving interests maintain an agent in nearly every town of any considerable size. These gentlemen are all alert, each thoroughly posted as to the merits of the wares which he is charged with selling, and each makes it a point of special merit that he is on terms of amity with as large a number as possible of the taxpayers of every street which it is contemplated, even remotely, to permanently improve. Under the influence of the active canvassing which results, the people of the prospectively improved street divide into several parties, each vigorously advocating such form of pavement as accident, self-interest, or friendly feeling for a paving company's representative happens to dictate. Very rarely is the question of ultimate economy considered other than remotely.

In a case which the writer noted not long since, petitions to the Common Council were in circulation for the following pavements: Medina sandstone, three different varieties of sheet asphalt, block asphalt, brick, cedar block, and two forms of Telford roadway, making

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nine in all. In at least one case on the street in question the discussion became so warm that two life-long neighbors were only prevented from coming to blows by the interposition of mutual friends.

At the present time eight varieties of pavements are in use in Rochester. Their designations and the approximate cost per square yard are indicated in the following statement, the prices there given including also the cost of all necessary excavation and foundation. In the case of the asphalts and brick the price further includes the cost of repairs for a term of years:

	Per Square Yard.
(1) Medina stone pavement, costing from	$1 75 to $2 25
(2) Trinidad sheet asphalt pavement, costing	2 75
(3) Sicily rock asphalt pavement, costing	2 75
(4) Vulcanite sheet asphalt pavement, costing	2 75
(5) Block asphalt pavement, costing	2 65
(6) Brick pavements, costing from	2 25 to 2 50
(7) Telford pavements, costing from	1 00 to 1 25
(8) Telford pavements, with gravel superstructure instead of broken stone, costing from	90 to 1 00

Of these several pavements it may be stated that the Medina sandstone is the natural material for heavy traffic in this city. The maximum development of this stone occurs at Albion, 30 miles west, on the line of the Erie Canal, and water transportation enables the rock to be laid down at a nominal cost during the navigation season. Sand for the foundation is obtained within or near the city limits, and there is no rational reason why the City of Rochester should ever go abroad in search of a fairly durable pavement material for its business streets. The price of $1.75 to $2.25 per square yard, as given in the foregoing, is the present ruling figure, the range in price being chiefly due to the local accidents of varying distances for hauling material by team, labor, etc. Quality of paving material has also, to some extent, an influence on the price.

While I refer to Medina sandstone as the normal material for pavements on streets with heavy traffic here, I do not wish to be understood as claiming for it superiority over granite and some other forms of stone block pavement, which, however, are not entirely available at Rochester by reason of long haul. My point, so far as I make one, is that by reason of its accessibility the Medina stone is the natural material, and durable pavements for heavy traffic can, all things considered, be procured here cheaper by its use than in any other way.

The price of $2.25 per square yard as a maximum, however, procures a pavement which, under the traffic of ordinary streets, will, with moderate expense of annual maintenance, show a life of nearly thirty years, at the end of which time the blocks will be so nearly worn out as to require entire renewal, which may ordinarily be accomplished, inas-

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much as the sand foundation is already in place, for about $1.50 to $1.75 per square yard.

It may be reasonably inferred from the foregoing that the Medina sandstone pavements used here are of the second class, that is, they are laid on sand foundations rather than concrete. Thus far no first-class stone pavements with concrete foundations have been put down in Rochester. Such pavements are hardly suited to streets which have not, in the matter of the various necessary underground conduits, reached what may be termed a fixed condition, the frequent cutting through of the concrete foundations being relatively a more serious detriment to the pavement than the removal and replacing foundations of sand. Medina pavements are mostly used on the business streets, and all these may be considered as to the present time, subject to more or less pulling up. For instance, it is contemplated to build on the line of the chief business streets during the coming season, new sewers much deeper than those now existing, the new sewer construction to be followed probably by the putting down of permanent first-class pavements on the two streets in question. The asphalt and brick pavements now in place in this city are on the contrary on streets where fixed conditions may be considered as existing, and on which there is therefore no objection to making a permanent concrete foundation.

On the proper solution of questions of the nature of the foregoing will depend to a great extent the quality of the pavements in any town which has reached approximately the finished state.

The Telford pavement, as used in Rochester, has little to recommend it. The only available material is a hard limestone which is still far short of furnishing a satisfactory roadway. As constructed here these pavements have about 10 inches of supermaterial, and under ordinary traffic wear down to the substructure in from three to five years.

At the present time there are no wood pavements in use in the city of Rochester, all having been replaced with other material a few years ago.

Medina sandstone pavements are objected to for residence streets on account of the noise made by passing vehicles, and in Rochester as elsewhere, a demand has arisen for a noiseless pavement on such streets. For this purpose asphalt has been largely used during the last few years, and brick are now being introduced for a like purpose. On residential and suburban streets with light traffic, probably no very serious objection to such use of the better quality of paving brick of recent manufacture can be made, though before adopting them largely, even for light traffic we ought to know more of the wearing and lasting qualities of the material. To adopt them for the principal business streets of a city of the size of Rochester, especially one with large iron or other manufacturing interests, involving much heavy drayage, would be an exceedingly bold departure. Mr. Calkins, while expressing the opinion, based

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on experience, that bricks composed of suitable material, properly made, burned and laid, will furnish a durable pavement, is still unable to give any definite statements of their relative efficiency in comparison with other paving materials under like conditions of use. This lack of definite information on a vital point, cannot be charged, however, in any degree upon the author of the paper. The responsibility for it, must be put largely upon the indifference of the street maintenance departments of our municipalities.

In illustration of the importance and value to an urban community of definite information on the question of relative costs of maintaining the different kinds of pavements, I present a tabulated statement of the first cost of pavements of all kinds, including the Telford as laid in the City of Rochester in the 10 years ending April 1st, 1891. The figures given are the totals of the final estimates for each year, and include the cost of curbs and such other accessories as are usually constructed in connection with permanent improvements, as for instance, additional surface sewers, lot laterals and manholes to sewers, the cleaning of sewers, etc., and in the case of the asphalt pavements the cost of extending water services to the curb for all vacant property. All these various items over and above the cost of pavement proper, may be taken at about 15 per cent. of the whole. On this basis the expenditure for pavement proper for ten years may be stated for even figures at $1 500 000.

TABLE SHOWING THE COST OF PERMANENT STREET PAVEMENTS IN THE CITY OF ROCHESTER, N. Y., FOR TEN YEARS. ALSO THE POPULATION BY YEARS FOR THE SAME PERIOD.

Final Year, April 1st to April 1st.	Medina Stone.	Asphalt, all kinds.	Brick.	Telford, all kinds.	Totals.	Cost of Repairs of Improved Roadways.	Population.
1881 to 1882.	$60 151 23	.....	.....	$76 804 97	$136 956 20	.....	*93 100
1882 to 1883.	25 772 70	.....	.....	69 415 76	95 188 46	.....	95 650
1883 to 1884.	51 602 31	.....	.....	60 728 23	112 330 54	.....	99 850
1884 to 1885	36 882 85	.....	.....	6 117 33	43 000 18	.....	104 200
1885 to 1886.	.....	$27 000 00	.....	.....	27 000 00	$14 405 95	108 500
1886 to 1887.	23 396 89	97 681 19	.....	14 887 59	135 965 67	28 937 64	113 900
1887 to 1888.	113 405 45	117 889 99	.....	86 779 03	318 074 47	20 960 03	119 700
1888 to 1889.	7 341 39	16 100 00	.....	55 777 21	79 218 60	12 939 25	126 500
1889 to 1890.	86 950 03	172 341 36	.....	58 650 24	317 941 63	11 596 63	132 000
1890 to 1891.	116 185 78	210 289 36	$58 843 89	24 173 83	409 492 86	16 696 05	138 000
Totals....	$521 688 63	$641 301 90	$58 843 89	$453 334 19	$1 775 168 61	.....	.....

* Official population in 1880, 88 366; in 1890, 133 896. No additions to territory.

As to the relative cost of maintaining various classes of pavements in the City of Rochester, practically nothing is known. Beginning with the year ending April 1st, 1886, some attempt has been made to show the total cost of repairs to all kinds of improved roadways, but even these

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statistics have not been kept in such manner as to inspire much confidence in their accuracy. So far as the writer is informed, aside from the year ending April 1st, 1886, no attempt has been made to separate the items of cost of repairs for the different classes of pavements, which is really all that is valuable in statistics of this character. As they stand, however, they make a strikingly elegant edition of the play of Hamlet with the moody Prince of Denmark absolutely left out.

It seems to the writer, however, so self-evident a proposition as to require no special proof of its reasonableness that in any city where an expenditure of $1 500 000 is made for paving materials and labor in ten years, that some little attention to relative values might be paid, as will be noted on inspection. During the past ten years the use of asphalt has begun in Rochester and has already so extended as to make a very large percentage of the total of pavements laid during that period. During the last two years brick has fairly gained a foothold, and present indications are that its use will be considerably extended in the next few years. In the meantime, definite information as to the permanency of the different grades of brick used for paving is greatly lacking. This fact was very forcibly impressed upon me on reading the discussion a few months ago of the question of paving brick at the meeting of the National Brick-makers' Association of Zanesville, O. The gentleman opening the discussion, Mr. Jones, said in effect that any good building brick clay would make, by proper treatment, a good paving brick for light travel; but that for heavy traffic a semi-fine clay or some of the shale clays should be used. The next speaker, Mr. Shea, took issue with Mr. Jones on the question of quality of clay, his view being that fire clays are to be avoided. Mr. Tiaffny, who spoke later in the meeting, expressed strongly the opinion that a clay, suitable for sewer pipe, would make a good vitrified brick, his view being clearly that some degree of vitrifaction was essential to a durable paving brick. On the question of vitrifaction the views of the manufacturers were equally at variance, as indeed might be expected, the quality of vitrifaction being a function of the clay. Mr. Beattie held that the ideal paving brick should be impervious to water and stand immense pressure, and stated that brick manufactured by his firm at Atchison, Kansas, stood 12 000 pounds to the square inch. He also stated as an additional necessary quality that paving brick must possess toughness in conjunction with vitrifaction. Mr. Ittiner, referring to the material in the vicinity of St. Louis, stated that vitrifaction was an injury to the clays found there. Mr. Tiffany, speaking to the question of vitrifaction, said it was a matter of experiment and trial with different clays in order to determine the quality of clay best suited to produce a paving brick which will answer all the several requirements.

On the question of absorptive properties of brick Mr. Calloway stated that he had in the exhibition-room a brick made on a soft clay

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machine that was thoroughly vitrified, and would stand 90 tons pressure. He offered to donate $10 000 to the Association if his brick showed any absorption after twenty-four hours' immersion in water. Messrs. Beattie, Ittiner and Purrington were all of the opinion that brick would eventually supersede all other forms of paving and Mr. Ittiner expressed the opinion that all present would live to see granite taken up and brick put down in its stead.

My purpose in giving the views of the several leading manufacturers as briefly abstracted in the foregoing, is to enforce the following points:

(1.) The manufacturers differ widely, not only as to what constitutes the ideal paving brick, but as to the best method of producing it. The statement of Mr. Tiffany that it is a matter of experiment and trial in each case probably represents the real state of the matter.

(2.) It is apparent that the present product of paving brick must vary considerably in different localities, some, undoubtedly, being far superior to others. In the absence of definite standards of quality, and with the strong commercial competition existing, it is clear the consumer has, as usual, to bear in many cases the burden of proving by actual experience the essential worthlessness of the inferior varieties.

If brick paving is really a legitimate competitor of granite and other first-class paving material, and has come to stay in that character, it is submitted that the manufacturers can very well afford to expend enough in the way of concerted scientific experimentation to remove from the subject the conflicting and contradictory views which now so extensively prevail. Independent of the greater probability of the consumer getting the worth of his money which may be expected to result from such work, there is a commercial side to the transactions, to the benefit of the manufacturers themselves. Brick pavements are, in this city, like asphalt laid with a guarantee of all repairs on the part of the contractor for a definite term of years, and probably the same condition is exacted elsewhere. The removal, by actual demonstration, of the uncertainty as to the wearing quality of brick, which now largely exists in the public mind, may be expected to operate to the advantage of the manufacturers, not only by entirely doing away with the time guarantee, but by leading to an increased use of brick for paving purposes.

In order to indicate some of the lines which such an investigation of the new paving material may reasonably pursue, I submit herewith, without comment, two self-explanatory letters published in "Paving and Municipal Engineering," for February, 1891.

Editor of Paving and Municipal Engineering:

In response to your call for suggestions in regard to a uniform scheme of thorough tests of paving material, I would submit the following for testing stone (natural or artificial), and brick:

1. In compression—stress being applied continuously and gradually.
2. By concussion—by dropping weights upon them. This test

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would determine their brittleness and their power to resist sudden shocks.

3. Porosity.

4. By Hacking.—In this test I would use some means of cutting the brick or stone, with some instrument of about the same degree of sharpness as a common trowel, a weighted knife dropped a known distance and determining its action, and number of strokes necessary to cut a certain amount, would be a very important test. One of the most worthy tests, being easily and quickly made, is to take a sample and hack it in two with a trowel. If it scales and splits off you may know that it will act the same way under traffic, and a little practice will soon enable you to determine its relative hardness and toughness.

5. Grinding on stone to determine its toughness.

6. Polishing.—Determine the angle of friction between a sample and steel or iron, and then after rubbing two samples together a stated time under known pressure, determine their angle of friction, which would show their tendency to polish.

7. Tumbling—by putting samples in a cylinder with sharp castings and determining their loss.

8. Determine the relative "noisiness" of samples by laying the material in the same manner as laid in the pavement and dropping steel or iron weights on them and measuring the distance the concussion can be heard.

These tests would, I think, about cover all the essential tests, unless it would be the "freezing," which would hardly be practical. The bicarbonate of soda test might be applied, but would hardly justify the undertaking.

HIRAM PHILLIPS, C. E.,
Eng'r Dep't Mo. University.

To the Editor of Paving and Municipal Engineering:

GENTLEMEN,—Accepting your invitation to submit practical tests for paving, permit me to suggest the following: That at Indianapolis six blocks, or six divisions, of equal length, aggregating twelve hundred feet, be selected by the committee; place upon the whole a uniform foundation of limestone macadam, or limestone Telford, or limestone native cement Beton, or limestone Portland cement concrete, or limestone Portland cement concrete interlocking, double-lever press blocks, as the committee may select.

On division "A," pave with granite-quartzite blocks.

On division "B," pave with Eastern granite blocks.

On division "C," pave with best sheet asphalt.

On division "D," pave with granite-quartzite selenite macadam.

On division "E," pave with best paving brick.

On division "F," pave with best cedar blocks.

Explanatory of location: The street to be used for tests should be

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such as will lie in and from the business section of the city, hence, permitting the arrangement by which granite-quartzite blocks, being the hardest of all material, would get the most exposure. The top of the foundation would be everywhere the same, but, as in the case of asphalt, etc., they would gladly put on a sub-top, to adjust the thin wearing coat to general conditions.

To meet actual expenses only, let the committee select a division where the city has laid an assessment. Secure a five-year guarantee for repairs or repaving, each company to have its own methods subject to the specifications and the orders of the City Engineer of Indianapolis. I cannot conceive of a more positive good than such a test would insure.

CHICAGO, January 22d, 1891. W. L. S. BAYLEY,
Civil Engineer.

Mr. Calkins' suggested standard of quality is in line with the foregoing, and I do not wish to be understood as in any way attempting to controvert the practical utility of the standards which he has proposed, although I wish to cite to him the Providence tests by way of indicating that his proposed standard of abrasion will be somewhat difficult to attain. My points are: (1) that we need more information; and (2), inasmuch as the municipal boards have thus far seen fit in only a few cases to assist in furnishing it, the magnitude of the paving interests is such as to justify the manufacturers themselves in furnishing by concerted action what is now lacking. The expense, while considerable, would not be excessive, and if the results of a thorough series of scientific tests essentially verify the claims which have been made, the benefits to the brick paving interests will be almost incalculable.

In reference to the specification for quality which Mr. Calkins cites, it may be remarked that if we had a standard of durability and fitness already established with approved methods of ascertaining the value of any given material with reference to the standard fully worked out, then the proposed clause would be a clear improvement on the ordinary specification as quoted by him. As the matter stands, however, there is at the present time absolutely no generally recognized standard of quality except the perfectly general one derived from actual experience in use. The clause which Mr. Calkins quotes with approbation, may be fairly interpreted therefore as admitting to use, any and all material of which it could be claimed that it had been burned specially for street paving purposes, excepting, of course, such brick as failed to conform to the specification for square edges, straightness, freedom from cracks, etc. The general adoption of the clause in question would, therefore, it seems to me, in the absence of a standard, lead to more controversy as to admissible quality than takes place under the ordinary clause. Mr. Calkins states that the author of the proposed substitute clause is to him unknown. The present writer noted it nearly two years ago in specifications for brick pavements, prepared by Mr. John L. Culley and published

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at page 219, Vol. XXIII of *Engineering News* (issue of March 8th, 1890); but whether or not Mr. Culley is the original author, I am unable to say. In thus doubting the utility of the clause in question, I do not overlook the fact that the use of tests for paving material is extending so rapidly as to mark a distinct advance toward more scientific methods in paving engineering. For such advance we are, I think, largely indebted, so far as this country is concerned, to the growth of the brick paving industry.

The few tests recorded in the last two or three years have shown such considerable variations in quality of material from different localities as to markedly emphasize the importance of a standard, probably the best thus far made.*

The need of a standard for tests is shown by those given below:

At Kansas City Mr. Butts tested for compression, absorption and abrasion, while in the Providence series Mr. Shedd made no tests whatever for compression, but tested for absorption, abrasion and penetration by oil. At Kansas City the absorption test was made by breaking the brick in two and immersing in water for forty-eight hours; at Providence the whole bricks were immersed for twenty-four hours. Again at Kansas City the abrasion test merely included placing the bricks in a foundry rattler with a lot of small castings and running the rattler at the rate of about 100 revolutions per minute for fifteen minutes; at Providence, on the contrary, the bricks were run in a rattler for six hours and the loss of weight taken at the end of each hour.

In order to make a specification, avoiding indefiniteness and at the same time furnishing the engineer a certain method of procedure, under any and all circumstances, two courses are open, namely: (1) to either reserve the right to make, before awarding the contract, any test of submitted material that the engineer may see fit to make, and award the contract in accordance with the results of such tests; or (2) prescribe in the specification the definite tests to which the material will be subjected, with such reservations as to time and place as the exigencies of each particular place seem to demand. In Cleveland, "The City Engineer reserves the right after the bids are received and before the awarding of the contract to any bidder, to make such tests of the sample bricks deposited in the Engineer's office, as said Engineer shall recommend or deem necessary to determine the durability and fitness of the material proposed to be furnished by such bidder as shown by his samples, and reserves the right to hold the bid not exceeding two weeks for such purpose, and any brick not fulfilling the requirements of the specifications or that may be deemed unfit for paving purposes, as shown by such tests, will be rejected and the bid considered as not in conformity with said specification."

* *Engineering News*, April 11th and September 26th, 1891; and *Engineering and Building Record*, May 2d and September 26th, 1891.

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I submit for comment and criticism the following clause:

The paving material shall consist of the best quality of sound, hard burned merchantable brick, manufactured with special reference to use for street pavements; all the brick furnished will be subject to test for absorption, abrasion and crushing. Each bid shall be accompanied by at least six bricks of the quality which it is proposed to use in the work, all plainly stamped with the bidder's name, and before the award of the contract the said tests shall be made upon the sample brick so furnished. The results of these preliminary tests upon the samples from the bidder whose proposition is accepted will be considered the standard of comparison during the progress of the work. Any one or all of the tests may be applied in the discretion of the Engineer, at any time while the work is in progress, and whenever, as the result of such tests, the said Engineer shall conclude that material not contemplated by the contract is either being used or proposed to be used, all of the same which he may designate shall be immediately removed, etc.

The following is the quality clause of the specifications for brick pavements now in use in Rochester:

"All brick used must be of the best quality manufactured by....., being uniform in size, clear-ringing, whole, hard-burned, burned entirely through, tough, free from brittleness and cracks, and with true and smooth faces."

The brick pavements, as constructed in Rochester, are provided with concrete foundations 6 inches in thickness, the upper surface of which is made smooth, and parallel with the surface of the pavement to be laid. Upon the concrete base, clean, sharp sand, free from small pebbles, is evenly spread of such thickness, that, after the pavement has been laid and thoroughly rammed, the compacted sand will be 1 inch in depth. Before laying the concrete the sand is thoroughly compacted with a 5-ton roller. The evenness of the sand is insured by a template formed to the prescribed crown of the street, which is laid across from curb to curb and drawn forward by hand.

The bricks are set on edge, vertical, and to line in courses at right angles to the curbs; and after being placed in position they are settled to place by ramming with heavy mauls used on 2-inch plank. After the ramming the joints are completely filled with an asphalt paving cement, used hot, and the joints repaved until completely filled. In good work from 2¼ to 2½ gallons of the cement are used per square yard, and at a cost, including labor, of about thirty cents for that area.

I offer, in conclusion, the following views:

(1.) For light traffic a brick pavement may be made at low cost in places where the material is close at hand, which will fairly meet the conditions for a number of years.

(2.) For medium traffic the making of the concrete foundation which appears necessary, in order to secure stability, increases the cost so

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much that no special gain can be predicated, provided other approved material is available.

(3.) For heavy traffic we as yet lack the data to justify the use of brick for paving.

(4.) In localities where no other durable paving material is available the use of brick is to be commended, and the perfecting of the material and methods of use have been of value to extensive regions, where this condition largely exists.

(5.) It is not fair to conclude that what is best suited to one locality is necessarily the indispensable condition for successful and economical paving in another. The fitness of the material for the special locality ought to chiefly govern the decision as an engineering question.

(6.) We need greatly a definite standard of quality in which shall be clearly defined by several different tests the relative efficiency of a number of the leading paving materials, in such manner as to indicate the approximate durability of a given material under given conditions of use.

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ELECTRIC LIGHTING AT TOPEKA, KANSAS.

By LEWIS KINGMAN, M. Am. Soc. C. E.

WITH DISCUSSION.

There are few wants more necessary in a well regulated community than light. After securing a generous supply of pure fresh air, good wholesome water, clean streets and surroundings well drained, we need light by night as well as by day. During the last few years many of our smaller cities, seeing the improvement made in larger places, have secured electric light plants. Some, from a spirit of rivalry, have gone into these enterprises blindly, without counting the cost or carefully considering the questions involved. The strong, convincing argument in favor of the arc lamps is the great concentration of light in one lamp. But the light received is as the square of the distance lighted, a fact that is often overlooked.

There are many inquiries constantly being made as to the cost and operation of an established plant. One of the first asked is, does the city own its plant? A question in municipal economy which should re-

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ceive more attention. That a city should be free to supply its own needs and that of its citizens at cost, without the presence of middlemen to overcharge or discriminate, would seem self-evident.

The next important question is the cost. In reply to such questions, the author has prepared a tabulated statement, taken from the reports of the superintendents, which covers the operation of the plant for the last twenty-six months, giving the number of nights and hours run each month and the principal elements of cost, together with the cost of fuel for each horse-power for one night and hour.

The plant belongs to the City of Topeka; it was contracted for in February, 1888, and furnished by the Jenney Electric Company, of Indianapolis. There are one hundred and eighty-four double carbon arc lamps, three boilers 16 feet long, 54 inches in diameter, with forty-five flues 4 inches in diameter; three Ball engines of 75 horse-power with 80 pounds of steam pressure, intended to run two hundred and fifty revolutions a minute, six dynamos of thirty arc lamp power, connected with six circuits which together have 24.72 miles of line and 33.5 miles of wire; the line is suspended upon 30-foot cedar poles, and the lamps are suspended at the center of the street intersections between two 40-foot poles. Soon after starting it was found necessary to use more than 80 pounds of steam pressure, and in doing this the capacity of the boilers was found inadequate, except with the best bituminous lump coal. From the tests and experience of a superintendent who was in charge of the plant about one year, it was found that the three engines had to work up to 235 horse-power to keep the lamps burning steadily. The exact actual horse-power record has not been kept during all this time.

The accompanying table shows some irregularities. Owing to lack of boiler capacity the plant has to be shut down two nights each month to clean them. This is usually done on moonlight nights, but sometimes it is cloudy and the moon obscured, much to the annoyance of our people. In October, 1891, the plant was shut down for fifteen days to reset the boilers.

The monthly statements of the cost of coal is for coal received and paid for during the month; this comes by the carload, which is ordered as needed, and very little is kept in stock at any time. Owing to the lack of capacity of the boilers, the poorer and cheaper coal, such as is used by other electrical plants in the city, cannot be burned.

An expert tested some of our lamps without the knowledge of the

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superintendent and operators, and found as an average of seventeen lamps in different parts of the city a pressure of 23.15 volts, a current of 19.0 ampères, and an electromotive force of 439.85 watts. The manufacturers of electrical plants using arc lamps have fixed an arbitrary standard of their own for candle-power. Six of the largest companies allow from 432 to 480 watts for a 2 000-candle lamp; the average of these is 450 watts to the lamp or 4.44 candles to the watt. The author has endeavored to find some reason for their assumption, but knows of none except that they overrate their lamps for what they can gain by imposing upon their customers. The best authority that he can find on the subject is Gordon Wigan, A. M., whose work was printed in 1884 by Cassel & Co., New York, and who makes one watt equivalent to 1.63 candles, and cites a test of candle-power at the Paris Electrical Exposition of 1881, when the Brush lamp was tested and shown to be 741 candle-power. By the American manufacturers' average arbitrary standard, the seventeen lamps tested averaged 1 953 candle-power, allowing 4.44 candles to the watt; but taking Wigan's and the European practice our lamps are only 717 candle-power, allowing 1.63 candles to the watt. This is a great difference, when it is considered that we use the same English standard candles as are used in Europe and elsewhere, for our standard in this country.

The location of the electric lamps in our city has been a subject of much criticism, with one lamp at every other street intersection ; the lamps are about 1 100 feet apart one way and 800 feet the other; the intervening intersections are not well lighted, which is hardly fair to the people living there. With one lamp at every intersection the light would be of much more value, even though the intensity was only one-half of the light of the present lamps.

We have in addition to our electric lamps 222 vapor lamps furnished and lighted by contract at $1.75 per month. These are located in the outlying districts. People complain because they are so insignificant as compared with the arc lamps, and when one compares a vapor lamp of 10 candle power with a 700 candle power lamp, it is not much to be wondered at that some of these districts should complain through their representatives. The City of Topeka is spending $21 260 a year for lighting its streets, and yet people are not satisfied. About one-half of those inside of the electric light district feel that an injustice has been done them, and all of the people outside of this district seem to think

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Cost of Operating the Topeka

The plant consists of a building and lot with a railroad switch in the
arc dynamos of 30 lamp-power each. There are six circuits, with 24.72
at the center of the street intersections between two 40-foot poles, one
184 arc lamps rated at 2 000 candle-power. An average test at the lamps

Year and Month.	Nights Run During Month.	Hours Run During Month.	Average Number of Hours Run.	Coal.		Cost of Horse
				Tons.	Cost.	Per Night
						Cents
1889. Nov.	28	340.03	12.14	236.0	$566 40	8.6
Dec.....	29	373.47	12.88	261.0	626 40	9.1
1890. Jan..	29	357.40	12.32	222.0	532 80	7.8
Feb.	26	298.73	11.49	216.9	520 56	8.5
March ..	27	274.83	10.18	194.85	459 40	7.2
April....	30	265.42	8.84	190.0	456 00	6.4
May	28	171.92	6.14	156.0	374 40	5.6
June.....	27	132.91	4.92	76.0	182 40	2.8
July	28	146.34	5.22	110.4	283 86	4.3
August..	28	182.50	6.51	144.0	345 60	5.2
Sept.....	27	211.69	7.87	154.3	403 66	6.3
Oct.....	25	204.04	8.16	110.0	361 11	6.1
Nov.....	24	206.00	8.58	126.35	385 37	6.8
Dec.....	25	198.25	7.93	123.4	373 70	6.3
1891. Jan..	24	189.17	7.88	122.65	374 08	6.6
Feb.	21	157.50	7.50	83.75	249 18	5.0
March...	23	158.50	6.89	102.80	302 73	5.6
April,....	21	141.83	6.75	109.0	317 42	6.4
May	23	127.05	5.52	101.0	294 99	5.4
June	23	118.08	5.13	86.8	264 74	4.8
July.....	27	138.75	5.14	75.13	230 73	3.6
August,..	28	172.05	6.14	90.10	274 80	4.1
Sept.....	30	251.05	8.37	108.95	331 55	4.7
Oct.....	16	140.75	8.79	106.00	323 30	8.6
Nov.....	28	244.41	8.73	136.45	395 70	6.0
Dec.....	27	227.50	8.43	143.20	415 28	6.5
Totals.....	672	5 430.17	Av. 8.08	3 587.03	$9 646 16	Av. 6.1
Per cent. of total expense.....					26.04	.....

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PLANT, OWNED AND RUN BY THE CITY OF TOPEKA.

boilers and three engines, worked up to 235 horse-power; there are six Jenney, Indianapolis
miles of wire. The line poles are 30 feet long, of which there are 943. The lamps are suspended
intersection, making them 1 100 feet apart one way and 800 feet the other. Together there are
3.15 volts and a current of 19.0 ampères, 439.85 watts, or 1 953 nominal candle-power.

Cost of Globes.	Cost of Oil and Waste.	Cost of Repairs.	Expenses Insurance Telephone.	Pay-roll of Superintendent and Labor.	Interest on Cost of Plant, at 6 per cent	Total Cost Entire Expense, per Month.	Cost per Lamp per Month.	Cost per Lamp per 100 Hours.
$13	$22 15	$12 15	$32 88	$483 32	$277 24	$1 517 71	$8 25	$2 43
15	27 27	133 74	29 58	531 00	277 24	1 775 88	9 65	2 59
15	26 93	167 97	30 68	497 50	277 24	1 694 27	9 21	2 58
00	23 31	154 89	28 58	496 25	277 24	1 632 83	8 88	2 97
00	23 45	180 13	29 58	498 00	277 24	1 603 80	8 71	3 17
38	25 29	90 21	32 59	509 75	277 24	1 514 13	8 23	3 11
17	24 06	27 47	29 38	508 33	277 24	1 319 32	7 17	4 17
50	32 25	94 07	31 83	516 50	277 24	1 188 69	6 46	4 86
50	19 60	100 78	37 93	523 38	277 24	1 296 29	7 04	4 82
50	21 36	57 90	32 48	515 75	277 24	1 323 10	7 19	3 94
00	25 32	48 95	32 73	516 50	277 24	1 391 38	7 56	3 56
00	22 10	64 72	34 48	467 75	277 24	1 311 96	7 13	3 50
00	22 00	79 78	49 03	523 41	277 24	1 419 85	7 72	3 75
50	15 61	86 11	31 78	525 00	277 24	1 396 59	7 59	3 83
00	19 89	100 29	38 08	530 25	277 24	1 417 53	7 70	4 07
50	17 30	195 94	33 26	529 50	277 24	1 371 64	7 46	4 75
50	13 83	102 11	36 73	528 98	277 24	1 335 82	7 26	4 60
00	13 98	41 35	37 36	525 75	277 24	1 280 90	6 96	4 90
00	10 30	56 29	45 56	525 00	277 24	1 272 78	6 92	5 45
00	9 92	56 04	47 46	525 00	277 24	1 229 60	6 68	5 66
00	10 36	39 80	40 91	525 00	277 24	1 183 84	6 43	4 63
00	14 62	67 57	33 31	525 00	277 24	1 266 54	6 88	4 00
00	20 70	171 95	35 61	525 00	277 24	1 457 05	7 81	3 11
00	10 85	730 69	35 16	510 00	277 24	1 949 24	10 59	7 51
00	19 50	110 59	35 86	533 10	277 24	1 477 99	8 03	3 29
00	12 40	74 29	33 01	525 97	277 24	1 432 19	7 78	3 43
$159 05	$504 35	$3 045 78	$905 84	$18 420 99	$7 208 24	$37 036 52	Av. $7 74	Av. $3 71
0.43	1.36	8.23	2.45	36.24	19.46	100.00		

Cost of lots and buildings..... $ 8 559 83
" " plant, poles and wire..... 46 888 65

Total cost..... $55 447 98

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they ought to have as good light as the electric lamps furnish, as they are assessed in the same proportion as those who have them. What we need is a system of lamps at every street intersection, with one circuit covering the business portion of the city on which the lamps shall have an intensity of 1 000 actual candle-power. Then with circuits covering the residence and outlying districts on which the lamps shall have an intensity of 200 candle-power. One objection, perhaps, to this would be the large additional number of lamps to clean, adjust and take care of; another would be the long distance for the transmission of the electric current and consequent loss or leakage.

The present arc lamps are good, perhaps, perfect in themselves, yet the cost is so great that a city occupying a large territory cannot expect to light all and do equal justice to its citizens.

The Edison lamps have not the candle-power suitable for street lighting, but it would seem to the author that a system between the two would be more suitable for many of the growing cities in our country.

The cleaning of the arc lamps and the globes is a large item of expense and is seldom done perfectly. Fifteen per cent. of light is absorbed by a good clean globe, 70 per cent. by an opal globe, and about the same amount by a dirty globe. The rods of an arc lamp must be kept clean or it will fail to burn, and the cleaner is at once reminded of his duty; but he can allow the globes to become obscured so that from 50 to 70 per cent. of the light is absorbed without any serious complaints from an average citizen. Municipal engineers are largely interested in the question of light. We should try and secure its proper diffusion, that equal justice may be done to all. An engineer should first know what his city needs, and then be able to advise, and prevent a fluent agent of a strong illuminating company from selling an extravagant plant, poorly adapted to the wants of its people.

DISCUSSION.

JOHN W. HILL, M. Am. Soc. C. E.—For public street lighting, arc lamps with clear glass globes, of less than 2 000 (so-called) candle power, at all street intersections, with weaker lamps midway of blocks, will probably give the best satisfaction in point of cost and illumination, to the people in the district lighted. The total lighting power of almost any town or village system of electric lighting will be sufficient to fur-

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nish a satisfactory illumination if this be divided so as to give a light at each street intersection, and in long blocks at least one lamp midway of intersections.

While incandescent lamps of 100 or less candle power can be easily and rapidly measured for illuminating power with the photometer, it is much less convenient to measure the illuminating power of arc lamps, and as a consequence there is much guessing at the capacity of such. If it were not for the great cost, I believe incandescent lamps of 100 candle power properly distributed would give a more satisfactory illumination of public streets and highways than arc lamps; with this objection, however, that the illuminating power of arc lamps with clean carbons and globes is a practically constant quantity, while incandescent lights are steadily on the wane, and our 100 candle power light, which, when new, may give 150 candle power effect, at the end of several weeks may have been worked down to 75 or 80 candle power, while the cost of maintaining the lamp will be the same at the lower as at the higher illuminating power.

From some experiments which I made several years ago with an isolated Edison Incandescent Electric Lighting plant, it appeared that the cost of a sixteen candle power light per hour is about three-fourths of a cent, or for 100 candle power lamps, 4½ cents per hour, and with an average illumination of twelve hours per night, the cost of each 100 candle power lamp would be $197 per year, and assuming two incandescent lamps for each 2 000 candle power arc lamp in the Topeka lighting circuit the annual cost would be $70 920, as against an average cost of $93 per lamp per year, or $16 718 per year for the whole number of one hundred and eighty lamps now in use. But the present system furnishes a total illumination of 717 x 180 = 129 060 candle power, while the three hundred and sixty incandescent lamps would furnish only 36 000 candle power, or while furnishing but 28 per cent. of illuminating effect, the incandescent system would cost nearly four and one-fourth times as much as the arc lighting system; or for the same total illuminating effect the cost of the incandescent system would be fifteen times as much as the cost by the present arc system of lighting.

It is this great difference of cost which discourages the use of the incandescent system for street lighting, although, where it can be afforded, the latter will give the most satisfactory results in public highways by bringing the lamps nearer together and closer to the surfaces to be illuminated.

E. E. Magovern, Jun. M. Am. Soc. C. E.—Mr. Kingman's paper, in its introduction, raises the interesting question, applicable to all kinds of municipal enterprise, should the city own its plant? It is difficult to appreciate how any intelligent community, exceptional cases aside, can come to any different conclusion, for electric light plants, at least, than that arrived at in the paper in question. The possible exceptional cases being instances where it would be cheaper for a city to contract tem-

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porarily for its electric lighting rather than bond the municipality in a tight money market at exceptionally high rates of interest. The arguments of those opposed to the almost axiomatic answer above indicated are found in a paper by Mr. M. J. Francisco, read before the National Electric Association in August, 1889, and may be recapitulated thus:

First.—That as the state has chartered a company for electric lighting, the city or town, as a composite unit of the State, infringes on the vested rights of the chartered company in providing its own plant.

Second.—That the cost, if correctly stated, to the municipality, is far in excess of the cost when supplied by a corporation.

Third.—If a municipality can sell electricity cheaper than private companies, why should it not manage all other industries and trades in the community?

Fourth.—That a municipality cannot afford to take the same risks of development in apparatus and in economical management. The writer's reply to these arguments is:

1. That a charter cannot be legally granted, giving exclusive rights to a corporation over individuals or municipalities.

2. Though Mr. Francisco, in a subsequent article (*Electrical Engineer*, Vol. XI, No. 154, page 440), criticises the list submitted below as being "the familiar list, which has proved a veritable will-o'-the-wisp, leading its votaries deeper and deeper into the labyrinths of political necromancy," and though the criticism made in the case of Chicago municipal plants is, to the writer's mind, well founded; yet the preponderance of evidence is clearly in favor of municipal management, and until Mr. Francisco is able to show that more conjuration of accounts occurs with municipal than with private plants, the writer submits that a true basis for judgment exists in the much maligned list (*Engineering Record*, Vol. XXIV, No. 5, page 78).

The following is a list of cities supplied with light by private corporations:

Name of City.	Schedule.	Number of lights.	Cost of light per night, cents.	Candle power.
Kansas City, Mo	All night.	114	55	2 000
Atlanta, Ga	"	596	53½	2 000
Baltimore, Md	"	607	40	2 000
Binghamton, N. Y	"	123	36	2 000
Birmingham, Ala	"	100	33	2 000
Buffalo, N. Y	"	1 296	40	2 000
Cleveland, O	"	100	38	2 000
Denver, Col	"	1 297	53	2 000
Duluth, Minn	"	100	35	2 000
Jackson, Mich	"	210	24	2 000
Lowell, Mass	"	180	50	2 000
Memphis, Tenn	"	100	50	2 000
New Bedford, Mass	"	90	40	2 000
New Haven, Conn	"	.....	48	3 000
Richmond, Va	"	134	40	1 200
St. Louis, Mo	"	1 587	20½	2 000
Sedalia, Mo	Moonlight—out.	91	24½	2 000
Springfield, Ill	"	110	38½	2 000
Troy, N. Y	All night.	297	39½	2 000
Wichita, Kansas	"	90	30	2 000

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Similarly, the following is a list of cities that own their electric light plants:

Name of City.	Schedule.	Number of lights.	Cost of light per night, cents.	Candle power.
Bay City, Mich	All night.	134	16⅔	2 000
Bloomington, Ill	"	220	20	2 000
Chicago, Ill	"	520	10	2 000
Hannibal, Mo	"	96	14¼	2 000
Lewiston, Me	"	96	11⅛	2 000
Little Rock, Ark	"	110	13½	2 000
Madison, Ind	"	85	13½	2 000
St. Joseph, Mo	"	208	14⅓	2 000

3. That it is a well known principle that we cannot logically generalize without abstracting, but that we may perform abstraction without generalization.

4. That the material and practical advances in economy in electric lighting are so slow, that the municipal plant, being less given to experimentation, can well afford to adopt, as they arise, the successful experiments made by private corporations.

Reverting to the table of costs of operating in Mr. Kingman's paper, it is readily found that the average cost per night's run per month is 25.8, and the average per night per light is 30 cents; an excellent result when compared with the figures given in the first table, of cities supplied with light by private corporations, when plants of the corresponding size and the location of the plant are considered. Moreover, the efficiency of the plant, considering the length of the circuit, should be thoroughly satisfactory, as the following calculation will show: One hundred and eighty-four lamps at 439.85 watts per hour equals 80 932.5 watts or 108.5 electrical horse-power (E. H. P.) at the lamp, an efficiency of over 46 per cent. The losses, 54 per cent., include engine friction, transmission to dynamo, dynamo losses, losses in wire transmission and losses at the lamp. The writer has seen higher figures of efficiency, but none as high where the circuit is as long and the lights as few.

The question of electric light standards has been happily presented by Mr. Kingman and is a subject worthy of attention. Some years since, the writer, while engaged in an engine test, in which a dynamo had been provided to furnish the resistance, was requested by the makers of the dynamo to certify to the number of sixteen candle power lamps per indicated horse-power per hour; the photometer provided, though of standard make, proved so unsatisfactory that no certificate could be furnished. While the illuminating power of the electric light was nearly constant, so long as the volt-ampères were constant, it was found that in a thirty-minute run the illuminating power of the standard candle varied nearly 20 per cent. Again, a committee appointed some

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years ago by the British Board of Trade, to report upon photometric standards, found that standard candles from different makers varied so greatly that one flame would in some cases give 16, in others 18 candle power. Beside this, it was found that a candle varied from day to day, and a variation was noted in the same candle of 12 per cent. in two minutes. It is, therefore, of extreme importance to the engineer that some more stable standard be fixed; several of these have been proposed. One, Harcourt's, is the burning of a mixture of 7 volumes of Pentane mixed with 20 volumes of air and the resultant combination burned at the rate of half a cubic foot per hour in a burner of special design so as to produce a flame of definite height. The objection to this standard is, to the writer's mind, in the inherent variable character of Pentane, to wit ("Fowne's Chemistry," edition 1878, page 499):

Normal Pentane or Ethyl-Propyl, C_2H_5—C_3H_7. Rankine has shown ("Steam Engine," etc., Section 224, page 271), "that the total heat of any compound of hydrogen and carbon is nearly the sum of the quantities of heat which the hydrogen and carbon contained in it would produce by their combustion if burned separately."

This would give: Pentane, modification, C_2H_5.

$$C \times 2 \times 12 = 24$$
$$H \times 5 \times 1 = 5$$

Molecular weight = 29

Heat units.

Hence $\frac{24}{29}$ pounds of carbon burned gives, $\frac{24}{29} \times 14\,500 =$ 12 000

and $\frac{5}{29}$ pounds of hydrogen burned gives $\frac{5}{29} \times 62\,032 =$ 10 695

giving for the total heat per pound of Pentane, modification, $C_2H_5 =$ 22 695

And, Pentane, modification, C_3H_7.

$$C \times 3 \times 12 = 36$$
$$H \times 7 \times 1 = 7$$

Molecular weight = 43

Hence $\frac{36}{43}$ pounds of carbon burned gives $\frac{36}{43} \times 14\,500 =$ 12 139

and $\frac{7}{43}$ pounds of hydrogen burned gives $\frac{7}{43} \times 62\,032 =$ 10 098

giving for the total heat of Pentane, modification, $C_3H_7 =$ ..22 237

The resultant mechanical energy is:

For C_2H_5, 22 695 × 772 =.....17 520 540 foot-pounds.

For C_3H_7, 22 237 × 772 =.....17 166 964 " "

Prof. Julius Thomsen (*Philosophical Magazine*, 1865, Vol. 30, page 246), has shown that the mechanical equivalent of light is about 12.28 foot-pounds per minute, or 736.8 foot-pounds per hour per candle power; hence the candle power per pound is:

C_2H_5, 17 520 540 ÷ 736.8 =.....23 779 candles.

C_3H_7, 17 166 964 ÷ 736.8 =.....23 299 "

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Accepting Regnault's determination of the weight of hydrogen, one litre at standard pressure and temperature weighs 0.0896 grams, and the gaseous densities being as one-half the molecular weights, we have, reverting to English measures, 1 cubic foot of C_2H_5 weighs .08107, and 1 cubic foot of C_3H_7 weighs .1202 pounds.

Taking the combination as above, viz.: 7 parts Pentane to 20 parts by volume of air, we have for the actual weight of Pentane burned per candle power per hour:

C_2H_5..... .010509 pounds.
C_3H_7..... .015581 "

and taking these proportions of the candle power of 1 pound of the combination, we have seven volumes Pentane, mixed with twenty volumes of air, and burned at the rate of five-tenths of a cubic foot per hour, equivalent to:

Modification: $C_2H_5 = 23\ 779 \times .010509 =$ ...250 candles.
Modification: $C_3H_7 = 23\ 299 \times .015581 =$ ...363 "

It is evident that this standard, varying as just shown, is as objectionable as that of a sperm candle, six to the pound and burning at the rate of 120 grains per hour. Other proposed standards will, when analyzed as above, be found equally objectionable. What is needed is an exact fixing of the mechanical equivalent of light in foot-pounds per hour or minute, and the requiring, by the engineer, of a maintenance at the lamp of the equivalent volt-ampères. The expression "exact fixing of the mechanical equivalent of light" is made advisedly, as another experimenter, Mr. M. G. Farmer (*American Journal of Science and Arts*, 1866, Vol. 41, page 215) gives such mechanical equivalent as 10.1 foot-pounds per minute. The writer presumes the reason for the assumption questioned by Mr. Kingman can be discovered thus: 33 000 foot-pounds per minute = 746 watts per minute, or 1 watt = 44.23 foot-pounds per minute; and as per Farmer's determination 10.1 foot-pounds per minute = 1 candle power; hence $44.23 \div 10.1 = 4.37$ candles = 1 watt. The figure quoted by Mr. Kingman, the origin of which was unknown to him, 4.44, is a close approximation to this.

The question of the location of the lamps brings to remembrance that a recent number of a well known technical journal contains the following remarkable, unqualified statement: "The proper height at which electric arc lights should be placed is 0.7 of the radius of the area to be illuminated." If the lights were thus placed in Topeka the resultant illumination would indeed be slight. The writer, in conclusion, believes the paper of Mr. Kingman to be exceptionally valuable in that it shows what may be done by competent engineering supervision as opposed to the happy-go lucky method of trusting to the word of such an agent as described, whose pay is in direct ratio to the sales made, and independent of the result of such sales.

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LEWIS KINGMAN, M. Am. Soc. C. E.—Mr. Magovern quotes from the *Engineering Record*, Vol. 24, No. 5, page 78, naming twenty cities supplied with arc lamps by private corporations and eight cities that own their own plants. These statements differ so materially that one is led to believe the facts are not correctly stated. Take Chicago, for instance, 10 cents per lamp all night would be $36.50 per year per lamp. On page 253, Vol. 13, No. 201 of the *Electrical Engineer* the editor states the cost of each arc lamp as being $136.96 per year or 37½ cents per night.

The City Clerk of Hannibal, Mo., informs me, April 15th, 1892, that they have fifty-four public arc lamps and forty-four private lamps, which cost the city $6,985.43 to operate last year, or $71.28 per lamp per year; this would be 19½ cents per lamp per night. The city collects $4,324.70 for the forty-four private lamps furnished, but this does not affect the cost. A much more extended comparative statement was printed by the *Water and Gas Review*, Vol. 2, No. 8, pages 7 to 10 inclusive, compiled by Charles Baillairge, City Engineer of Quebec, in a list of ninety cities; but two are run at less than $90 per lamp per year on an all-night basis, and only eleven out of the ninety are run for less than $100 per lamp per year on the same basis.

Mr. J. P. BARRETT, of Chicago, writes: Our lights are now costing about $100 per annum, not including interest, taxes or depreciation. Our lamps are 2 000 candle-power (nominal), 601 being high tension and 392 low tension; operated from four power stations, with the circuits all underground. The lamps are lighted from dusk till daylight every night in the year, the average being ten hours and forty-five minutes each night. The stations were designed to have capacity for 1 000 arc lamps of 2 000 candle-power each.

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TRANSACTIONS OF THE AMERICAN SOCIETY OF CIVIL ENGINEERS.

Edited by the Secretary, under the direction of the Committee on Publication.

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ON UNITS OF MEASUREMENT :—Thomas Egleston, George M. Bond. S. S. Wheeler, E. Sherman Gould, George W. Plympton.

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The rooms of the Society are open from 9 to 17 o'clock each business day, except Saturday (when they are closed at 15 o'clock), and at other times on application to the Janitor.

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TRANSACTIONS.

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526.
(Vol. XXVI.—May, 1892.)

ON THE LOSS OF HEAD RESULTING FROM THE PASSAGE OF WATER THROUGH A 24-INCH STOP VALVE.

By E. KUICHLING, M. Am. Soc. C. E.

WITH DISCUSSION.

Some time ago the writer had occasion to investigate closely the actual loss of head due to friction in a certain large water pipe under high pressure; and for the purpose of directly measuring this loss in a moderate length of pipe, he devised the simple apparatus shown on Plate XXXVII, whereby the difference in the hydraulic pressures existing at any two points in a line of water pipe is at once exhibited by the difference in height of two communicating columns of mercury. The apparatus is, therefore, merely a pressure-difference gauge, which has the merit of being both sensitive and accurate, and which may also be applied in other similar investigations, such as the determination of the loss of head due to the passage of a certain quantity of water through a partially opened stop valve.

In principle, the gauge consists of two vertical glass tubes, connected together at the bottom and partially filled with mercury, while the upper ends of these tubes are connected with the water main by means of suitable cocks and piping. On the admission of the water into the two tubes, the mercury will be depressed in one and raised in the other until equilibrium is established, whereupon the difference in the

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heights of the two mercury columns is to be read off on a suitable scale, whose divisions correspond to known pressures of water, as determined by careful experiment beforehand. In practice, however, it is necessary to exercise the utmost care to expel all of the air in the tubes above the mercury, except when compressed air alone is applied in both, and for this purpose a little complication of the apparatus seems unavoidable.

The actual construction of the gauge is clearly shown in Plate XXXVIII, where it is seen standing upon the table used for operations in the field. The latter consists simply of a short piece of plank, to which three equidistant tee branches are attached by means of rods, so as to form collars, which may slide upon three long iron stakes driven into the ground, and may then be fixed to these stakes by set screws after the plank is approximately level at the desired height. The gauge is fastened to the plank table by three screws in the tripod which forms its base, and is then brought into a vertical position by means of the leveling screws and spirit levels. To insure absolutely equal initial water pressure upon the two columns of mercury, the two tubes are prolonged upward into a small reservoir or cistern partly filled with water; and if the mercury does not stand at the same height in both tubes, it is evident that some air is contained in one of them, which must be expelled by a judicious manipulation of the inlet cocks. To prevent the accidental loss of mercury in this operation, a hood is fitted over the small cistern mentioned, and is held in place by a set screw. After the gauge has thus been adjusted, the two upper cocks are closed and the full pressure from the main admitted into each tube from the inlet cocks. From time to time the pressure may also be shut off, and the above-described test or adjustment repeated. The scale is mounted between the two tubes, and is provided with two slides carrying arms which extend over both glasses, so that one may be used on one glass and the other one on the other glass. In case of considerable fluctuation, an observer is needed for each tube, and a good check on the observation will be gained if the two observers change places after each reading, and repeat.

An application of this instrument was made on May 31st, 1891, by John Thomson, M. Am. Soc. C. E., and the writer, to the 24-inch inlet pipe of the distributing reservoir of the Rochester, N. Y., Water Works, for the purpose of measuring the losses of head in the passage of the water through a partly opened 24-inch stop valve. This valve

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PLATE XXXVII.
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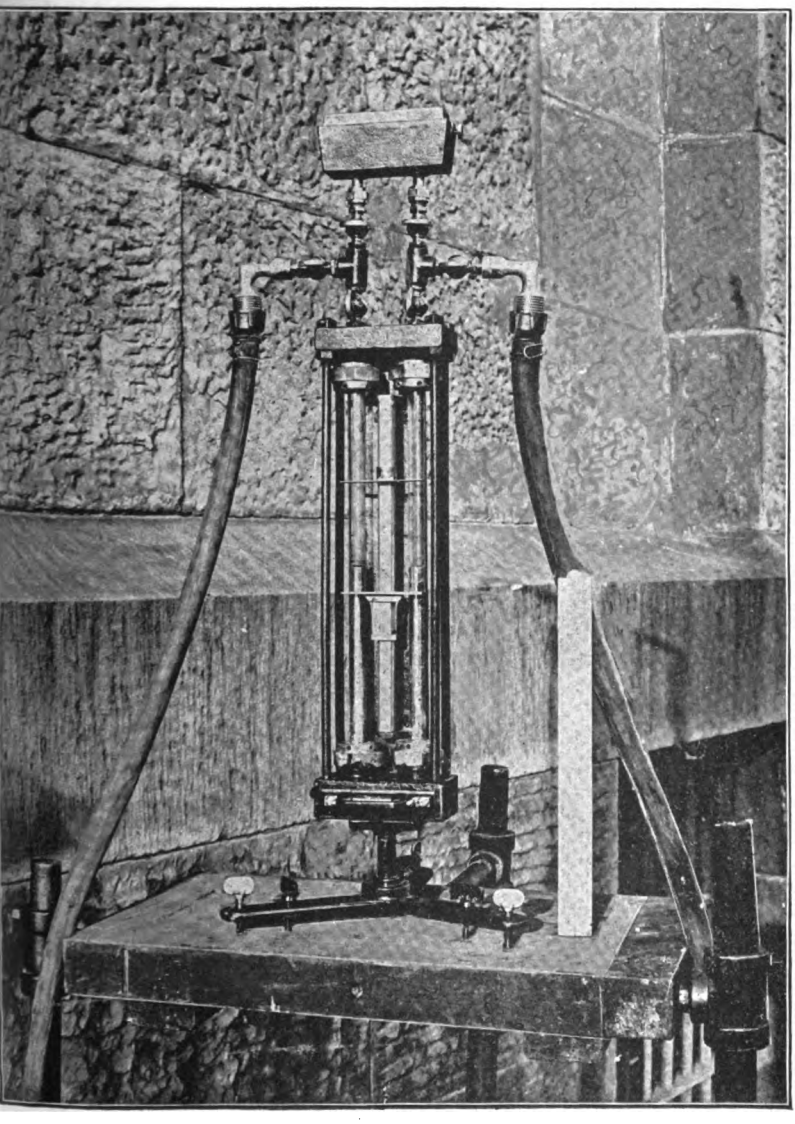
KUICHLING ON LOSS OF HEAD FROM
24-INCH STOP VALVE.



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Plan of Mercury-Pressure-Difference Gauge.

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was made by the Ludlow Valve Manufacturing Co., of Troy, N. Y., in 1874, and has two parallel and circular cast iron disks, whose faces are about 6 inches apart and 26.5 inches in diameter. The brass rings forming the two valve seats are each 24.0625 inches in internal diameter, as stated by the manufacturers, so that, as the disks are raised up, a lune-shaped orifice (governed by the two diameters mentioned) is presented for the discharge of the water. The disks are moved by means of a screw stem having three threads per inch of length, and they overlap the valve seat on the bottom about 1 inch, whereby about three turns of the stem are required to bring the valves to a full bearing after the area of the lune has been reduced to zero in closing; and, conversely, the same number of turns is necessary in opening the valve before the lower edges of the disks coincide with the valve seats and begin to make an appreciable width of orifice of discharge. A check on the amount of overlap in the gate valve under consideration was secured at the time of the experiment by fully closing the valve, and then noting the number of turns of the screw stem required in opening before any appreciable discharge occurred, as determined by a waterphone; and by this method, also, the overlap was found to be about 1 inch, or three turns of the stem.

The area of the lune-shaped orifice of discharge for different positions of the disks, or heights of opening, was computed from the above values of the diameters as given by the manufacturers, viz.: $d_1 = 24.062$ inches, and $d_2 = 26.5$ inches, and the results are submitted in the following table, the headings of which are self-explanatory:

TABLE NO. 1—SHOWING AREAS OF LUNE-SHAPED ORIFICES IN A 24-INCH LUDLOW STOP VALVE FOR DIFFERENT HEIGHTS OF OPENING.

Number of turns of stem required to raise disks. (N)	Ratio of diameter (d_1) of full orifice to height of opening (nd_1). $n = \frac{nd_1}{d_1}$	Ratio of full orifice (A) to lune-shaped orifice (F). $m = \left(\frac{F}{A}\right)$	Area of lune shaped orifice of discharge (F) in square feet.
3	0.0	0.0000	0.00000
10.2	0.1	0.1061	0.33524
17.4	0.2	0.2328	0.73537
24.6	0.3	0.3590	1.13401
31.8	0.4	0.4815	1.52078
39	0.5	0.5984	1.89002
46.2	0.6	0.7077	2.23527
53.4	0.7	0.8070	2.54873
60.6	0.8	0.8933	2.82113
67.8	0.9	0.9614	3.03711
75	1.0	1.0000	3.15809

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By plotting the values of (m) or (F) as ordinates to the corresponding values of (n) or (N) as abscissas, a diagram can easily be constructed from which the values of the ratio (m) or the area of the lune-shaped orifice (F), for any other values of (n) or (N), may quickly be found by inspection. Such a diagram is shown on Plate XXXIX.

It may also be of interest to note the difference between the above values of the ratio (m) and those found by Weisbach in his experiments with small pipes and valves, as given in the following table:

For: $n =$	0	1/8	1/4	3/8	1/2	5/8	3/4	7/8	1.0
Above Table gives: $m =$	0.000	0.138	0.296	0.451	0.598	0.733	0.853	0.948	1.000
Weisbach " $m =$	0.000	0.159	0.315	0.466	0.609	0.740	0.856	0.948	1.000

The relation between (n) and (m) cannot be expressed analytically in convenient form, but if an approximation is deemed sufficient for any particular purpose, the following expressions, which were found after a number of trials, may be of service:

(1).....$m = 1.17n - 0.17n^6$ for the above-named 24-inch valve; and

(2).....$m = 1.26n - 0.26n^4$ for Weisbach's valve.

The valve under consideration is set on one arm of a Y-branch in the pipe conduit, and the taps for the hose attachments with the pressure-difference gauge were inserted with a tapping machine, so as to stand truly at right angles to the surface of the 24-inch cast iron pipes and without projecting into the interior thereof. The relative positions of the valve, taps, etc., is fully shown on Plate XL.

The water supply was derived by gravity from the storage reservoir through a continuous line of cast iron 24-inch pipe, about 46 219 feet in length to the valve in question, and was discharged from said valve into the distributing reservoir through about 648 feet of similar pipe, the difference in level between the two reservoirs being 112.89 feet at the time of the experiments. With the valve wide open, the pipe would accordingly have a total length (l) of about 46 867 feet, a total head (h) of 112.89 feet, and a nominal diameter (d) of 2.0 feet. The actual mean diameter of the conduit is probably a little greater than 24 inches, since a recent measurement of a few remaining pieces of the lightest class of the original lot of pipes gave a diameter of 24.84 inches, while in the heaviest class the diameter was 23.97 inches, thus indicating that the

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PLATE XXXIX.
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24-INCH STOP VALVE.

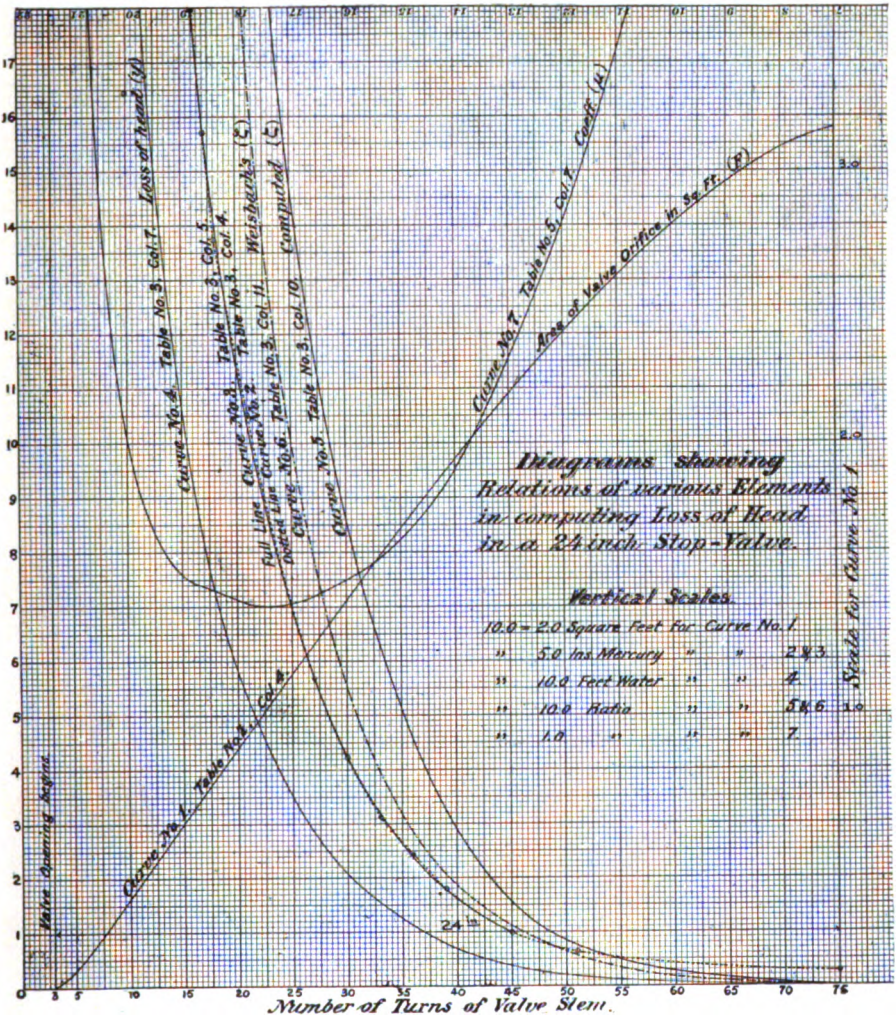

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PLATE XL.
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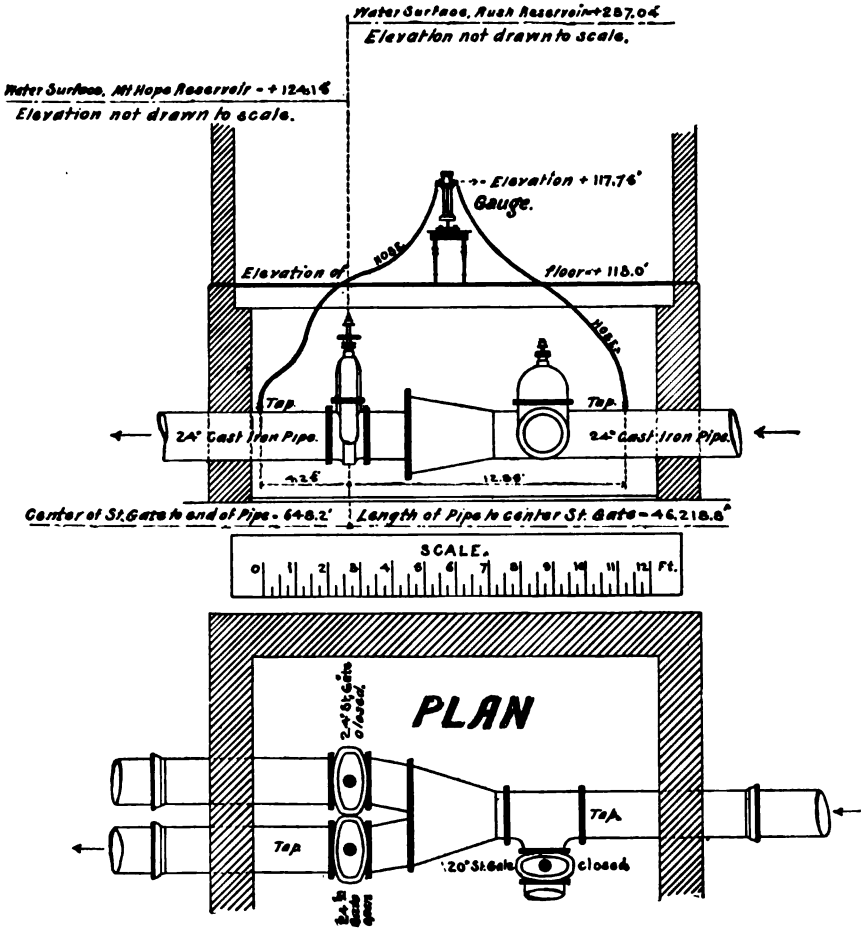
KUICHLING ON LOSS OF HEAD FROM
24-INCH STOP VALVE

Diagram showing Position of 24-inch Valve and Difference Gauge.



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specified variations in shell thickness for the different classes of 24-inch pipe used in the construction of the conduit, were, in some instances at least, effected by increasing the interior diameter above the 24 inches required for the thickest pipe. It may also be remarked that none of the heaviest class of such pipe was used between the aforesaid reservoirs. Assuming that the mean diameter of the conduit is exactly 24 inches, it was found from other recent gaugings that the discharge from said pipe may be computed approximately from the formulas:

$$(3) \ldots . v = 114.5 \sqrt{rs} = 57.25 \sqrt{\frac{d.h}{l}} = 0.37399 \sqrt{h}; \text{ and}$$

$$(4) \ldots . Q = 44.964 \sqrt{d^5 s} = 44.964 \sqrt{\frac{d^5 h}{l}} = 1.17491 \sqrt{h},$$

where (v) denotes the mean velocity in feet per second, and (Q) the discharge in cubic feet per second; also where the last expressions for (v) and (Q) result from the substitution of the above numerical values for (d) and (l). Both of these expressions will be useful for finding the velocity or discharge when the valve is partially closed, in which case it will only be necessary to substitute for (h) the difference between the total fall of 112.89 feet above mentioned and the observed loss of head in passing through the valve.

Previous to making the experiments, the mercury gauge had been carefully calibrated in the office at a temperature of about 65 degrees Fahr., the water used at the time being about 50 degrees Fahr. The results thus obtained were as follows:

Measured Head of Water in Feet.	Observed Difference in height of Mercury Columns in Inches.	Deduced Equivalent of 1 Inch of Mercury in Feet of Water.
1.0	0.955	1.0471
2.0	1.905	1.0499
3.0	2.870	1.0453
4.0	3.800	1.0526
5.0	4.765	1.0493
		Average = 1.0488

The scale was graduated into inches and tenths, and with the vernier on the sliding arm, readings could be taken directly to hundredths of an inch, while the thousandths were estimated. As any slight error of observation is relatively greater for the head of 1 foot than for the greater heads, the first observation may be rejected, whereupon the average of the remaining four observations will give a head of 1.0493

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feet of water to balance a head of 1 inch of mercury in the gauge, and this factor has accordingly been adopted in the reduction of the following observations. It may also be remarked in this connection that the glass tubes were not quite of equal bore, one being a little over and the other a little under three-eighths of an inch in diameter at each end.

After the gauge had been connected to the 24-inch pipe on each side of the valve, and properly adjusted as above described, the following observations were made: (See Table No. 2.)

It should be remarked that in attempting to test the adjustment of the gauge after the second observation, some of the mercury was blown out through carelessness, so that the experiment had to be continued with the remainder. A subsequent calibration of the gauge, however, showed no appreciable difference from the one obtained originally, and hence the above factor (1.0493) for reducing heads in inches of mercury to feet of water has been retained. The Crosby spring pressure gauge referred to in Table No. 2, is used for ascertaining the loss of head approximately, in comparison with the observed depth of water in the reservoir, and is connected with the conduit pipe independently from a tap located about 14 feet above the 24-inch valve. The records of this spring gauge are of interest here, only in so far as they show a noticeable lack of sensitiveness in the instrument for the purpose of such experiments.

TABLE NO. 2.—GIVING RECORD OF OBSERVATIONS WITH PRESSURE-DIFFERENCE GAUGE FOR VARIOUS HEIGHTS OF OPENING OF 24-INCH VALVE.

No. of turns of valve stem in opening.				Height of Mercury Column in Left Tube, as per Scale (inches).				Height of Mercury Column in Right Tube, as per Scale (inches).				Loss of head in inches of Mercury.
Total.	Estimated from valve seat.	Lift of Valve Disks above Seat, as per Indicator (inches).	Reading of Crosby Spring Pressure Gauge (pounds).	Maximum.	Estimated Average.	Minimum.	Computed Average.	Minimum	Estimated Average.	Maximum.	Computed Average.	
16	13		7.50	1.100	—	0.400	0.750	9.280	—	10.180	9.730	8.980
17	14			1.600	—	0.960	1.280	8.780	—	9.490	9.135	7.855
18	15	5	6.75	0.650	0.360	0.050	0.353	6.860	7.240	7.580	7.227	6.874
21	18	6	6.00	1.440	1.040	0.820	1.100	6.040	6.395	6.650	6.362	5.262
27	24		5.00	2.410	2.170	2.050	2.210	4.850	5.100	5.220	5.057	2.847
30	27		5.00	2.650	2.570	2.400	2.540	4.520	4.655	4.780	4.652	2.112
33	30	10	5.00	2.960	2.800	2.650	2.803	4.230	4.370	4.500	4.367	1.564
36	33		4.75	3.095	2.990	2.840	2.975	4.095	4.195	4.290	4.193	1.218
39	36		4.75	3.230	3.115	2.985	3.110	3.850	4.000	4.175	4.008	0.898
45	42	14	4.50	3.340	3.310	3.225	3.292	3.720	3.790	3.850	3.787	0.495
51	48		4.25	—	3.385	—	3.385	—	3.700	—	3.700	0.315
75	72	24	4.00	—	3.465	—	3.465	—	3.600	—	3.600	0.136

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Several hours were spent in making the observations recorded above, and much care was taken to obtain accurate results. The maximum and minimum heights in the two tubes were taken simultaneously by an observer at each tube, who adjusted one of the two slides to the level of the mercury and uttered a call when coincidence occurred. No reading of the scale was taken until at least two calls were pronounced simultaneously, and to verify the observation the observers changed places and repeated the calls. In every instance, also, several minutes were allowed to elapse after changing the position of the valve disks before making an observation, in order to allow a uniform flow to be established; and it may be remarked that any change of the disks was immediately followed by an alteration in the heights of the mercury columns, which appeared to remain constant until another change of the disks occurred.

As will be seen from the records, there was much oscillation of the mercury in the two tubes while the disks were raised only a few inches above the valve seat, and it will also be noticed that the amplitude of these oscillations gradually diminished as the opening became larger. After the disks had been fully raised, however, there was still a considerable oscillation even after a long interval of time, which may be attributed either to variations of barometric pressure at the two reservoirs, or to eddies in the water, or to both of these causes conjointly. The same oscillation was also observed, both previous and subsequent to these experiments, on applying the gauge to the same line of pipe at a locality about 6 miles distant from the distributing reservoir, and at a time when no change whatever was made in any valve or other fixture on the line. No regularity in these fluctuations could be detected, either in respect to time or magnitude, and hence the writer is inclined to ascribe them more to sudden variations in atmospheric pressure at the two basins of water 9 miles apart, than to eddies in the current flowing through the pipe.

It will be observed from Table No. 2 that even after the 24-inch valve had been fully opened, a loss of head of 0.135 inches of mercury was still indicated by the gauge. This loss is obviously due to the friction in a length of 17.10 feet of pipe and special casting, combined with changes both in direction and sectional area. To find the loss of head caused by the valve disks alone, this quantity should be deducted from the total losses of head in the last column of said table, and, as its magnitude will probably not undergo material change within the range of the experiments, it may be regarded as constant. Before making this de-

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duction, however, it will be of interest to ascertain some notion of the law governing these losses of head, and for this purpose they have been plotted on Plate XXXIX as ordinates to the corresponding degree of opening of the valve, or number of turns of the stem, as abscissas. Through most of the points thus obtained it is easy to pass a continuous and apparently regular curve by bending a flexible steel or wooden rod, and, as is usual in all such cases, it will be assumed that this curve will represent the law more correctly than the actual observations. By this means a new series of values for the losses of head in inches of mercury will be obtained, from which the aforesaid deduction of 0.135 inch for friction, etc., can be made, whereupon the reduction into feet of water, as well as other operations, may follow. In Table No. 3 the figures given in the fourth column are the observed losses of head, as in Table No. 2; those in the fifth column are obtained from the said curve drawn to a large scale, and are designated as "corrected total losses of head"; those in the sixth column are obtained by subtracting 0.135 from the figures in the preceding column, as above mentioned, and represent the probable losses of head, expressed in inches of mercury, from the valve disks alone; while the seventh column gives the same expressed in feet of water, using the factor 1.0493 as already mentioned. The remaining columns are sufficiently characterized by their headings.

TABLE No. 3.—SHOWING LOSSES OF HEAD DUE TO VALVE, TOGETHER WITH COEFFICIENT FOR COMPUTING SUCH LOSS.

Number of Turns of Valve Stem in Opening.		Ratio of diam. of full orifice (d_1) to height of opening $n = \frac{nd_1}{d_1}$	Observed Total Loss of Head in Inches of Mercury.	Corrected Total Loss of Head in Inches of Mercury.	Corrected Loss of Head due to the Valve Disks alone.		Net fall in entire Pipe Line between Reservoirs ($H-y$) feet.	Mean Velocity in Pipe from Eq. 3. (v) feet per second.	Computed Coefficient. $\zeta = \frac{2gy}{v^2}$	Weisbach's Coefficient. $\zeta_1 = \frac{2gy_1}{v_1^2}$
Total.	Estimat'd from Valve Seat				Inches of Mercury.	Feet of Water (y)				
1	2	3	4	5	6	7	8	9	10	11
16	13	1/8	8.980	8.980	8.845	9.281	103.61	3.8068	41.205	43.00
17	14	1/8	7.855	7.855	7.720	8.100	104.79	3.8284	35.557	35.00
18	15	1/8	6.874	7.000	6.865	7.203	105.69	3.8448	31.350	28.00
21	18	1/4	5.262	5.190	5.055	5.304	107.59	3.8792	22.677	17.00
27	24	1/3	2.847	2.845	2.710	2.844	110.05	3.9233	11.888	7.92
30	27	1/3	2.112	2.115	1.980	2.078	110.81	3.9368	8.626	5.52
33	30	1/3	1.564	1.595	1.460	1.532	111.36	3.9466	6.328	3.97
36	33	1/3	1.218	1.195	1.060	1.112	111.78	3.9540	4.576	2.87
39	36	1/3	0.898	0.895	0.760	0.797	112.09	3.9595	3.271	2.06
45	42	1/2	0.495	0.495	0.360	0.378	112.51	3.9669	1.545	1.11
51	48	1/2	0.315	0.315	0.180	0.189	112.70	3.9703	0.771	0.57
75	72	1	0.135	0.135	0.000	0.000	112.89	3.9736	0.000	0.00

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The numerical values of the loss of head due to the valve disks alone, as given in the seventh column of Table No. 3, may also be plotted as ordinates for the corresponding number of turns of the valve stem in the second column as abscissas, thus obtaining another curve similar to the one referred to in the foregoing paragraph. To avoid confusion in the diagram, this curve has been omitted from Plate XXXIX. It will, however, be of interest to ascertain the equation of this curve, and hence also an expression of the law governing the losses of head mentioned. For this purpose, we denote the degree of opening of the valve, as given by the number of effective turns of the stem in the second column of Table No. 3 by (x); the loss of head due to the disks alone, as per seventh column of said table by (y); the total fall in the conduit by $H = 112.89$; and the number of effective turns of the stem to fully raise the disks by $C = 72$. Since the loss of head (y) cannot under ordinary circumstances become greater than H, we have at once the first condition, that for $x = 0$, $y = H$; and as the loss of head (y) is inappreciable after the disks are fully raised, there follows the second condition, that for $x = C$, $y = 0$. To satisfy these conditions the equation must have the form $y = H + ax^m + bx^n + \ldots$. After a number of trials the writer found that the expression:

$$(5) \ldots\ldots\ldots y = 112.89 + 9.98\sqrt{x} - 84.00\sqrt[6]{x}$$

gives values for (y) which correspond fairly well with those in the seventh column of Table 3. A somewhat closer correspondence can be obtained by using more complicated fractional exponents for the terms involving (x), with other coefficients; but as the use of such factors is very tedious, only the simplest form has been given.

It is customary with the authors of modern text-books on hydraulics to cite the results obtained by Weisbach in his experiments with valves of relatively small diameter; and in order to compare the foregoing figures with the latter, Weisbach's method of expressing the loss of head (y) in terms of the mean velocity (v) in the conduit may now be applied. To find this velocity, the values of $h = H - y$ in the eighth column of Table No. 3 are to be substituted in the above equation 3, viz., $v = 0\ 37399\sqrt{h}$, whence the figures given in the ninth column of said table are obtained; and to express the relation between (y) and (v), we may place with Weisbach: $y = \zeta \frac{v^2}{2g}$, or $\zeta = \frac{2gy}{v^2}$. From Pierce's formula for the acceleration of gravity (g) we also have, for the locality of

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our experiments, where the latitude is about 43 degrees with an elevation of about 630 feet above the sea, $2g = 64.339$; and from these elements the values of (ζ) given in the tenth column of Table No. 3 have been computed. The results found by Weisbach, however, are always tabulated for a series of ratios (n) of the diameter (d_1) of the full orifice to the height (nd_1) of the lune-shaped orifice, which is somewhat different from the ratios occurring in our experiments, as given in the third column of said Table; and in order to institute a proper comparison, it is necessary to obtain Weisbach's values (ζ_1) corresponding to the latter ratios. For this purpose the values of (ζ_1) have been plotted as ordinates to the corresponding values of (n) as abscissas; and since there is a fixed relation between the ratio (n) and the number (x) of effective turns of the valve stem, it was easy to obtain from the resulting diagram the value of (ζ_1) corresponding to other values of (n) or (x). For the particular values of (n) or (x) here considered, the values of Weisbach's coefficient (ζ_1) are given in the eleventh column of Table No. 3.

The comparison of our values of the coefficient (ζ) with those given by Weisbach's experiments, shows a fair agreement only in the first three entries of Table No. 3, where the height of the lune-shaped opening is less than one-fourth of the diameter of the pipe or valve. For the remaining cases the values of (ζ) and (ζ_1) differ greatly until the valve has been raised nearly three-fourths of the whole diameter, whereupon they begin to approach each other again.

Attempts were also made to express analytically the relation of the coefficient (ζ) to the ratio (n) of the diameter (d_1) of the full valve opening to the height (nd_1) of the lune-shaped opening, but without much success. Since $\zeta = 0$ for $n = 1$, and $\zeta = \infty$ for $n = 0$, it is obvious that such relation must have the general form of—

$$\zeta = a\left(\frac{1-n}{n}\right)^s + b\left(\frac{1-n}{n}\right)^t + \ldots\ldots = aZ^s + bZ^t + \ldots\ldots$$

where for brevity we may place $Z = \frac{1-n}{n}$. The most satisfactory of the various forms tried are—for the coefficient (ζ) derived from our experiments—

$$(6)\ldots\ldots\ldots \zeta = Z^2\,(4.9354 + 0.0614\,Z^2) - Z\,(0.8284 + 0.8974\,Z^2),$$

and for the coefficients (ζ_1) from Weisbach's experiments—

$$(7)\ldots\ldots\ldots \zeta_1 = Z^2\,(2.21129 + 0.01903\,Z^2) - Z\,(0.00648 + 0.16384\,Z^2),$$

or, the simpler form:

$$(8)\ldots\ldots\ldots \zeta_1 = 0.1\sqrt{Z} + 1.98\,Z^2.$$

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To indicate how closely these equations correspond with the experimental results obtained by the writer and by Weisbach respectively, a series of computations from said equations have been carried out in the following Table No. 4. Similar agreements, but not exact ones throughout, were obtained with fractional exponents; and as the use of such expressions is too tedious, they were discarded in favor of the simpler expressions mentioned. The law governing the relation of (ζ) to (n) is probably quite complicated, as may be seen by substituting for (y) in the expression for (ζ), its value as given by equation 5, along with the value of (v) from equation 3.

TABLE No. 4.—SHOWING COMPARISON OF EXPERIMENTAL AND COMPUTED RESULTS FOR THE COEFFICIENT (ζ).

No. of Turns of valve stem in opening. Estimated from valve seat.	Ratio of Diameter of full orifice (d_1) to height of opening (nd_1) $n = \frac{nd_1}{d_1}$	Factor $Z = \frac{1-n}{n}$	Coefficient $\zeta = \frac{2gy}{v^2}$		Values of Coefficient (ζ) Computed from		
			Computed from present Experiments.	Given by Weisbach.	Eq. 6.	Eq. 7.	Eq. 8.
0	0	∞	.....	.....	∞	∞	∞
9	$\frac{1}{8}$	7	.....	*97.80	75.649	97.80	97.28
13	$\frac{13}{72}$	$\frac{59}{13}$	41.205	43.00	40.057	.....	.....
14	$\frac{7}{36}$	$\frac{29}{7}$	35.557	35.00	35.554	30.17	34.19
15	$\frac{5}{24}$	$\frac{19}{5}$	31.350	28.00	31.679	.....	.....
18	$\frac{1}{4}$	3	22.677	*17.00	22.677	17.00	17.99
24	$\frac{1}{3}$	2	11.888	7.92	11.888	.....	.....
27	$\frac{3}{8}$	$\frac{5}{3}$	8.626	*5.52	8.815	5.53	5.50
30	$\frac{5}{12}$	$\frac{7}{5}$	6.328	3.97	6.287	3.95	4.00
33	$\frac{11}{24}$	$\frac{13}{11}$	4.576	2.89	4.553	.....	.....
36	$\frac{1}{2}$	1	3.271	*2.06	3.271	2.06	2.08
42	$\frac{7}{12}$	$\frac{5}{7}$	1.545	1.11	1.717	.....	.....
45	$\frac{5}{8}$	$\frac{3}{5}$	.....	*0.81	1.094	0.76	0.79
48	$\frac{3}{4}$	$\frac{1}{3}$	0.771	0.57	0.711	.....	.....
54	$\frac{3}{4}$	$\frac{1}{3}$	.....	*0.26	0.240	0.238	0.277
63	$\frac{7}{8}$	$\frac{1}{7}$	.....	*0.07	0.019	0.044	0.077
72	1	0	.....	.....	0.000	0.00	0.000

It will be observed that the difference-gauge above described measures directly the loss of head due to the lune-shaped orifice formed by partially opening the 24-inch stop valve, and it has occurred to the writer that in cases where said loss is known the discharge might be computed directly if the coefficient of discharge for such an orifice were known. From the foregoing data it is easy to compute this coefficient for the present case, by regarding the opening as a submerged orifice discharging under the head (y), from Table No. 3, and the area (F), from Table No. 1, in which event we have the well-known expression—

$$Q = \mu F \sqrt{2gy}\,; \text{ or, } \mu = \frac{Q}{F\sqrt{2gy}}\,;$$

The figures marked with an * are given directly by Weisbach. The remaining figures in this column are derived by scale from plotted curve.

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where (Q) denotes the discharge in cubic feet per second, (F) the area of the lune in square feet, (y) the head in feet, and (μ) the required coefficient. Let us also denote by (A) the area of the complete circle formed by fully raising the gate, which, in cases like the present, is the same as the sectional area of the 24-inch pipe, and by (H) the total fall between the two reservoirs, thus leaving the effective head or fall in the conduit or pipe $= h = H - y$, with the length $= l$. The discharge of the pipe may be then represented by $Q = cA\sqrt{h}$, as in equation 4; and by substituting this latter value of (Q) in the above equation for the coefficient (μ), and combining the several constants, we obtain

$$(9)\ldots\ldots\ldots\ldots \mu = 0.046625\,\frac{A}{F}\sqrt{\frac{h}{y}} = \frac{0.14648}{F}\sqrt{\frac{h}{y}}.$$

As the values (y) and (h) are given in columns 7 and 8 of Table No. 3, for various degrees of valve opening, and the corresponding values of (F) are readily obtained either by interpolation in the fourth column of Table No. 1, or by scale from the curve of (F) on Plate XXXVIII, the values of (μ) may now be easily computed, and then plotted as ordinates in a diagram as before. The results of this computation are given in the following table:

TABLE No. 5.—SHOWING VALUES OF COEFFICIENT OF DISCHARGE WHEN THE VALVE OPENING IS CONSIDERED AS A SUBMERGED ORIFICE.

Number of Turns of valve stem in opening.		Ratio of Diameter of full orifice (d_1) to height of opening (nd_1) $n = \frac{nd_1}{d_1}$	Area of orifice (F). Square feet.	Loss of head due to valve disks alone (y). Feet.	Net fall in entire pipe line between reservoirs. $h = (H - y)$. Feet.	Computed coefficient of discharge through orifice (μ).
Total.	Estimated from valve seat.					
3	0	0	0.00000	112.890	0.00	∞*
16	13	$\frac{1}{32}$	0.65760	9.281	103.61	0.74424
17	14	$\frac{1}{16}$	0.71315	8.100	104.79	0.73876
18	15	$\frac{3}{32}$	0.768	7.203	105.69	0.73058
21	18	$\frac{1}{8}$	0.934	5.804	107.59	0.70633
27	24	$\frac{3}{16}$	1.266	2.844	110.05	0.71972
30	27	$\frac{7}{32}$	1.427	2.078	110.81	0.74957
33	30	$\frac{1}{4}$	1.584	1.532	111.36	0.78840
36	33	$\frac{9}{32}$	1.738	1.112	111.78	0.84499
39	36	$\frac{5}{16}$	1.890	0.797	112.09	0.91910
45	42	$\frac{3}{8}$	2.180	0.378	112.51	1.15920
51	48	$\frac{7}{16}$	2.446	0.189	112.70	1.46230
75	72	1	3.15809	0.000	112.89	∞

* At the limit of $n = 0$, we have from equation 9, $\mu = \infty$ or $\frac{0}{0}$, since $F = 0$. Doubtless the proper value is $\mu = \infty$, since the values of (μ) from equation 9 steadily increase as (n) decreases from $\frac{1}{32}$ toward 0. Thus, if we assume that equation 5 correctly represents the relation of (y) to the number of effective turns of opening, we will have for an opening of only four turns $y = 21.97$ feet, $h = 90.92$ feet, and $F = 0.16$ square feet, whence $\mu = 1.862$.

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From the figures in the last column of the foregoing table, the peculiar variation in the value of the coefficient (μ) is readily perceived, and can be made more apparent when these values are plotted as ordinates. While under ordinary circumstances where the discharge occurs from a higher into a lower reservoir with known difference of level, the use of the foregoing coefficient (μ) would not be desirable, yet there are often cases where it is difficult to determine correctly the value of (H), and in such cases the discharge through a throttled stop valve may be computed with fair accuracy by means of this coefficient when (F) and (y) are known. For any particular valve, the values of (F) can easily be computed, as in the present case, and by means of the mercurial difference-gauge above described, the values of (y) are readily obtained.

It is also of interest to note the irregularity in the curve for (μ) which appears in the diagram between the twelfth and nineteenth turn of the valve stem in opening, estimated from the top of the valve seat. This irregularity is obviously due to some slight errors in obtaining the loss of head with the pressure-difference gauge, owing to the considerable fluctuations of the mercury at these stages of valve opening. There is no reason why the said curve for (μ) should not be regular and continuous throughout; and the computation of the values of (μ) has accordingly proved to be not only an excellent check on the general correctness of the gauge observations, but also an indicator of the locality of possible errors in such observations.

DISCUSSION.

CLEMENS HERSCHEL, M. Am. Soc. C. E.—I did not intend to say anything on this paper, although it is one of those contributions to the *Transactions* of the Society which, to my mind, mark a distinct advance, even though it be in only a small branch of hydraulics, but still an advance, clearly and distinctly; a contribution of something new in engineering literature. It goes without saying that I am highly pleased, nay, delighted, with the paper. The treatment of the subject is a broad one; it is handled in a masterly, in an exhaustive, manner, and the results are such as may undoubtedly be used in future by others.

Comparison is made in the paper with results found by Weisbach; and I think that there is not, perhaps, sufficient emphasis given to the fact that whereas these experiments were made on a 2-foot valve, all of

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Weisbach's experiments were made on small valves, say 1, 1½ and ¾ inch valves. This might be called an experiment out in the open, in the field, while Weisbach's were laboratory experiments. Under these circumstances the differences in the results of these experiments from Weisbach's, instead of surprising any one as differing from them, should excite surprise as to their being so nearly alike with them. One thing has pleased me very much—a person always likes to see the influence of his own work showing itself in course of time in other people's writings—viz., the use of what is called the difference gauge; and finally the rating, as we may call it, of this 2-foot valve for purposes of a water meter, by observing the differences of pressure on the two sides of the valve. Some members of the Society may remember that a couple of years ago I read a paper on what was called the Venturi water meter, one that I tested in a pipe of 9 feet diameter, also in a 1-foot pipe, and have since tested it in a 4-foot and a 1-inch pipe, which meter was based on this same idea of differences of pressure; that is, gauging the quantity of water flowing through a pipe and through this meter, by observing simply the differences of pressure at two points in the meter. That meter has been sufficiently tested now to prove it a valuable instrument. In it the coefficient is a constant, within ordinary limits, whereas in Mr. Kuichling's 2-foot valve, the coefficient naturally varies between wide limits as portrayed on his curve of these coefficients.

The subject taken as a whole is, of course, only a simple thing; although we can all appreciate, that to work it out and portray it in all its parts was the labor of a good many days, and had to be done with the greatest care and discretion in order to reach reliable results. I mean when I say it is a simple thing, that, after all, it is only the rating of a 2-foot valve; but little by little the whole science of hydraulics is built up of just such simple things. I can only regret that hydraulic engineers are not more frequently in position to make experiments of this sort. I believe it was Galileo who said that it was a matter of grief to him, that he could learn more about the course of the stars and planets than he could about the flow of water right on the earth which he inhabited. What he regretted is true to this day. In the case in hand Mr. Kuichling has been luckily so placed that he has been able to give us the action of water, as it flows through a 2-foot valve, and we have, in consequence, gained that much for the cause of hydraulics.

As to the subject of pulsations in flowing water, I remember very well when Mr. D. Farrand Henry was at work on the Detroit River, being in correspondence with him at the time; and from that day to this I have often noticed these pulsations, as anybody can, who will closely observe flowing water. I am not disposed to seek the cause; it is simply a fact; the cause is probably complex, but there it is. It is inherent in all flowing water, whether it goes over a weir, through a canal or river, or through a valve in a pipe. The fact is, there is no such

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thing as uniformly flowing water, ordinarily speaking; there certainly is not, unless the water be issued at a very great velocity, and, even then, more delicate observation may reveal it. In ordinary velocities it plainly does not exist, and I have never seen any flowing water without pulsations. Of course, as to what constitutes a roughness, it may be said that the matter of roughness and smoothness is one of relative terms. If we take a canal 180 feet wide and 15 feet deep, and compare that with the roughness of a rubble wall on each side, the canal might be called a smooth channel, yet these pulsations exist in the thread of the current. I, myself, simply am inclined to the belief that they exist in all flowing water.

J. Foster Crowell, M. Am. Soc. C. E.—In Table No. 5 of the paper, which gives the effect on the velocities of different openings of the valve, there is a very significant result which shows that a 24-inch valve can be largely closed without affecting very much the discharge, and it would be interesting to trace the relations. The curves on the diagram giving the values of the table show the effect very clearly. I think I could perhaps make it out, but I fear, as I have not prepared myself, it would be rather tedious for the other members present. I will, however, call your attention generally, which the author of the paper has already done, to the remarkable variations in the last column of the table and to the considerations which they suggest. It would appear that the water flowing in the full pipe section on reaching the lune-shaped opening is subjected to momentary compression at the orifice, which imparts a great increase of velocity to the current through the orifice. Were the sides of the orifice prolonged sufficiently, the velocity would be again decreased by the greater friction; but, for the short length of the valve contraction, the vis-inertia of the moving water is sufficient to overcome the increased friction, and the water passes through in a series of impulses with scarcely diminished volume of discharge. It may be that here is the solution of the oscillations in the flow observed in the gauges.

John W. Hill, M. Am. Soc. C. E.—The facility afforded by a partially closed stop valve in a water discharge pipe, for adjusting the resistance against which pumping engines are worked during contract and other trials, is often taken advantage of to produce a head on the pumps considerably greater than the normal pressure in the mains; by means of which the pumps may be caused to work under a pressure or head of 80, 90, 100 or more pounds, while the pressure in the main outside the engine-room may be maintained at 35 or 40 pounds, or at the static head of reservoir or stand-pipe, or at the usual service pressure. As the pressure on the pumps rises or falls, the stop valve or regulating valve is cautiously opened and closed a few turns or partial turns of the stem, by an observer in charge, who takes his cue from a pair of pressure gauges; one of which is connected to the discharge pipe

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between the pumps and stop valve, and the other to the discharge pipe beyond the stop valve. Meanwhile the pumps may be worked at contract speed, or may be operated (as in direct service water-works) to meet the current demand for water.

I have made several duty trials of pumping engines in this manner, the only variable condition being the rate of discharge of the pumps while the steam pressure and head pumped against were constant; and at all other times all the conditions of pump delivery, head pumped against and steam pressure have been as nearly constant as it was possible by careful management to have them. In such cases the head in the discharge main between the stop valve and the pumps may be maintained at any desired pressure, while the head outside the engine room or beyond the stop valve, remains at the lower or usual service pressure.

But while I have used the principle of increased resistance by a partially closed stop valve in a pump discharge main to maintain a satisfactory head to pump against, it has never occurred to me that this might be employed to even approximately determine the discharge of the pumps; perhaps because of the more convenient and more reliable method generally adopted in such cases, viz., to calculate the actual displacement of the pump plungers from their net areas and travel, and allow an arbitrary loss of action or slip.

Apart from tests of pumping engines, situations may arise where it will be convenient to estimate the flow of water by means of the difference of pressures on opposite sides of a partially closed stop valve; but from present information on the subject I should be inclined to accept any such measurement as a very rough approximation indeed. If it be true that experimenters in hydraulics are not agreed upon the coefficients of resistance and discharge for the orifices of regular shape (round, square and rectangular), then how can we hope to reconcile the discrepancies which will arise when we attempt measurements of flow through variable lune-shaped orifices, with their coefficients of resistance and efflux changing with every change in the relative size of the orifice? and how will we apply an expression for discharge which may be obtained from experiments with a 24-inch Ludlow valve, to valves from the dozen or more other makers, or to valves of other sizes from the same maker, without special tests with each, which tests in themselves will generally dispose of the main question, viz., the determination of the discharge in a particular case.

The experiments of Mr. Kuichling are quite interesting, especially when the results are compared with those from Weisbach, with his simple gate valve, the differences in $\frac{F}{A}$, and in the coefficient of resistance (?) being accounted for, no doubt, by the difference in the forms and proportions of the Ludlow double disk and Weisbach simple sliding-

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gate valve; and these differences serve to emphasize the difficulty surrounding attempts at estimating the flow of water through pipes by means of constricted lune-shaped valve orifices.

It is doubtful if the experiments have any practical value, because if it be required to measure the flow of water in a line of pipe of known diameter, it can be done more conveniently by means of accurate piezometers placed a measured distance apart and adjusted to a common datum plane; the difference of heads at the up and down stream stations (gauges) and diameter and character of wet perimeter of the pipe furnish all the necessary data for the use of Chezy or Kutter formula. This, of course, in situations where it is impossible or inconvenient to measure the discharge or flow by wier, or in tanks, or by reservoir.

JOHN THOMSON, M. Am. Soc. C. E.—In regard to the gauge illustrated in the paper by Mr. Kuichling, it is an instrument which ought to be in the hands of every engineer having to do with hydraulic operations. Its sensitiveness and yet relatively high range of reading, together with its uniformly exact indications, would render it invaluable in many places. For instance, if applied to a pump, a meter or a steam engine, any faulty throttling through the valves or channels could at once be detected. I am not fully persuaded, however, that the slip gauges and vernier as devised by Mr. Kuichling are the best arrangements that could be applied for reading the height of the columns. I have made a drawing of a differential mercury gauge intended for my own use in which a screw is disposed between the glass tubes, acting as a column to bind the head and base together. The screw is to be of 0.1 inch pitch, upon which a nut will be mounted, graduated to read in 0.01 and 0.005 inches. I am also of the opinion that steel indicators, floated upon the mercury, having sharply defined nicks or cross-lines, would be an assistance to accurate and ready reading. Such floats would require to be Bower-Barffed to resist corrosion, as they would be partially immersed in water. But these criticisms of detail are not to be taken as detractive of the gauge as a whole, the performance of which was highly satisfactory.

In view of a recent discussion in which I took part involving the matter of spring gauges as instruments of precision, particular attention is called to Mr. Kuichling's Table No. 2 in so far as the indications of the mercury columns are comparable to those of the spring gauge. Thus, in one case the spring gauge indicated a difference of 1 pound while the difference gauge denoted over 2 pounds; in another instance, the spring gauge showed no difference, while the actual change in condition amounted to nearly 1.35 pounds. As to the accuracy of the record there can be but little doubt. There was no chance for "argument" between the observers during the instant of any reading, as to whether, carpenter-like, it was "full" or "scant." The attention of each being intently

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and solely directed to one column, even unconscious cerebration could not assist in improperly bringing the personal equation of the observers into unison. Moreover, after the complete series of observations had been concluded, the valve was reset to several known positions and the gauge again read. In all of such instances the observations were practically identical, and this was regarded as fair proof that the condition of the line, the valve, the gauge and the observers had not changed their rating.

I do not concur with Mr. Kuichling's opinion as to the cause of the oscillations of the mercurial columns. As will be seen by inspection of the drawings, the "legs" of the gauge were attached to the top of the conduit. Now, I take it that entrained air in a pipe-line travels at the top, being rolled along at a rate of speed probably less than that of the maximum velocity of flow. But, irrespective of the rate of travel, should even a globule of air pass the piezometric openings leading to the gauge, it would tend to disturb the *static* conditions which obtain thereat. And the fact that these fluctuations increased with the depression of the valve, in other words, were in a measure proportionate to the velocity of flow through the lune-shaped orifice, would seem to bear out this theory, in that any air driven down toward and thence through the lune-shaped opening would again ascend and impinge at a relatively high velocity against the upper surface of the pipe. If an experiment of this kind were to be duplicated, I would therefore suggest that the legs of the gauge be connected at the side or bottom of the pipe; or, better still, in the manner described by John R. Freeman, M. Am. Soc. C. E., in his paper on the hydraulics of fire streams, in which a circumferential channel is employed having a series of ducts communicating with the interior of the pipe. My present point may be illustrated by supposing that if Mr. Kuichling—instead of amply spreading the legs of his gauge, as he properly did—had connected them close up to opposite sides of the valve, then, in such an event, the suction of the discharge past the valve-disks would undoubtedly produce a negative effect somewhat proportionate to the increase of velocity at the orifice and a false conclusion might then have been deduced.

While Mr. Kuichling has chosen to treat his subject analytically, the practical importance of his work and its demonstration ought not to be left for doubtful recognition. It is, of course, to be expected that those, more or less familiar with throttling diaphragms and venturi-nozzles, have been aware of their relatively high discharging capacity when inserted in tubes or pipes of greater diameter, because such a condition is to the entire flow for an instant the friction of a point, so to speak, merging pressure into velocity. Nevertheless, speaking for myself, although having some reason to anticipate the results which Mr. Kuichling has presented, I question if I should have had the temerity,

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without the assurance of this demonstration, to suggest placing in a force main of 2 feet diameter stop valves of, say, 1½ or 1¼ feet diameter. And yet this, in my judgment, is what with perfect propriety might be done in analogous instances; as such an introduction, without resulting in any practical diminution of the discharging capacity of the line, would not only reduce the cost, but would greatly increase the ease and celerity with which the valves could be operated.

It is not intended by this to, in the slightest degree, disparage the ordinary frictional losses known to occur in flowing water; rather let it be taken as an exemplification thereof. In the *Engineering News* there was published some time past, several excerpts from the notes of the late W. J. McAlpine, in which occurred in effect this statement, which is worthy of record as a theorem: "As nature abhors a vacuum, so does water abhor an angle." But the action of a constricted opening, disposed transversely to the path of the current, results only in a simple deflection of a *portion* of the mass, and well illustrates the persistency to flow in a straight line, the abhorrence of an angle. Collateral to this it may be of interest to briefly describe a simple experiment, which I recently tried with the view of clearly fixing this fact in my own mind through the medium of ocular demonstration. A short piece of pliant rubber tubing, about three-fourths inch inside diameter, was connected to an ordinary faucet. The pipe was then curved to approximately a half circle, a small brass tube about one-fourth inch diameter was inserted so as to point towards the centre of said circular curve, and also another on the opposite side of the pipe projecting radially outward. The small tubes were, in fact, horizontal piezometers to the main section of pliant tubing. With the rubber tube discharging a full stream, the external piezometer would also discharge a full stream, but no flow would take place from the internal piezometer. This indicated no pressure upon the inner wall of the tube. The piezometers were then removed, and a section, about 2 inches long by, say, one-half inch wide, was cut from the inner wall of the loop, when the water would flow past the opening thus formed in a perfect stream, crystal-like and unbroken in contour. Obviously, the tangential tendency or persistency was entirely resisted by the exterior wall. It will, of course, be understood that the flow as just described would not continue at high velocities. From this the query naturally arises, what would be the probable difference in discharging capacity, supposing a valve to be situated in a circular loop, and so disposed that its disk would cut the flow from the exterior or from the interior side thereof?

In respect of Mr. Kuichling's reference to me as a co-acting observer, his close adherence to grammatical etiquette gives me undue honor: for in this work I was in nowise a principal, but was rather as "a student at the shrine of sages." Mr. Kuichling's paper is a distinct scientific addition to the subject of which it treats, and I am sufficiently honored in

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having been the means of persuading him to prepare it. After the conclusion of the experiment, which consumed nearly all of a bright, pleasant Sunday, with kodak in hand I went up the bank of Mount Hope Reservoir, Rochester, N. Y., near where the gauge had been applied, and, turning quickly, caught a "snap shot" of Mr. Kuichling as he was mounting the stairs. The smile of satisfaction which yet lingered upon his features, filled the "field" of my instrument. He said that mine spilled over the bank. Possibly it did, for I had never taken part in a more interesting, instructive and successful demonstration, and my satisfaction at the time could not have been indicated in inches of mercury.

JOHN C. TRAUTWINE, JR.—As to the cause of the oscillation observed in the heights of the mercury columns under a given opening of the valve, it is evidently, as our author intimates, a matter of individual opinion how much of these oscillations was due to the partial obstruction at the valve, and how much to variations in the barometric pressure upon one or both of the reservoirs. For my own part, my labors with Kutter's coefficient "n" of roughness have left me with a lively (perhaps exaggerated) sense of the disturbances caused by even very slight irregularities in the pipe; and I am therefore disposed to look rather to the obstructions at the valve, than (with the author) to barometric changes, for the cause of even the remaining slight oscillations observed in the mercury columns after the valve was fully opened. Even in the smoothest and straightest pipes and channels, the particles of water next to the walls must move very much slower than those nearer to the axis of the pipe, and this difference of velocity must produce lateral currents and eddies which cannot but cause variations in the piezometric pressure at a given opening in the pipe. Now the positive though partial obstruction caused by even a fully opened valve, must greatly aggravate this disturbance, and it seems to me that we ought to expect, at openings placed near such an obstruction, just such oscillations as are here recorded, independently of possible variations in the air pressures at the reservoirs. As remarked by the author, the oscillations, of course, decreased rapidly and almost uniformly (see Table No. 2) as the obstruction was diminished by the progressive opening of the valve; and it can hardly be doubted that a still further reduction of the oscillation would have been observed if the valve could have been removed altogether and a smooth length of pipe substituted for it. It will also be noticed that as a rule the oscillations were greater at the lower than in the upper gauge tube, as might be expected of oscillations due to the obstruction. It is true, however, that this distinction is much less marked, and therefore less conclusive, with wider than with narrower openings of the valve.

The author states that "the same (query, equal?) oscillation was observed * * * on applying the gauge to the same line of pipe at a locality about 6 miles from the distributing reservoir, and at a time

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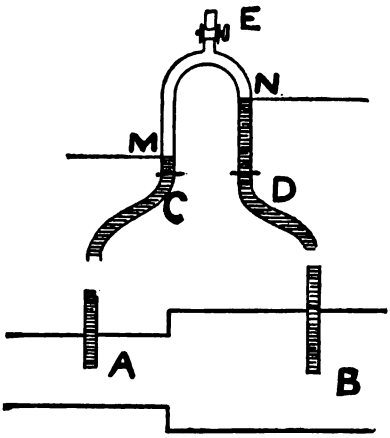
when no change whatever was made in any valve or other fixture on the line"; but we are not told whether in this case there was a valve or other partial obstruction between the two piezometric openings. And, even if there were none, it seems to me it might still be a matter of conjecture whether the oscillations were due chiefly to barometric changes, or to those eddying currents which must exist even in practically "smooth" pipes. It would be interesting, in this connection, to have the results of similar experiments accompanied by careful and simultaneous barometric records taken at the two reservoirs.

We are not informed as to the time during which the oscillations took place; but, as the entire series of observations was made within "several hours," it may properly be asked whether the time occupied by the separate oscillations was sufficient to render admissible the assumption of the necessary barometric changes at the reservoirs. In the absence of the plates referred to, which no doubt show the relative elevations of the valve and of the lower reservoir, we may ask also whether and how it was known that the pipe just below the valve ran constantly full, especially under the smaller openings recorded. I am advised that the reason for the absence of records of experiments with openings of less than thirteen turns, is that the limit of the gauge was reached at that point.

R. FLETCHER, Assoc. Am. Soc. C. E.—Collignon* shows an ideal sketch of a form of differential piezometer (see Fig.) devised by Bélanger, which he describes substantially as follows: Small differences of piezometric columns may be obtained by joining together two small pipes fixed in the conduit, one at *A*, the other at *B*. These are terminated at *C* and *D* by a single curved glass tube, *CED*, at the summit of which an opening, *E*, is provided with a stop cock. The water mounts in the piezometer and compresses the contained air. The stop cock is opened and controlled so as to allow a part of the contained air to escape until the water appears in both branches of the glass tube, when it is arrested, as at *M*, in one branch, and at *N* in the other. The difference of level between *M* and *N* measures by liquid column the difference of pressure sought.



The form thus described has the advantage of affording a differential

* "Cours de Mécanique Appliquée aux Constructions, 2me Partie, Hydraulique." M. Edouard Collignon, Paris, 1870.

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piezometric column more than twelve times as long as the corresponding mercury column, and may be useful for measuring small differences of pressure if the conditions are not such as to cause extreme or long-continued oscillations. It has the further advantage of simplicity in detail and small cost. But the range of its adaptation is limited, as the use of a differential water column more than 3 or 4 feet high would be generally inconvenient. It may be presumed, also, that under pressures exceeding 80 to 100 feet, with a long line of pipe, the oscillations would be very troublesome.

It would appear that the oscillations observed by Mr. Kuichling may well be due to the recognized and unexplained irregularities in the flow of water under conditions calculated to secure the highest degree of uniformity. An assumption of sudden variations in atmospheric pressure would need strong confirmation from simultaneous barometric observations. Mr. Kuichling's valuable and interesting results are an important addition to our stock of hydraulic data.

GEORGE W. RAFTER, M. Am. Soc. C. E.—Mr. Kuichling's paper brings to the attention of hydraulicians an interesting, and to some extent important, line of experimentation, more especially because the stop valves used were much larger than those used by Weisbach, who, thus far, is the only experimenter who has published results of this character. The analytical portion of the paper may, moreover, be denominated elegant, by reason of the ingenious treatment which the purely theoretical side of the subject has received. In reference to the appliances used and the deductions from the observations, the paper, however, admits of criticism; and there are a number of important discrepancies which it is hoped the author of the paper will clear up in his final discussion.

The method of calibration, by reason of apparently eliminating the error due, not only to the impurity of the mercury, but to variations in diameter of the tube, is ingenious, and its author should receive especial credit for it. On this point, however, some confusion apparently exists, as Mr. Kuichling refers to a graduation of his tube in inches and tenths. If such a graduation was actually made and used, the error due to change in diameter of the tubes is not eliminated and it would vitiate the result. Following this thought further, it may be noted that the equivalent of an inch of mercury is given as 1.0493 feet of water, or 1 foot of mercury balances 12.59 feet of water, that is to say, the specific gravity of the mercury used was, for the actual temperature and elevation, 12.59, instead of about 13.55, as it should be for pure mercury. It is hoped that Mr. Kuichling will explain whether the apparent discrepancy here is due to impurity of the mercury or to variation in size of his tubes, as upon this point will hinge very largely the real utility of his experiments.

Again, it may be noted, the record shows a loss of 0.135 inch of

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mercury. Even after the gate is entirely open, this loss taking place according to the paper in a length of main of 17.1 feet (which in a length of 1 000 feet, the ordinary unit for statement of hydraulic gradient) amounts to a loss of 8.28 feet. At the time of making the experiments, the total fall between the two reservoirs, connected by the 24-inch main, in which the stop gate experimented upon occurs, was 112.89 feet, giving, for a total length of connecting main of 46 867 feet, a rate of gradient of 2.41 feet per 1 000. There is, however, a slight bend in the special *Y* branch included between the two points of attachment of the apparatus, but the small amount of deviation from a right line occurring here cannot be taken as explaining the large value of the deduced gradient of 8.28 per 1 000 feet for full discharge. In my opinion, this fact alone should make us very cautious in accepting these results as otherwise than accidentally correct. This remark is further emphasized by the wide variation in value of the coefficients given in Column 10 of Table No. 3, in comparison with those of Weisbach, in Column 11.

Again, the propriety of selecting the 24-inch gate of the Mount Hope Reservoir gate-house for these experiments may be fairly questioned, in view of the possibility of the disturbing influence of the special; which, while an exceedingly small quantity with full gate and its consequent low velocity of a little less than 4 feet per second, is nevertheless an important factor with a partially throttled gate and the resulting high velocity through the same. The location selected admitted conveniently of a distance of only 17.1 feet between points of attachment for the difference-gauge apparatus, a distance which the great fluctuations in the earlier stages of the experiments shows to be entirely insufficient for accurate work. Moreover, there are at least two points on the same pipe line where the experiments could be made and a stretch of nearly straight pipe of several hundred feet on either side of the gate obtained. A statement of the reasons in detail, by Mr. Kuichling, of why this particular gate was selected, in view of the obvious objections in preference to either of the others, will also assist greatly in determining the real utility of his experiments.

These criticisms are advanced with the hope that, inasmuch as his results are so widely different from those of Weisbach, which many hydraulicians have been in the habit of considering classic, Mr. Kuichling may be able to clear up the dark points in reference to them which now apparently exist.

D. Farrand Henry, M. Am. Soc. C. E.—Mr. Kuichling's paper on the loss of head through a 24-inch valve interested me very much, and I think his observations and deductions will be useful in the future. But when he speaks of the continuous pulsation of the mercury in his gauge when the valve was fully opened and the current normal, he touches a strange and to me yet a mysterious property of flowing water. These fluctuations in the strength or velocity of the current were

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observed by M. Darcy when using a Pitot's tube—an instrument very similar to Mr. Kuichling's gauge—but they were never measured until during the measurement of the outflow of the great lakes. I invented and used the telegraphic current meter—as noticed in my pamphlet on the "Flow of Water in Rivers and Canals," and the lake survey reports. When the meter was first placed in the current the most noticeable thing was the marked irregularity in the beat of the sounder—each "click" representing one revolution of the submerged wheel—at times slow and stately, and again with a buzz almost continuous. Wherever I placed my meter in these rivers, in the Chicago Water Works tunnel or in the tail race of a mill, the same changeableness in the correct velocity was manifest. These pulsations of the current were greater at the bottom and least at the surface, but without regularity as to time or force, the common maxima being from five to perhaps twenty seconds apart, while every ten or fifteen minutes would come a rush of the current which would set the sounder buzzing. I noticed the same thing at Niagara. Standing at the base of the Fall, or under the sheet, when the principal maxima came the earth would tremble under one's feet, and in the whirlpool rapids the waters would clash together and the center waves seem to rise above the level of the eyes. I have some notes also of the survey for the Cuba cable, which show that these pulsations also affect the flow of that wonderful current—the Gulf stream. In fact, what we term "uniform flow" of water seems almost as unstable as the motion of the wind. To measure these fluctuations I placed a Morse paper register in the circuit, each revolution of the current wheel making a dot on the paper, and I kept a few of these paper slips, which I enclose, as they may be of interest, being probably the only autographic record extant of the current of a great river.* As the Morse register was found to be rather irregular in its action, depending perhaps upon the thickness of the paper passing through the rolls, which thus retarded it more or less, I had the observer make a pencil mark across the paper every half minute as it passed the stylus. They were taken in the St. Clair River in 1868, but the notes of the survey are buried in the archives of the office of the Engineer Bureau at Washington, so I cannot give their position in the river, but most of them are evidently in the deeper portion of the river, while one set (September 16th) was taken near shore, where the water was not over 5 or 6 feet in depth. I tabulated hundreds of these records, but was never able to establish any law of pulsation.

Several years subsequently, when chief engineer of the water works at Detroit, Mich., I placed a glass mercury gauge in an upper room in the office, which was high enough not to make the tube inconveniently long, and I used to watch it at night when everything was quiet, when few taps

*The paper slips here referred to were exhibited when the paper was read, but it is impracticable to publish them here.

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would be opened, and the regular night use and the waste was all the demand on the supply. At such times I would turn the full pressure on the gauge, and the mercury would commence slowly sweeping up and down the tube, the fluctuations being ordinarily from 3 to 6 inches every quarter of a minute or less, but rising often to 8 inches or more. This would keep up as long as I cared to watch the pulsation. The fluctuations noted by Mr. Kuichling might therefore have been due to this curious and as yet unexplained instability of the velocity in flowing water.

JAMES DUANE, M. Am. Soc. C. E.—In reading Mr. Kuichling's paper one is struck most forcibly by the great waste of algebraic ingenuity shown in the construction of formulas which the author admits are of but doubtful utility in his special case—especially as they would not be of general applicability. In fact, the whole question is one for experimental solution only, and is not susceptible of satisfactory analytical investigation. The pith of the entire article seems to be contained in the last column of the last table, where the coefficients of discharge are given. That these are given to the fifth place of decimals seems an unnecessary refinement, as these values probably contain errors in the second place of decimals at least. Further, while we are willing to admit that the coefficient of discharge through the throttled valve when half open may approximate to unity, it is difficult to see how, for any ratio of opening whatever, it can ever exceed unity.

In filling sections on 36 and 48-inch pipe lines, the writer has had opportunities of approximately determining the coefficient of discharge for different degrees of valve opening under quite a range of heads. The method of making these observations was as follows: the new pipe lines to be filled had blow-offs in the hollows, and, as is usual with us, fire hydrants placed at the summits acted as air cocks. When a section of the pipe line was to be filled, the blow-offs were first opened to insure the emptying of the pipe, so that we knew just how many cubic feet were necessary to fill that section. The blow-offs were then closed. The filling was done from a 6 or 12-inch branch supplied from the general distribution, and as near the summit as possible, so as to insure a light and practically uniform back pressure. The men stationed at the air cocks were instructed to close them as soon as the water showed, so that there was no waste at these points. The time of turning on the water and of its appearance at each air cock was recorded.

In each case the feeding gates were opened a certain number of turns before being set, and a roughly shaped piece of card-board placed against the valve, and the lune-shaped opening carefully traced thereon. The area of this tracing was determined by a planimeter, and is believed to have been correct within very close limits. The vertical distance between the point where the feed pipe became completely filled below the gate and the center of a spring pressure-gauge located just above

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the feeding gate was measured, and the pressure was determined from time to time during the operations of filling. The employment of the spring pressure-gauge is the weak link in the experimental chain. We have never been able to obtain spring gauges that could be regarded as instruments of precision, their employment usually resulting in errors of 2 or 3 per cent. at least.

Two examples covering about the greatest range of head that occurred in filling a 36-inch line recently are appended.

1*st*. 6-inch gate open 1-inch; area = .035 square foot; head, 12.1 feet. Q = 34.360 cubic feet; time, thirteen hours; coefficient = 0.75.

2*d*. 12-inch gate open 1-inch; area = .058 square foot; head, 52.0 feet. Q = 39 100 cubic feet; time, four hours thirty minutes; coefficient, 0.75.

It will be seen that these coefficients agree fairly well with those deduced by Mr. Kuichling for about the same amplitude of valve opening.

E. KUICHLING, M. Am. Soc. C. E.—The discussion appears to be confined to comments on the observed oscillations of the piezometrical mercury columns, the details of the pressure-difference gauge, and the practical value of the results of the experiments made.

The remarks of Messrs. Herschel, Henry and Thomson amply corroborate the existence of the oscillations referred to, but still leave a satisfactory explanation of their cause in doubt. While eddies or internal disturbances of flow may account for much of the fluctation, yet the effect of momentary differences of barometric pressure at the two reservoirs, nine miles apart, cannot safely be ignored; and that such differences or rapid variations in the pressure of the atmosphere at any given place do occur, is well known to all who have attempted to do leveling with either a mercurial or an aneroid barometer. During the summer of 1891, the writer made hundreds of observations for obtaining topography on the water-shed of Hemlock Lake, New York, with a delicate aneroid having a range of only about 4 inches and a diameter of dial of 5 inches; and he frequently observed sudden variations in the pressure, amounting at times to several hundredths of an inch (from 10 to 40 feet of elevation), which lasted only a few minutes, or even seconds. On all these occasions the instrument was at perfect rest and observed from the same position. A sharp watch was kept, particularly for the purpose of detecting such atmospheric waves, and they were found in all kinds of places and at nearly all hours of the day, but more frequently about noon. Through these observations, as well as many others since made, the writer is led to believe that such momentary waves may account for some of the mercurial fluctuations.

A similar phenomenon was noticed in observing the rise of water in a large reservoir, about 1 600 feet long, 400 feet wide and 14 feet deep, which was receiving near one end the uniform discharge of a gravity

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conduit at the rate of nearly 11 cubic feet per second, the outlet having been tightly closed. The observations were made near the middle of the long side of the reservoir in a cistern of 6-inch iron pipe, set rigidly so as to project about 2 feet into the water, and provided with a hole near the bottom only one-fourth of an inch in diameter. With little or no wind at the time, a slight oscillation of the water surface, without any apparent regularity or uniformity, was always detected with a hook-gauge; the amplitude of these waves in the cistern ranging from 0.05 to 0.20 inch at irregular intervals varying from one to three minutes.

With reference to the remarks of Mr. Trautwine, it may be stated that the pressure-difference gauge described was applied several times to a certain straight and clear section of the 24-inch cast-iron pipe connecting the two reservoirs mentioned, the gauge being placed midway between two taps inserted in the top of said pipe about 1 000 feet apart, and coupled thereto with ¼-inch wrought-iron pipe. The locality was on a plateau about three miles from the upper reservoir, and an air-valve near the gauge showed that the water was free from entrained air. After the gauge had been properly adjusted, the fluctuations took place as described, both with the conduit in full operation from the upper to the lower reservoir, and with the lower portion of the conduit shut off by an intermediate stop-valve, while the discharge from a 6-inch blow-off was being carefully measured over a suitable weir. The purpose of the experiments was to ascertain with the gauge the actual loss of head on a measured length of said conduit, for known discharges, and thence to deduce the corresponding coefficients of friction. It should also be remarked that the blow-off was located in a depression about one-half mile below the gauge, and that the fluctuations of the mercury were very frequent and large on all occasions.

On the other hand, during a similar application of the gauge, late last year, to various portions of the ten-mile section of 24-inch pipe conduit between Hemlock Lake and the aforesaid upper reservoir, the fluctuations were found to be very slight, ranging from only 0.02 to 0.03 inch at intervals of two to four minutes, while during these intervals the mercury remained nearly stationary. It is therefore seen that on long, clear and comparatively straight sections of 24-inch pipe, free from entrained air, and discharging at the rate of about 11 cubic feet per second, we may have at one time extensive fluctuations of the mercury, while at another time the fluctuations are almost imperceptible. For this circumstance the most reasonable explanation seems to be, the presence or absence of differences of barometric pressure at the communicating bodies of water.

The theory of Mr. Thomson, that the fluctuations are largely due to entrained air, is open to the criticism that the existence of a bubble of compressed air at the top of the main pipe, or even in the tube leading to the difference-gauge, cannot affect the results, since differences of pres-

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sure are indicated by the instrument, and because the assumed globule of air must have the same pressure as the adjacent molecule of water. In the case of the escape of large entrained air bubbles from a submerged pipe, it is probable that a series of shocks would occur which would be indicated by fluctuations of pressure; but, in the experiments described, no appearance of any such air bubbles at the outlet in the reservoir was detected. The suggestion of Mr. Thomson to make use of a circumferential pressure-channel around the conduit, instead of attaching the gauge to single taps at or near the top of the pipe, is good, although in the case of large pipes the application of such a device in the field is somewhat difficult.

It is also to be remarked, in response to the comments of Mr. Trautwine, that the difference in the magnitude of the simultaneous oscillations in the two legs or tubes of the gauge is due to the difference in diameter of the said tubes, since the volume of mercury displaced or transferred from one leg to the other for a given difference of pressure must be the same. Were the two tubes exactly alike in diameter, the rise of mercury in the one would be precisely equal to the fall in the other. Greater differences of pressure between the points where the taps are inserted in the pipe are naturally attended with greater variations of head of mercury, and also with relatively greater fluctuations or oscillations as exhibited in the table. The 24-inch pipe behind the valve experimented with was kept full by the back-pressure from the reservoir, as will be seen from the drawing.

With regard to the construction of the instrument, the writer has no doubt as to its capability of improvement. From the experience with the apparatus depicted, it was found that the slides for the arms or tubes must admit of quick motion and be set rapidly, as the mercury seldom remains quiet; and the question therefore arises, whether a greater refinement of measurement than the limit of error in setting is necessary. Practically, the slides can be set to within 0.01 inch, and it therefore seemed that a vernier scale reading to this limit was sufficiently accurate for measuring the mercury head or distance between the two slides. The introduction of suitable floats or pointers on the mercury would, doubtless, be an improvement; but, in this event, care must be taken to have both the interior of the instrument and the mercury perfectly clean, otherwise a slight impurity lodging between the float and the glass tube might cause a failure of correct indication.

The method of calibrating the gauge, as described in the paper, is conceded by Mr. Rafter to be correct, but still he seems to be dissatisfied with it in some particulars. By actually measuring the head of water in feet, and with uniform graduations of inches on the scale of the gauge, the errors due to differences of diameter of the tubes at different levels are duly exhibited in the paper, and are seen to be so very slight (the average differing only a few thousandths of an inch from the ex-

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treme), that it was considered justifiable to make use of the mean value of a head of 1 inch of mercury thus found, in terms of feet of water. For the fact that 1 inch of this mercury balanced 12.59 inches of the same water which was used in the experiments, instead of 13.55 inches, the writer does not venture now to offer an explanation. The metal was procured of a reputable dealer and supposed to be pure. The results obtained are, however, in no wise vitiated, as Mr. Rafter appears to think, since the comparisons are given throughout for the equivalent feet of water. Any other experimenter with stop valves may therefore easily compare his work with that of the writer by using heads in feet instead of in inches of mercury.

In relation to the practical value of the experiments little need be said, since the estimate of such work is necessarily governed to a large extent by the experience in practical hydraulics of the person making it. Without impugning in the slightest degree the skill of any one who fails to discover any utility in such work, it seems to the writer that a computation of discharge through any orifice based upon the use of a well-established coefficient, is preferable to one which is based upon the displacement of a pump piston or plunger, whose actual loss of action, or slip, is entirely unknown. Again, in the construction of a large pipe conduit line, it may be very desirable to know whether the use of valves which are smaller in diameter than the pipe itself is admissible. From the coefficients obtained, it now becomes possible to compute with close approximation the resulting loss of head involved in such use of smaller valves. Neither is it necessary to make a rating of each particular valve, unless great accuracy is required, since Mr. Duane has pointed out in his remarks a close agreement in the values of the coefficients for a 6, 12 and 24-inch valve for about the same amplitude of opening. In ascertaining the delivery of a conduit, also, whose exact diameter and internal condition are unknown, but in which a duplicate stop-valve is available, the above results are directly applicable with much greater presumption of accuracy than would be secured by the use of the Chezy or Kutter formula, as proposed by Mr. Hill. It is not enough to merely measure the loss of head between a number of points in the pipe line, and then assume the coefficient of friction and the diameter, since both of these elements are important factors in the discharge. The results of such a computation with an old line of cast-iron pipe, whose components were made at several different foundries, might easily differ ten per cent., or even more, from the truth.

A few words as to the "waste of algebraic ingenuity shown in the construction of the formula," may also be permitted. Such formulas serve to express the laws underlying the flow of water through orifices, etc., much better and more compactly than tables, diagrams, or statements; and it should be the aim of every engineer, or experimenter to find these laws from the facts at hand. To simply publish a mass of

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undigested observations may be good for the printer, but their perusal becomes a source of intense exasperation to a busy professional man who wishes to apply the principle to a somewhat different case. If such work has no other value, it at least serves to point out to other seekers after truth what labor to avoid, and possibly also what lines of further examination to follow. Efforts of the kind referred to cannot therefore be called wasted, but may rather be termed sign-posts on the perplexing road to knowledge, whose multiplication should rather be encouraged than discouraged.

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THE CONTINUOUS GIRDER AS A TIPPER.

By C. H. LINDENBERGER, Assoc. Am. Soc. C. E.

The problem of finding the strains on the Tipper has been pretty thoroughly discussed by Mr. Clemens Herschel, M. Am. Soc. C. E., years ago. Nevertheless, the formulas obtained by his method are long and tedious in their application, and later writers like Professor Howe and Professor Du Bois only indicate the general manner of solution and do not give equations that can be readily used in practice, especially in complex cases such as a variable moment of inertia, loads varying in amount in all the spans, etc.

It may interest the members of this society if a method is demonstrated by which any one who can deduce the moments at the supports for the comparatively simple case of "supports on a level," can immediately write the equations for the moments for the Tipper, for the form in which it usually occurs in practice. The analysis is new and the method quite different from those of previous authors.

The Tipper is a continuous girder resting on four supports, but is distinguished by the fact that its two middle ones rest on a bar or

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unyielding framework and this again is supported at its center. The accompanying figure will explain it.

The first support is numbered 1 and also the first span. P_n is a weight on the nth span whose length is l_n and a_n is its distance from the left support. M_n is the moment at the nth support, and will be considered positive when there is tension on the upper chord or flange. h_n is the distance that the nth support lies below a given axis of reference. R_n is the reaction at the nth support and is positive when it acts upward.

The only case that need be discussed in this paper is that where the axis of reference is a line passing through all four of the supports which are on a level for no load on the girder, then evidently $h_2 = -h_3 = h$. Also the reactions at the middle supports are necessarily equal.

We will suppose three cases:

First, that the supports are on a level and are rigid and unyielding and let us call the moments M'_2 and M'_3 and the middle reactions R'_2 and R'_3, the girder being loaded in any manner whatever.

Second, let all the load be removed and let one of the middle supports sink a certain distance $= h$ and the other rise an equal amount. Let us call the moments M''_2 and M''_3 and the reactions R''_2 and R''_3. The supports are to be rigid and unyielding as before.

Third, let the previous load be again placed upon the girder and let us designate the moments by M_2 and M_3 and the two middle reactions by R_2 and R_3. It is evident that these are due to the combination of the effects due to the two previous cases, or in other words,

$$M_2 = M'_2 + M''_2 \qquad M_3 = M'_3 + M''_3$$
$$R_2 = R'_2 + R''_2 \qquad R_3 = R'_3 + R''_3.$$

The difficulty which suggests itself is that we do not know what the proper deflection is, but it will be shown that it is only necessary to assume that it is the proper one for the given loading.

Now, in the second case, since the deflection of each of the middle supports is equal in amount and opposite in direction, the moments are proportional to the amount of that deflection (since there is no load upon the girder). In other words, we have

$$M''_2 = F_2 h \ldots\ldots (1)$$
$$M''_3 = -F_3 h \ldots\ldots (2)$$

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where F_2 and F_3 are constants for the particular girder. Also, since the values of R_2 and R_3 must be equal,

$$R'_2 + R''_2 = R'_3 + R''_3, \text{ whence}$$

$$R'_2 - R'_3 = -(R''_2 - R''_3) \quad \ldots\ldots\ldots\ldots\ldots\ldots (3)$$

The principles of statics give equations for the difference of these reactions which are entirely independent of any theory of elasticity, the first one of which is as follows:

$$R'_2 - R'_3 = \frac{M'_2}{l_1} - \frac{M'_3}{l_3} + \frac{2(M'_2 - M'_3)}{l_2} + \Sigma P_1 \frac{a_1}{l_2}$$
$$+ \Sigma P_2 \left(1 - \frac{2a_2}{l_2}\right) - \Sigma P_3 \left(1 - \frac{a_3}{l_3}\right).$$

Now, for convenience, let us put

$$\frac{1}{l_1} + \frac{2}{l_2} = \frac{1}{f_1} \qquad \frac{1}{l_3} + \frac{2}{l_2} = \frac{1}{f_3}$$

$$\Sigma P_1 \frac{a_1}{l_1} + \Sigma P_2 \left(1 - \frac{2a_2}{l_2}\right) - \Sigma P_3 \left(1 - \frac{a_3}{l_3}\right) = Q$$

and we obtain

$$R'_2 - R'_3 = \frac{M'_2}{f_1} - \frac{M'_3}{f_3} + Q;$$

and from the above, remembering that in the second case $Q = 0$, we obtain by the aid also of equations (1) and (2),

$$R''_2 - R''_3 = \frac{M''_2}{f_1} - \frac{M''_3}{f_3} = M''_2 \left(\frac{1}{f_1} + \frac{1}{f_3}\frac{F_3}{F'_2}\right) = -M''_3 \left(\frac{1}{f_1}\frac{F_2}{F_3} + \frac{1}{f_3}\right)$$

$$\frac{R'_2 - R'_3}{R''_2 - R''_3} = -1 = \frac{\frac{M'_2}{f_1} - \frac{M'_3}{f_3} + Q}{M''_2 \left(\frac{1}{f_1} + \frac{1}{f_3}\frac{F_3}{F'_2}\right)} = -\frac{\frac{M'_2}{f_1} - \frac{M'_3}{f_3} + Q}{M''_3 \left(\frac{1}{f_1}\frac{F'_2}{F_3} + \frac{1}{f_3}\right)}$$

Whence

$$M''_2 = -\frac{M'_2 f_3 - M'_3 f_1 + Q f_1 f_3}{f_3 + f_1 \frac{F_3}{F'_2}}$$

$$M''_3 = \frac{M'_2 f_3 - M'_3 f_1 + Q f_1 f_3}{f_3 \frac{F_2}{F'_3} + f_1}$$

Therefore

$$M_2 = M'_2 + M''_2 = \frac{f_1 \left(M'_2 \frac{F_3}{F'_2} + M'_3\right)}{f_3 + f_1 \frac{F_3}{F'_2}} - \frac{Q f_1 f_3}{f_3 + f_1 \frac{F_3}{F'_2}} \quad \ldots\ldots\ldots (4)$$

$$M_3 = M'_3 + M''_3 = \frac{f_3 \left(M'_2 + M'_3 \frac{F_2}{F'_3}\right)}{f_3 \frac{F_2}{F_3} + f_1} + \frac{Q f_1 f_3}{f_3 \frac{F'_2}{F'_3} + f_1} \quad \ldots\ldots\ldots (5)$$

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The terms F_2 and F_3 can be computed if the construction is given and the correct theory is known. It is to be noted that so far the argument is deduced from a consideration of the laws of statics alone and nothing is said of the method by which the moments are computed.

If the two end spans are equal and the girder is so constructed as to be symmetrical in all its parts with reference to the middle of the center span, we will, according to the ordinary theory of the continuous girder, have $M'_2 = -M'_3$ or $\frac{F_2}{F_3} = 1$. Let l be the length of each end span and ml that of the middle span, then $f_1 = f_3 = \frac{ml}{2+m}$ and the above becomes

$$M_2 = \frac{M'_2 + M'_3}{2} - \frac{Qml}{2\,(2+m)} \quad \ldots\ldots\ldots\ldots\ldots\ldots (6)$$

$$M_3 = \frac{M'_2 + M'_3}{2} + \frac{Qml}{2\,(2+m)} \quad \ldots\ldots\ldots\ldots\ldots\ldots (7)$$

If these equations are compared with those of Mr. Herschel, it will be found that, for the case he gives, the results are identical, but it is evident that they are practically simpler, easier to remember, and also much more general.

To show that $F_2 = F_3$ for the case just mentioned for variable moment of inertia, use the formula published in the journal of the Franklin Institute for January, 1891, and reduce by making $G_1 = T_3$ and $G_2 = T_2$.

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THE TRANSITION CURVE WHOSE CURVATURE VARIES DIRECTLY AS ITS LENGTH FROM THE P. C. OR POINT WHERE IT CONNECTS WITH THE TANGENT.

BY WILLIAM CAIN, M. Am. Soc. C. E.

The ideal transition curve, to pass from a tangent to a circular curve of given degree, is one whose curvature is zero at the point where it leaves the tangent (P. C.) and increases directly as its length, measured along the curve, to where it connects with the circular curve, at which point it should have the same tangent and rate of curvature as the circular curve. By the use of such curves on railroads or street car lines to ease off the ends of circular curves, the super-elevation of the outer rail for the circular part is gradually attained without shock, and the sudden change from the tangent to a circular curve, so often experienced on unadjusted railroad curves, with its annoying and damaging lurch, is avoided.

A. M. Wellington, M. Am. Soc. C. E., was the first to propose such a transition curve, and recently Mr. C. R. Howard has given his treatment of the same curve. As both these authors develop the theory, to some extent, from approximate considerations, it occurred to the author to

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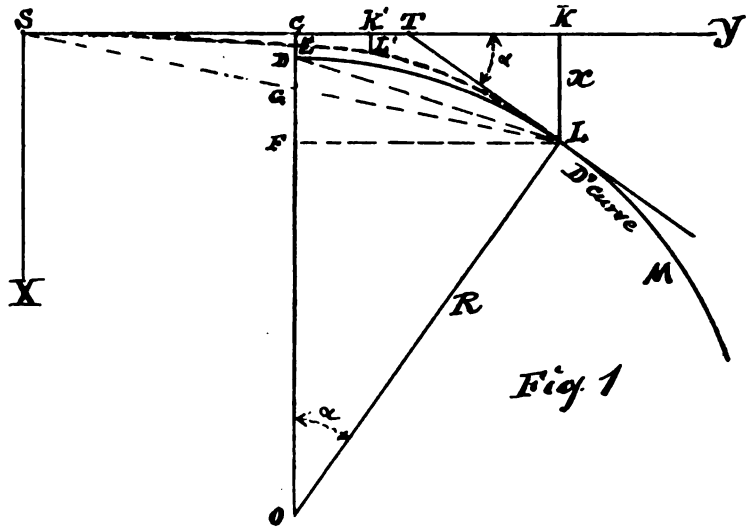
endeavor to make an exact solution, and it is here presented. The solution is simple and direct, and requires only a knowledge of a few elementary principles of the calculus. Certain constants will be computed and certain useful approximate formulas will also be deduced.

In Fig. 1, let the transition curve SEL begin at S and be tangent to the axis SY. Refer the curve to rectangular axes SY and SX and denote the co-ordinates of any point L by X and Y ($KL = x$ and $SK = y$) and the length of curve SEL by s. At L, the transition curve (or spiral) has the same tangent and degree of curvature ($D°$) as the circular curve DLM (of radius r) with which it connects. A similar notation would apply to any other point on the curve as L'.



The tangent at L makes the angle α with the tangent SY; therefore when α is expressed in "circular measure" or in "arc," the "curvature" at the point L (x, y) is represented by,

$$\text{limit}\left(\frac{\Delta\alpha}{\Delta s}\right) = \frac{d\alpha}{ds} = \frac{1}{r};$$

and this, by the definition of the curve, must equal a constant $(2a)$ times s.

$$\therefore \frac{d\alpha}{ds} = \frac{1}{r} = 2as \ldots\ldots\ldots\ldots\ldots\ldots (1)$$

whence,

$$\alpha = as^2 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (2)$$

since $\alpha = o$ when $s = o$. From (1) we have likewise $r = \infty$ when $s = o$.

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From the differential triangle, we have—

$$dx = ds \text{ sin. } \alpha = ds \text{ sin. } (as^2) \ldots\ldots\ldots\ldots\ldots\ldots (3)$$

$$dy = ds \text{ cos. } \alpha = ds \text{ cos. } (as^2) \ldots\ldots\ldots\ldots\ldots\ldots (4)$$

As these expressions cannot be integrated in finite terms, the equation of the curve in terms of x and y cannot be obtained.

In practice, the curve OL will be run by measuring N chords of c feet each along the curve. When these chords are sufficiently short, they can be regarded of the same length as the arcs subtended by them, hence we shall always write,

$$s = Nc \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (5)$$

for the length of curve from the origin S to any point considered, as L.

In running circular curves, such a chord length should be taken that the arc and chord are practically equal. Thus for 1, 2, 3 and 4 degree curves, chord lengths of 100 feet may be taken; for 8-degree curves, 50-foot chord lengths should be used, and so on; in which case the degree of any curve D° (as for DLM at L) is equal to the radius of a 1-degree curve divided by r

$$\therefore D^\circ = \frac{18\,000}{\pi r} = \frac{36\,000}{\pi}\, as \ldots\ldots\ldots\ldots\ldots (6)$$

To express the arc α (equation 2) in minutes, we notice that its ratio to a semi-circumference whose radius is one, is $\frac{d}{\pi}$ and multiplying this by 180 x 60, we find from (2) and (5)

$$\alpha \text{ (in minutes)} = \frac{\alpha \text{ (in arc)}}{\pi} 180 \times 60 = \frac{as^2}{\pi} 180 \text{ x } 60$$

$$\therefore \alpha \text{ (in minutes)} = a\frac{180 \times 60}{\pi} c^2 N^2.$$

Now, as a and π are constant, we see that α varies with c^2N^2, and if we assume, as Mr. Howard does, for any length of chord c, that α is the same for the same value of N, and, in minutes, is equal to 6 N^2 (as by the above equation it varies as N^2), we have—

$$\alpha \text{ (in minutes)} = 6\, N^2 \ldots\ldots\ldots\ldots\ldots\ldots (7)$$

Of course, we have a right to use any other constant than 6 as the coefficient of N^2 (and, doubtless, others will be used in time in addition), but having assumed (7) as a fundamental equation, we must find the corresponding value of a by equation (7) and the equation just above it; whence—

$$a = \frac{\pi}{1800\, c^2} \ldots\ldots\ldots\ldots\ldots\ldots\ldots (8)$$

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On substituting this value of a in (6), we have—

$$D^\circ = \frac{18\,000}{\pi r} = \frac{36\,000}{\pi} as = \frac{20s}{c^2} = \frac{20N}{c} \quad \ldots\ldots\ldots\ldots \quad (9),$$

which gives the degree of curvature of the transition curve, N stations of c feet each, from the origin s. If it connects there with the circular curve, the tangent and degree of curvature at the connecting point are the same for both curves.

By multiplying both sides of equation (9) by $\frac{s}{200}$ we have—

$$\frac{D^\circ s}{200} = \frac{D^\circ Nc}{200} = \frac{20N}{c} \cdot \frac{Nc}{200} = \frac{N^2}{10};$$

but from (7), α in degrees $= \frac{6N^2}{60} = \frac{N^2}{10}$; hence—

$$\alpha \text{ (in degrees)} = \frac{D^\circ s}{200} = \frac{D^\circ Nc}{200} \quad \ldots\ldots\ldots\ldots \quad (10)$$

In Fig. 1, at O, the center of the circular curve DLM, having at L the same tangent and degree of curvature as the spiral SEL, draw a perpendicular OC upon the tangent SY, cutting the spiral at E and the circular curve produced at D and draw the chords SL and DL. Then $LOC = \alpha = LTK$ and $\frac{DL}{100}$ = number of 100 foot stations in arc DL

$\therefore D^\circ \frac{\text{arc } DL}{100}$ = total angle turned in length $DL = \alpha = \frac{D^\circ s}{200}$ by (10);

whence—

$$\text{arc } DL = \tfrac{1}{2} s = \tfrac{1}{2} \text{ length of spiral } SEL \quad \ldots\ldots\ldots\ldots \quad (11)$$

From this we have, since for flat arc $LD = LE$ nearly, $LE = \frac{1}{2}$ SEL or point E is nearly at middle of spiral. The "gap" $CD = q$ between the tangent sy and the circular curve may then be regarded as the offset at the middle of the transition curve from circular curve to tangent sy.

We can deduce two useful formulas by aid of (9) above.

$$c = \frac{20\,N}{D^\circ} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (12)$$

$$Nc = s = \frac{20\,N^2}{D^\circ} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (13)$$

We shall next deduce expressions for computing x and y for given values of s. From equation (3) we have, developing sin. (as^2) by the usual formula—

$$dx = ds \left(as^2 - \frac{1}{6} (as^2)^3 + \frac{1}{120} (as^2)^5 - \text{etc.} \right);$$

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whence integrating and making the constant zero, since $x = o$, when $s = o$, and placing for brevity α for (as^2) from (2), we have—

$$x = s\,\alpha\left(\frac{1}{3} - \frac{\alpha^2}{42} + \frac{\alpha^4}{1320} - \text{etc.}\right) \ldots\ldots\ldots\ldots\ldots (14)$$

Proceeding similarly with $dy = ds \cos.\ (as^2)$, and we deduce,

$$y = s\left(1 - \frac{\alpha^2}{10} + \frac{\alpha^4}{216} + \frac{\alpha^6}{9360} + \text{etc.}\right) \ldots\ldots\ldots (15)$$

From (8), $a = \frac{\pi}{1800c^2}$; hence α in equations (14) and (15) is given (in length of arc on a unit circle) by—

$$\alpha = as^2 = ac^2N^2 = \frac{\pi}{1800}N^2 = .001745329\ N^2;$$

$$\therefore \log.\ \alpha = 7.2418774 - 10 + \log.\ (N^2) \ldots\ldots\ldots\ldots\ldots (16)$$

The above value of α can be obtained likewise from equation (7). It is independent of c, as in fact was assumed from the first; but since $s = Nc$, the values of both x and y above, vary directly as c. Hence, if we compute from (14) and (15) successive values of x and y for $c = 100$, corresponding to $N = 1, 2, 3 \ldots 15$, and denote these values, respectively, by—

$$X_1, X_2, X_3 \ldots X_{15},$$
$$Y_1, Y_2, Y_3 \ldots Y_{15},$$

the subscripts denoting the station to which they refer; then when c has any other value than 100, we have by (14) and (15),

$$x = \frac{Xc}{100} = \frac{0.2\,NX}{D^\circ} \ldots\ldots\ldots\ldots\ldots\ldots (17)$$

$$y = \frac{Yc}{100} = \frac{0.2\,NY}{D^\circ} \ldots\ldots\ldots\ldots\ldots\ldots (18)$$

The last forms being derived from (12) by putting for c its value

$$\frac{20\,N}{D^\circ}.$$

The results of the computation are given in the adjoining table under the corresponding values of N, given at the tops of the columns.

In computing the values of X given by (14), it was found that the results could be found correctly to the last figure given, for $N = 1, 2$, by taking only the first term of the parentheses and neglecting the others; for $N = 3, 4, 5, 6, 7, 8, 9, 10, 11$, two terms are needed, and for $N = 12, 13, 14, 15$, only three terms are required.

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For the values of Y, two terms of the parenthesis in (15) were used up to $N = 9$; for greater values of N three terms only are required. A six-figure table of logarithms was used except for the larger values, where a seven-place table proved desirable.

The series is very converging for the small values of α used, and, in fact, would answer for much larger values.

N.	1	2	3	4	5	6	7	8
X.....	.058178	.46542	1.57074	3.72316	7.27121	12.5628	19.9445	29.760
Y.....	100.000	199.999	299.993	399.969	499.904	599.763	699.486	799.002
F.....	.25000	.25000	.25000	.25000	.25001	.25002	.25005	.25011
Q.....	.002909	.046542	.235611	.744632	1.81785	3.7692	6.982	11.910
α.....	6′	24′	54′	1° 36′	2° 30′	3° 36′	4° 54′	6° 24′
Δ.....	2′	8′	18′	32′	50′	1° 12′	1° 38′	2° 07′ 59″

N.	9	10	11	12	13	14	15
X.....	42.351	58.051	77.188	100.078	127.024	158.310	194.197
Y.....	898.201	996.958	1095.104	1192.442	1288.735	1383.706	1477.033
F.....	.25018	.25026	.25038	.25056	.25077	.25104	.25137
Q.....	19.071	29.056	42.519	60.180	82.819	111.278	146.448
α.....	8° 06′	10° 00′	12° 06′	14° 24′	16° 54′	19° 36′	22° 30′
Δ.....	2° 41′ 58″	3° 19′ 57″	4° 01′ 55″	4° 47′ 51″	5° 37′ 45″	6° 31′ 37″	7° 29′ 25″

The angle made by a chord drawn from station 0 (S) to station N, with the axis SY (Fig. 1), will be designated by Δ_n. This angle is readily found from the formula—

$$\text{tan. } \Delta_n = \frac{X_n}{Y_n} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (20)$$

Thus, for Δ_{12} we have tan. $\Delta_{12} = \frac{100.078}{1192.442}$; whence $\Delta_{12} = 4° 47′ 51″$.

The values of the angles Δ are inserted in the adjoining table under the proper value of N, and the values of α derived from equation (7) are placed above for comparison, from which we note that if we express Δ to the nearest minute that

$$\Delta = \frac{\alpha}{3} = 2N^2 \text{ (in minutes)} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (19)$$

except for $N = 15$, where the error of using the formula is 35 seconds, a matter of no practical importance. There is no use to be made of (19), however, except in checking roughly the values of Δ in the table. If the table is extended, as it easily can be, to any desirable values of N, the values of tan. Δ must all be computed by dividing the X by the

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corresponding Y and finding the angle Δ corresponding. As in practice, the table will always be at hand, there is no need for formula (19) except in checking for the smaller values of N.

The angle made by any chord connecting any two stations of the curve with the Y axis (line SY), will be designated by i with two subscripts, giving the station numbers through which the chord is drawn; thus i_{3-9} indicates the angle made by the chord joining stations 3 and 9 with the Y axis. Its value is readily found from the equation,

$$\text{tan. } i_{3-9} = \frac{X_9 - X_3}{Y_9 - Y_3} = \frac{40.7803}{598.208}$$

to be $i_{3-9} = 3^\circ\ 54'\ 00''$.

Similarly, we find the inclinations of all the chords to the Y axis, these chords being drawn between any two stations whatsoever.

A demonstration of a more rapid way of computing these angles to the nearest minute will be given further on.

Line Q in the table gives the product qD°, where q is the distance CD (Fig. 1) and D° the degree of the circular curve LA. We find this product as follows:

Call for brevity, R_1 = radius of a 1-degree curve = 5729.65; then (Fig. 1), $R = \frac{R_1}{D^\circ}$ and $q = KL + OF - OD = x + R \text{ cos. } \alpha - R$.

Therefore from (17),

$$q = \frac{.2NX}{D^\circ} + \frac{R_1 (\text{cos. } \alpha - 1)}{D^\circ}$$

$$\therefore\ Q = qD^\circ = 0.2\ NX - R_1 (1 - \text{cos. } \alpha) \quad \ldots\ldots\ldots\ldots (21)$$

Calling now the ratio of q to the ordinate at $L = F$, we have,

$$F = \frac{q}{x} = \frac{qD^\circ}{.2NX} = \frac{Q}{.2NX} \quad \ldots\ldots\ldots\ldots\ldots\ldots (22)$$

In computing the quantities Q and F given in the table, a seven-figure logarithmic table was used except for $N = 1$ to 6 inclusive, where a ten-place table was needed. This last computation, though, is not absolutely necessary, as we shall see presently that for N small, the value of F must approach one-fourth, as given in the table. If we assume it then at 0.25 for $N = 1, 2, 3, 4$, we have from (22) $Q = .05NX$ for these values of N as given in the table.

The fundamental formulas given above are all that are needed for solving any problem concerning the transition curve.

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Approximate Formulas.

On referring to the table of quantities, we notice that the ordinate X at the middle of any length of curve is nearly one-eighth that at the end. Thus $\frac{1}{8} X_{12} = 12.51$, and this is nearly equal to $X_6 = 12.56$. We can see that this is generally true if we write $x = \frac{1}{8} as^3$ approximately from (14) on neglecting all terms after the first; then designating by x_o the ordinate corresponding to $s_o = \frac{1}{2}s$, we have $x_o = \frac{1}{8}(\frac{1}{8} as^3)$ or one-eighth the extreme ordinate x.

The equation $x = \frac{1}{6} ay^3$ is that of the cubic parabola, and we see that the equation $x = \frac{1}{8} as^3$ closely approximates to it for very flat arcs.

We have found, for flat arcs, that radius OD (Fig. 1), drawn from O perpendicular to SK produced, nearly exactly bisects the curve SEL; hence SG is nearly equal to LD, and since Δ (in arc) $= \frac{CG}{SG}$ nearly, and $\frac{\alpha}{2}$ (in arc) $= DLF$ (in arc) $= \frac{FD}{LD}$ and $\Delta = \frac{\alpha}{3}$ nearly, we have, approximately,

$$\frac{\Delta}{\left(\frac{\alpha}{2}\right)} = \frac{CG}{FD} = \frac{2}{3}$$

or since $CG = \frac{KL}{2} = \frac{x}{2}$ $\therefore$ $FD = \frac{3}{4} x$, and,

$$DC = FC - FD = x - \tfrac{3}{4} x = \tfrac{1}{4} x = q.$$

This last result explains why $Q = \frac{q}{x}$ in the table, is $\frac{1}{4} = 0.25$, for small values of N.

Now, since $CE = x_o = \frac{1}{8} x$ (nearly), as shown above, and $q = \frac{x}{4}$ (nearly) we have approximately,

$$CE = \tfrac{1}{2} CD = \tfrac{1}{2} q;$$

so that the curve SEL nearly bisects the distance q between the tangent and the circular curve.

We shall now proceed to deduce formulas for quickly computing the angles i to the nearest minute.

We have seen that the formula (19), $\Delta = \alpha \div 3$, is correct to the nearest minute up to and including $N = 14$ (and near enough for $N = 15$). In fact, it is exact to the second, up to and including $N = 7$. Referring now to equation (14) and neglecting all terms in the parentheses after the first, and calling x^1 the approximate value of x_1 we have

$$\frac{x^1}{s} = \frac{1}{3} \alpha;$$

and this equals Δ to the nearest minute within the limits taken.

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We should naturally infer, within the same limits, that the angle i (in circular measure) could be expressed by a similar approximate formula,

$$i = \frac{x_1^1 - x^1}{s_1 - s}$$

where $x_1^1 - x^1$ is the difference in the ordinates, computed by the approximate formula above, and $s_1 - s$ is the length of curve between them. This formula reduces to the preceding when x^1 and s are zero, in which case i reduces to Δ.

We have.....$x^1 = \frac{1}{6} \alpha s = \frac{1}{6} \alpha s^3 = \frac{1}{6} \alpha c^3 N^3$;

hence,

$$i = \frac{x_1^1 - x^1}{s_1 - s} = \frac{\alpha}{3} \frac{c^3 (N_1^3 - N^3)}{c (N_1 - N)} = \frac{\alpha c^2}{3} (N_1^2 + N_1 N + N^2).$$

Replacing αc^2 by its value $\frac{\pi}{1\,800}$ (equation 8) and multiplying both sides of the equation by $\frac{180 \times 60}{\pi}$ to reduce to minutes, we have,

$$i \text{ (in minutes)} = 2 (N_1^2 + N_1 N + N^2) \ldots\ldots\ldots\ldots\ldots\ldots (23)$$

This gives, approximately, the angle i in minutes, for the inclination of the chord passing through stations N and N_1. It is evidently immaterial which we take as the forward station, as the same method of procedure will lead to the same formula (23), whether N_1 or N be regarded as the forward station.

As an application, let $N_1 = 10$, $N = 5$;

$$\therefore\ i_{5-10} = 2 (10^2 + 10 \times 5 + 5^2) = 350' = 5^\circ\ 50',$$

which is correct to the nearest minute, and, in fact, differs only a few seconds from the exact value. It is found, by trial, that the formula can be safely used up to $N = 15$ for computing i to the nearest minute. Where a whole table has to be computed, it is more expeditious to proceed by the method of differences. On changing N to $(N + 1)$ in (23), we get the angle i for the chord from station $(N + 1)$ to station N_1. Subtracting (23) from this we have the angle between the two chords from N to N_1 and $(N + 1)$ to N_1 equal to (in minutes),

$$\text{1st difference} = 2 (N_1 + 2 N + 1) \ldots\ldots\ldots\ldots\ldots\ldots (24)$$

As N increases one at a time, the first difference increases $4'$; hence,

$$\text{2d difference} = 4'.$$

We observe from the formula (23) that for $N = 0$, $i = \Delta = 2 N_1^2$, which agrees with (19), and for $N = N_1$ we find that the right member becomes $6 N^2$, which by (7) is exactly equal to the α corresponding to the

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station. Hence, starting with $N = 0$ in (23), which gives Δ, and increasing N one at a time, we compute the corresponding i's until $N = N_1$ is reached, when the α at stations $N = N_1$ is found. As N again increases one at a time the corresponding i's are found.

In Fig. 2 is given an illustration for $N_1 = 5$ $\therefore$ first difference $= 12 + 4N$ by (24) and second difference $= 4$.

For $N = 0$, angle between chords 05 and 15 $= 12'$

$N = 1$, " " " 15 " 25 $= 16'$

$N = 2$, " " " 25 " 35 $= 20'$

Similarly for the others, starting with $\Delta_5 = 50'$ and adding the successive differences above, we deduce at once the values given in the figure: $i_{1-5} = 50 + 12 = 62'$, $i_{2-5} = 78'$, $i_{3-5} = 98'$, $i_{4-5} = 122'$, $i_{5-5} = \alpha_5 = 150'$, $i_{5-6} = 182'$, and so on.

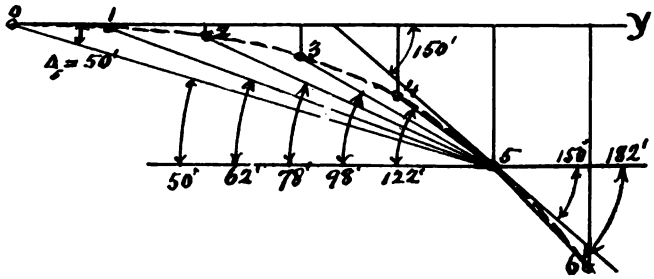

Fig. 2

Mr. Howard has noted, without demonstrating it generally, however, that the above procedure would lead to the same values of i (to the nearest minute), as is found by a more exact method. An interesting result will now be noted.

The angle i that a chord from station $N_1 - 1$ to station N_1 makes with the Y axis, is found from (23) by changing N to N_{1-1}.

$$\therefore i = 6N_1^2 - 6N_1 + 2.$$

Subtracting this from the α at $N_1 = 6N_1^2$, we have $\alpha - i = 6N_1 - 2$.

If N_1 is the point of connection with the circular curve, we have, $D^\circ = \frac{20N_1}{c}$, and the first deflection from the tangent on the circular curve for a chord of c feet is $\frac{1}{2}\ 60\ D^\circ\ \frac{c}{100} = 6N_1$ minutes.

This is greater by 2 minutes always than the angle between the tangent at station N_1 and the chord from station $N_1 - 1$ to station N_1,

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as we have just found the latter angle to equal $(6\ N_1 - 2)$ minutes. This is as it should be, for from the definition of the curve the curvature increases regularly up to the point of connection with the circular curve, at which point alone the transition curve has exactly the same rate of curvature as the circular curve.

Having given above the methods for computing, either exactly or approximately, all needed elements of the transition curve, only a hint remains to be given as to the method of running in the curve. By formulas (17) and (18) x and y can be quickly found, by aid of the tabular values of X and Y, and the stations located; otherwise, with a table of values of Δ, α and i, the curve can be run by the method of deflection angles, as in the case of a circular curve, and with equal facility.

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THE MAIN RELIEF SEWER OF BROOKLYN.

By WILLARD BEAHAN, M. Am. Soc. C. E.

WITH DISCUSSION.

The relief of the overcharged sewers of our older and growing cities is one of the problems in engineering now before us. The Main Relief Sewer of the City of Brooklyn, N. Y., has at the outset the merit of an explanatory name. It is now completed, with marked success, by the Department of City Works of Brooklyn, of which Mr. John P. Adams is Commissioner, Robert Van Buren, M. Am. Soc. C. E., Chief Engineer. L. Russell Clapp, M. Am. Soc. C. E., was Assistant Engineer in charge. The western portion of Long Island, on which the city is situated, has rugged topographical features, which grow less marked as the Narrows are approached, but which form the distinctive feature of the site. The city is divided into sewer districts, the boundary lines of which are the dividing ridges in the secondary or tertiary drainage. Each district is distinct in system, the combined system of sewers is used, and the main sewer of each district empties into tide water. The sewers are of various dates of construction; some are of vitrified pipe, some of cement pipe, and the main district

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sewers are of brick or stone. In the recent rapid growth of the city, those in the old residence district of Brooklyn Heights needed to be supplemented. Much of the new residence district is found in Sewer Districts "F," "K" and "L," whose main sewers became surcharged in storms. To relieve all of District "F" and the portions of Districts "K" and "L" lying south of Greene Avenue is the object of the Main Relief Sewer of which we speak. The accompanying map clearly shows the situation (Plate XLII).

For the outlet of the new sewer, it was decided to use the Gowanus Canal, this giving the shortest egress to tide-water. As it was to carry excess water in storms, its outflow would improve rather than injure the condition of the canal. Starting, then, at the head of the Gowanus Canal, near Butler Street, at a point between Nevins Street and Bond Street, the first district main to be relieved is the Raymond Street main, at the foot of Hanson Place, this conveying all of the drainage of District "F." The next is the Grand Avenue main, intercepted at Grand and Greene Avenues, and there charged with the sewage of the part of District "K" which lies south of Greene Avenue. The portion of District "L" lying south of Greene Avenue is relieved at three points on Greene Avenue, viz., Bedford, Nostrand and Marcy Avenues. At Marcy Avenue is the present terminus of the Main Relief Sewer; its location is shown on the map, and its length is 12 300 feet.

The drainage area, the sewers of which are intercepted, is 2 000 acres. The flood water of storms was that for which provision was to be made, and the house drainage was to be allowed to flow by in the present mains. It was deemed safe to provide for the storm water from an area of 1 300 acres. The maximum rainfall in Brooklyn is 4 inches per hour, and of this it was assumed that 1 inch per hour would reach the Relief Sewer. The bottom of the invert at Gowanus Canal was fixed at the elevation of mean high water for that place, thus establishing the grade elevation at the outlet. At Nostrand Avenue on Greene Avenue, it will be noticed that the profile (Plates XLIII and XLIV) shows low ground, and the bottom of the brickwork of the Nostrand Avenue main sewer, where it crossed Greene Avenue, was a point the elevation of which controlled the maximum elevation allowable for the top of the brickwork of the Main Relief Sewer, thus giving a second controlling point in the grade line. The level of the ground water for the city as caused by rains and held by

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impervious ground is at an elevation of about 11 feet above high tide. Strictly speaking, this water level is an inclined plane, and fluctuates at times, rising at occasional points in a way not readily accounted for. To lay a sewer grade-line so that the bottom of the excavation passes below this plane, necessitates ceaseless pumping.

The elevation of this water at different points along the line of the sewer was next determined by boring; the character of the material to be met with on the line of the sewer being determined at the same time. At the Department of City Works of Brooklyn the results of borings are given but little credence. The writer inclines to the opinion that the little that can be done in boring is a small fraction of what we assume ourselves able to do. The complete record of the gross errors of conclusion made from borings, would stagger any man. On Long Island a couple of men had been seen digging wells for farmers in a way that seemed to be adapted to the problem in hand. They were employed to sink test shafts 4 feet square on the center line of the proposed sewer to and below the proposed sewer sections, using four horizontal boards in a rectangle, notched and pinned together at the corners. They thus built, as did the Irishman his chimney—"from the top downward," to the water surface. Notes of the material were kept as they progressed, and the distance to water surface noted; the material met was the drift on which Brooklyn is built, consisting for the most part of sand and gravel. The sand was sometimes dry and fine, but usually damp enough to stand well. The gravel was of all possible sorts; it was cemented together in some places, and in a few instances was a mass of cobble stones with no intermixed material. Clay or hard pan was met on Hanson Place. Heavy granite boulders were found sometimes in the clay, sometimes in sand or gravel, but always in passing into or away from the clay. These boulders, as it was subsequently found, ranged from the size of a cobble stone to those containing 10 cubic yards. In sinking the shaft at St. Felix Street and Hanson Place, a water pocket was met in the sand, necessitating tight curbing for some 12 feet. Clay underlaid this water, and probably a depression in the upper surface of this clay stratum caused the water to collect there. The bottom of the pocket was nearly as low as the top of the proposed brickwork.

The form of section chosen for the sewer was a circle, except where the head room was insufficient for such a section, as this is the strongest

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section for loose material and the section of least resistance to the full flow. The only change from this form was at the outlet on Butler Street, where there was insufficient head room, and I-beams were required; on this account the gradient on this street was increased, since the reduced section required it. The grade line as adopted is shown on the profile.

After construction had begun, owing to excessive storms, or the fact of a layer of quicksand at Shaft No. 3, the water there was found to be higher than the test shafts had shown, and the pumps removed the sand with the water. The grade line was raised to correspond with the water surface as found, although some pumping was required while laying the bottom courses of brick, for a long way from this shaft. The author does not seek to explain this change in water level. It is now plain that to raise the grade line was the only practicable solution.

Of the entire drainage area, 2 000 acres, of the three sewer districts relieved but 1 300 acres would ever empty storm water into the sewer to be built, on which let us take, as stated, the maximum rainfall per hour as 4 inches, of which 1 inch per hour reaches the sewer as a maximum. By Kutter's formula, $Q = av$ where $v = c\sqrt{rs}$. Determining c for a coefficient n in the formula $= .015$ for brick, and using a fall of 1 in 1 000, we get for a circular sewer 15 feet in diameter $v = 237.9 \times .031623 = 7.52$. $\therefore Q = 176.72 \times 7.52 = 1325.4$ cubic feet per second for the maximum discharge. One inch per hour on 1 300 acres is 1 313 cubic feet per second. Fifteen feet is the diameter employed below where the Raymond Street sewer enters, and the gradient is somewhat steeper than 1 in 1 000 over that section.

The general structure of the sewer is briefly described thus (Plate XLV): Starting at its upper end on Greene Avenue, beyond Marcy Avenue, we have a circular brick sewer 10 feet in diameter, and at Marcy Avenue one of 12 feet in diameter. At Marcy, Nostrand and Bedford Avenues, the sewers of District "L" are intercepted. Just before reaching Grand Avenue the diameter increases to 14 feet, and at Grand Avenue the main sewer of District "K" is relieved. At Raymond Street and Hanson Place the diameter of the sewer is increased to 15 feet, and the main from District "F" relieved. Just after leaving Fourth Avenue and turning down Butler Street, the head room is insufficient for a circular sewer of such diameter, and a segmental invert, verti-

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cal side walls and a flat top composed of I-beams (Plate XLVI), with flat brick arches to fill spaces between these beams and carry the street surface, are employed. Finally, we have the large trap basin with all its appurtenances, at tide water at the canal.

The estimate of cost was, at the first, $1 000 000. The actual cost was kept within that estimate.

The method of construction was the next thing to be decided. At Vanderbilt Avenue the depth from the surface of the street to the bottom of the invert was 90 feet. For a great part of the way the average depth exceeded 50 feet. Clearly, this portion had to be tunneled. Where the sewer entered the Gowanus Canal the cutting was shallow, and continued so to Fourth Avenue. It was decided, therefore, to build the sewer in open cut 2 940 feet from the canal to Dean Street, and to tunnel for the remainder of the distance. The open cut work was designated Section No. 1, and on July 1st, 1890, the contract for it was awarded to Daniel J. Creem, Brooklyn, two hundred working days being given in which to complete the work. The tunnel portion of the work 9 340 feet in total length was designated Section No. 2, and on June 27th, 1890, the contract was awarded to Charles Hart, Fourth Avenue and DeGraw Street, Brooklyn, and Anderson & Barr, of No. 240 Eleventh Avenue, Jersey City, under the firm name of Hart, Anderson & Barr, and three hundred working days named as the time within which work was to be finished. There was no stipulation as to the method of tunneling. Work on each section was begun at once, and was completed by the parties named, Section No. 1 being finished in December, 1891, and Section No. 2 in February, 1892.

In constructing Section No. 1, sheeting and timbering were used in the ordinary manner for open cut work. The material excavated was lifted by derrick and bucket to cars on a track running alongside the work. The cars were hauled by horses back to where the brickwork was completed, and there dumped. Work was begun at the lower end and carried forward as described. No difficulties occurred, save the falling of a small portion of the arch where the section was changing from circular to flat topped. This it was thought arose either from striking the centers before giving all the time needed for the cement to set, or from a slight yielding of the upper portion of the trench walls. The trap basin formed no small part of the work of Section No. 1. Its construction is most instructive and is shown on plans

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herewith (Plate XLVII). No difficulties were met in carrying out the designs. The special features of plan and detail, as well as the sewer as a whole, will be treated of later.

The depth of Section No. 2 varied from a least depth at Dean Street of 37 feet, to a greatest depth of 90 feet at Vanderbilt Avenue. The diameter of the sewer inside the brickwork was to be, starting at Dean Street, 15 feet for 700 feet, 14 feet for 4 700 feet more, and then 12 feet to Marcy Avenue. The route lay along paved streets, asphalt or stone all the way, and under horse-car tracks and a cement pipe sewer for much of the way. It was a populous residence district, with its usual complement of gas, water and sewer pipes. The residences were usually three-story brick buildings with stone fronts. The Brooklyn Tabernacle and many other churches, with heavy spires close to the sidewalk, were on the route. The lines of three elevated roads were also to be passed under.

The material, as shown in the test shafts, was the drift already described. This work comes, therefore, under the general case of tunnels of medium cross-section in ordinary soft ground, and is a fair basis for the discussion of methods for tunneling in such material; herein lies its interest to engineers. In these days of rapid transit, of provisions for larger drainage or increased water supply, and of increased restrictions placed upon the bridging of navigable streams, the question of tunnel systems is one of interest to us. Any current newspaper clamoring for rapid underground transit sagely mentions a shield as the *summum bonum* of tunneldom. Any railroad engineer thwarted by the War Department in an attempted bridge over navigable waters seeks the shelter of a shield, for he has heard of a shield being used at the Sarnia or St. Clair tunnel to aid in such an extremity as the one he is in. To him it seems a veritable "Hobson's choice." If a tunnel cannot or will not be built best by the use of a shield, it is all the fault of the tunnel, he thinks. We all use and resort to that which is before our eyes, and tunneling in soft ground is not frequent enough in its examples to allow us to make the distinctions which circumstances would dictate on closer study. There is no one method for tunneling in such ground that is the best. The size of the cross section of the tunnel, the kind of ground to be tunneled through, and the surroundings are considerations that must rule in planning how to do the work. A method of tunneling well adapted to a tunnel 28 feet in

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diameter is quite apt to be less adapted to a tunnel of half that diameter. Again, a method of tunneling practicable in a putty-like clay under a river, is not necessarily best among huge boulders. The author is aware of the outcry that will be made against this, to him, sound reasoning, but he has read tunnel literature in newspapers and engineering journals, and visited tunnels under construction; talked with tunnel men, and heard the praises of "tunnel systems" sung, until he feels called upon to say that common sense applies even to expert tunneling. The best method to use in building a certain tunnel is that which costs the least to do the desired work.

There are to-day three general and distinct methods of tunneling in soft ground. The first in the order of introduction is the timbered system. It embraces several distinct types and many modifications or mergings of them. The English, Austrian, German and Belgian are the principal ones.

In Pennsylvania and in the South, the German method of segmental timbering with a supporting core, and the English bar method, are used.

Tunneling was begun in that way for railroads, usually not in sand, but in clay. A combination of the English and German methods is in use in trying circumstances by the Baltimore and Ohio Railroad, for their double-track belt line in Baltimore, and is fairly successful, but the Brooklyn tunnel was much smaller and poorly adapted for its use. Those methods are never inexpensive or rapid. The German, with its core of soft ground, is dangerous to the arch, and makes the side-wall construction difficult. Timber is more expensive than in former times, and any timber exceeding in length two-thirds of the finished diameter of the tunnel section is difficult and expensive to handle. To draw the crown timbers from the top of an arch in the English method is never desirable, and requires a great pulling force when the material driven through is dry sand, boulders or gravel. In the streets of Brooklyn, to draw them would cause a settlement sufficient to break pipe sewers and water pipes above, and would damage pavements. If left in, there is still considerable space to be filled with earth by ramming, and horizontal ramming is never satisfactory in irregular spaces, especially in such material. For these reasons it was concluded that timbering in so small a section would be cumbersome and slow, and was not a desirable method.

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The second system, in which a shield is used, was then considered. A shield calls for a large initial outlay and a considerable addition to the cost of operating machinery during the progress of a work. This method was first used by Brunel in the stiff London clay, and, later, was improved and further used by Greathead. A simpler form was designed by Beach and patented in 1869 in this country.

Recently, we have been using shields of better models than those of Brunel, but of the same type in all essentials, and we all must recognize his originality and boldness. Greathead and others have improved it, if we take the old shield as shown in "Drinker on Tunneling," as an illustration. Mr. Beach's shield as patented would not be recognizable as a shield at Sarnia. But the writer is not aware of any patents in force, and the pseudo-wisdom and exclusive knowledge some of us profess in this matter is most ill-timed. Is not a shield essentially a pneumatic appliance? Shields, like cantilever bridges, are perhaps imperative under certain circumstances. A few years ago we thought we must use a cantilever, where now we see we could use less expensive types better. So of shields in tunneling—we talk shield now as we have been talking cantilever. The author thinks no material is fit to use a shield in, unless that material will flow. Under water, where air pressure is needed, a shield protects. If the material is soft, as in the Hudson River Tunnel, a shield increases the safety of the men, and is a good system, but not an indispensable one. The latter is the most rational place in which to use a shield the author ever saw or heard of, and Mr. Moier did rapid work there last year by its use, and demonstrated its efficiency. At the St. Clair Tunnel there was little use for shields, as the clay generally could have been tunneled by any system that could stand the shearing stress, for it would not fall or flow and was air and water tight. Where they met the water-bearing pocket of sandy material, the shield gave considerable aid. Usually, the excavating was done by the men in front of the shield and the muck passed out through the doors—an expensive process. The shield was then shoved forward, shaving off the ragged edges of the cut to the neat lines. This seems a sentimental use of Brunel's invention as compared with the Hudson River Tunnel work.

Several years ago, Hart, Anderson & Barr built and used a shield in the Knickerbocker Avenue extension sewer on South Fifth Street,

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Brooklyn. The material was presumably the same as for the work now to be done. It was found on trial that the shield was difficult to keep up to grade line, and made smooth brickwork impossible. It was also found that even in fine sand no amount of pressure would make that sand flow through the doors. The men were obliged to go through the doors, pole away the sand from before the cutting edges of the shield, and shovel it out through the doors before the shield could possibly be shoved forward. Had the material been dry instead of damp sand that stood well, the shield could never have been moved at all. The material would have taken such a slope, that to relieve the cutting edges around the bottom of the shield, would necessitate removing material enough to cause the sand to come down in front of the cutting edge at the roof of the shield, thereby causing a settlement of the surface of the street. As it was expected that some boulders would be met, the outlook for using this shield on South Fifth Street was discouraging. A large boulder squarely across the cutting edge in the front of the bottom of the shield would stop it effectually, and after working it for some time and making very slow progress, the shield was abandoned and bricked in.

The third and last system of tunneling in soft ground, called the pilot system, was introduced in 1880 by Anderson, who was then superintendent of the Hudson River Tunnel. It consists primarily of a small tunnel advanced by the poling process ahead of the full-sized section, and this smaller circular tunnel is lined completely with steel plates bolted together. By poling, the roof and then the sides and invert of the full section are excavated from around this pilot, and the braces supporting the roof rest on the pilot. This pilot or exploration tunnel had originally for its purpose the finding out of dangerous ground ahead. As it is carried in the center of the section it reduces the length of all braces, so that nothing but small, short timbers are needed. These braces are therefore but a small part of the cost of the shoring necessary for a timbered construction, and no crown pieces are needed. The brickwork is built out to the lining or sheeting all the way around the section, thus doing away with tamping and settling. Smaller timbers and no voids are the features of this method as against the timbered plans of work.

The comparative cost of these three methods for the case in hand is the next question. The cost of a timbered section and its advantages

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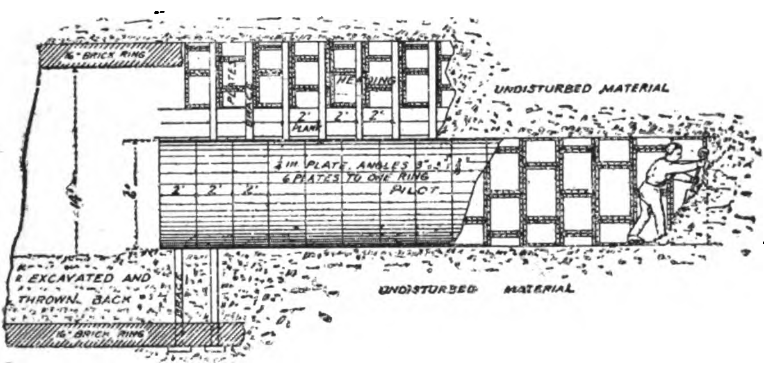
have already been sufficiently stated. The cost of using the shield system needs more consideration. The shields for the St. Clair Tunnel weighed 85 tons each. They were of the best quality of steel, and, of course, the patterns were quite expensive. Except where there are few headings this initial cost is quite prohibitive. By the time the shield is built and at the bottom of the shaft or tunnel portal, with its complement of jacks and machinery, no small percentage of the expected profits has been expended on an ordinary tunnel contract. But the greatest outlay has still to be met, that is, the cast iron lining which so far has always been found to be required. Mr.



LONGITUDINAL SECTION. MAIN RELIEF SEWER—IN TUNNEL.

FIG. 1.

Hobson used twenty-four hydraulic jacks at St. Clair with each shield, each jack having a capacity of 125 tons. These jacks need something strong to stand their back thrust. Brickwork will not stand such compression by any means now known. Nothing else than a cast-iron lining has served thus far here. At St. Clair this lining was 2 inches thick and heavily flanged. At the Hudson River Tunnel they used thinner plates for a time, but were later obliged to make them heavier —of the 2-inch thickness, I believe. The cost of such cast-iron lining alone, about equals the cost of a brick-lined tunnel of the same diameter. So long as this be true, shields will be used by corporations rather than by contractors, for they cannot compete in price with the other ways of doing the work.

Inasmuch as the pilot system would avoid the settlement of the streets and be less cumbersome in the section to be built than a tim-

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bered system of doing the Brooklyn work, it was considered preferable; and, inasmuch as the pilot system would contend with boulders successfully and this material would not flow, the pilot was thought better than the shield. The pilot system was therefore used.

The method of work pursued on Section No. 1, the open cut, has already been spoken of. Work was begun at the trap basin at Gowanus Canal and carried forward by derrick, bucket, and track for cars alongside the cut. Surface pipes were shifted, or slung, or troughs carried in their stead. On Fourth Avenue, the line of the sewer being on one side of the street, and the cutting exceeding 25 feet, the sheeting would yield enough to disturb the curb and sidewalks slightly, and it caused a few doors of buildings to show disturbance. The sheeting was 2-inch plank, driven by hand, and held by 6 x 8-inch rangers and braces, in the usual way. Interruptions of the work by rains increased the loss of this sandy material through cracks in sheeting and around ends of planking. Section No. 1 was completed expeditiously. It had a maximum cutting of 36 feet at Dean Street, where it joined with Section No. 2. The method of timbering the cut, the height to which the invert was carried before centering was put up, the centers and the arch, are shown in Plate XLVIII.

On the tunnel section, some other way than commencing at one end and working toward the other had to be adopted. A tunnel on comparatively level ground must be attacked differently from a tunnel through a mountain or under water. To do the work most economically there must be more points of attack. The distance the shafts should be apart is determinate, and depends upon the depths and consequent cost of the shaft, and the cost of transportation of muck and material. On the profile the shaft locations as numbered are shown. On this work the cars were pushed by men, and there was necessarily one passing switch in the tunnel near the shaft and another near the heading. When there was time, after putting a loaded car on the cage, to take an empty car from the siding at the shaft and put it on the siding at the heading before the last empty car taken from there could be shoveled full, the distance from the heading to the shaft was not too great for economy. Otherwise, a middle siding would have had to be used, and the pay-roll would have been increased without any increase in the amount of muck sent out. This limit of distance was about 800 feet, but good muck to shovel would decrease it. This gave, then, 1 600 feet between the shafts as the economic distance.

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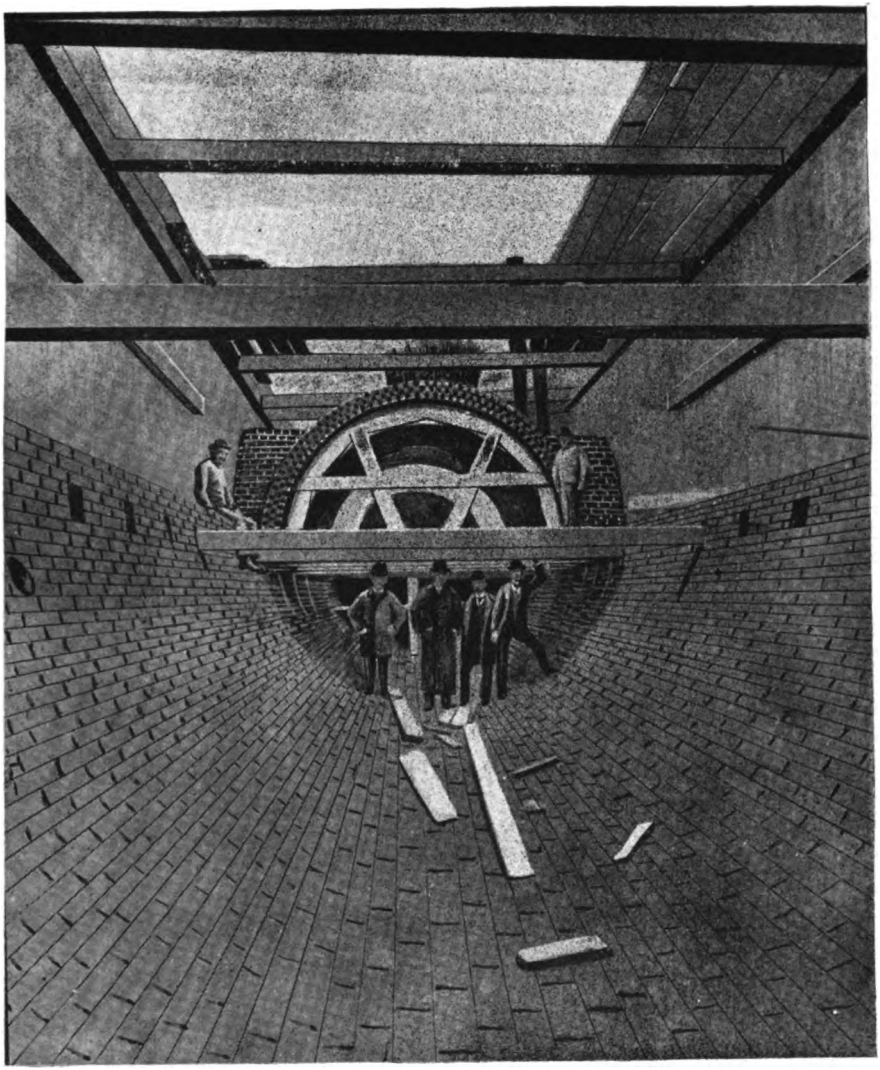
PLATE XLVIII.
TRANS. AM. SOC. CIV. ENGS.
VOL. XXVI, NO. 529.
BEAHAN ON BROOKLYN RELIEF SEWER.



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In crowded city streets there are other considerations than economy for the contractor, and manhole locations are to be taken into account in deep work. The variations from the distance named and the reasons for it are not essential here. The shafts were sunk over the tunnel in all cases, and exactly on the center line, save where street-car tracks prevented. They were of rectangular section, about 18 x 24 feet on top. The greater length was along the axis of the sewer, so as to give greater length of base line for prolonging the center line into the headings with the transit and plumb bobs. Two-inch sheeting, 13 feet long, and driven by hand was used. Rangers 6 x 8 inches were placed in three sets on each sheeting length. Two braces were put in, and in loose ground these were made double. Two cross-braces were put in and completed the set, and the space for the cage was formed within the inner braces. The guides to the cage and all top work at a shaft are as shown in the cut from a photograph at Shaft No. 3 (Plate XLIX). The cage was a wooden platform with a tram-car track across it. The muck was loaded into cars, pushed by men to and upon the cage, and securely blocked. The cage, with the car, was hoisted up to the trestle track, and the car pushed out on the trestle and dumped sidewise on screens of such mesh and set at such angle as to let the sand through the screen under the trestle, and carry the gravel across the screen to one side of the trestle, all without labor.

The brick, when needed, was loaded into these cars from the lower track on the street, placed on the cage, lowered down, pushed to the heading and dumped—there to be passed to the bricklayers.

The cars were made by the C. W. Hunt Co., and proved durable. On reasonably good track they were seldom derailed, but they are too wide on top for a tunnel car, and flaring sides are not desirable. In dumping brick or other material from them on a surface level with the track, one-third remains in the car and must be taken out by hand. A car should dump its entire load and get out of the way speedily. In taking out muck, the gravel stones, if not cleaned away with the fingers from the rails, would block a car loaded with one cubic yard so that five men could not stir it. Larger wheels would avoid this. A special pattern designed with these facts in mind could surely be made which would still retain the desirable features of the present car, which is the best now in the market.

The shaft sunk, the pilot is next to be started. We have seen that

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the pilot is cylindrical, usually about 6 feet in diameter, with rings 2 feet long, each ring consisting of six plates of equal length; all fastened together with bolts. The plates are of steel, with inside flanges riveted on them; the weight of metal and method of construction depend on the material to be met; they are used repeatedly, and enough for 40 feet of pilot is needed for a heading. The first two rings are set up in the shaft—blocking them up to center and grade lines. The sheeting is then bored through on the line of the pilot ring, cutting out the plank and rangers piece by piece, and working in the iron poling boards like those shown in the cut of heading work. These iron poling boards are carried down 2 feet from the center. A half plate is then put in, *i. e.*, a plate made half width for ease in holding up while bolting. We next pole down on either side, using a wooden poling board, to the center of the sides of the pilot and put a half plate on each side. Now, go back and put another half plate in the roof and again fill out the sides, bulkhead up the front face, and put in the three full bottom plates. This completes a ring, and is the entire process of driving a pilot—barring difficulties. The next ring is put ahead in like manner except that the first roof plate instead of being in the center has one end at the center. The horizontal joints of one ring are thus put in the center of the plates of the adjacent rings. The tools used are much the same as those shown in the heading work.

One man and a helper will keep the pilot sufficiently advanced; it should be kept on the center line for direction, and carried for grade at the right distance below the roof to allow the center heading man to stand upon the pilot and conveniently put in his heading plates. The pilot is guided by wooden wedges, the method of use and limits of efficiency of which are not pertinent here. When one pilot is a few rings inside the sheeting, the other may be started. A pilot being well under way, the heading is started over it by marking out the circle of the outer line of the brickwork on the sheeting, and boring the sheeting so as to take it out in small pieces. The heading plates are then bolted together in two rings and set up in the shaft; iron poling boards are brought into requisition, and the roof is worked ahead by the various and varying means and ways which experience alone can teach. The material just outside the sheeting is always loose, as it has been shaken in sinking the shaft. To start the work from a shaft without losing material, always requires care and

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PLATE XLIX.
TRANS. AM. SOC. CIV. ENGS.
VOL. XXVI, NO. 529.
BEAHAN ON BROOKLYN RELIEF SEWER.

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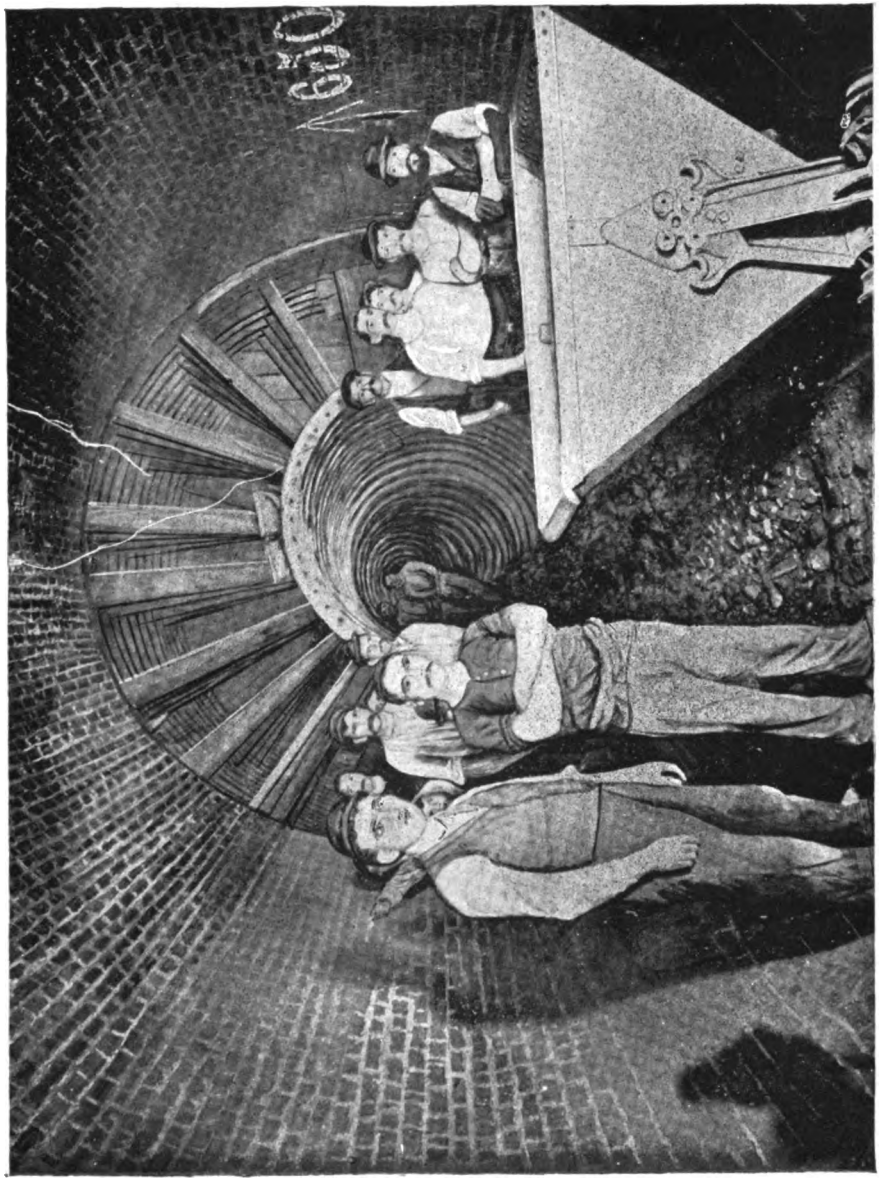
PLATE L.
TRANS. AM. SOC. CIV. ENGS.
VOL. XXVI, NO. 529,
BEAHAN ON BROOKLYN RELIEF SEWER.



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special safeguards. In a view shown of the heading (Plate L) the roof plates are curved to the radius of the outer circle of the brickwork, and to place them there, is the desideratum in heading work; they go together in rings 1 foot long, and are carried down as far as much side pressure is felt in the material met. At each second ring a bulkhead is built down in front and braces put from the last ring to the pilot. Heading plates are kept in direction, in grade, and in correct circle, all by wedging. In short, a wooden wedge is the fine point in tunneling by the pilot system, and with sufficient practice the pilot can be guided in any direction. Even Weisbach does not fully realize the potency of a wedge. The detail of heading work has no place here; no description should induce any careful man to try to do the work without having attended school in a heading. This is a pointed reflection on engineers who visit tunnels and go away to do what they think they saw done in the way they thought they saw it done.

The view of heading work (Fig. 2) shows some features which should

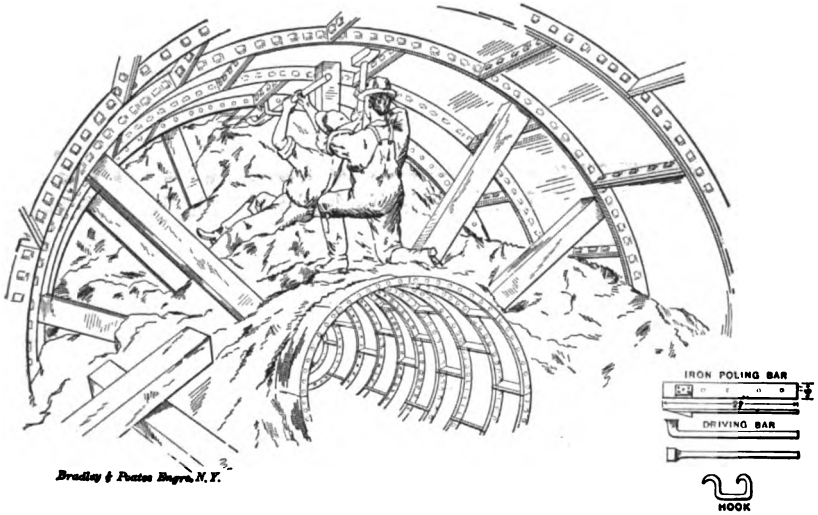

FIG. 2.

not pass unnoticed. The plate shows a narrow, thick piece of cast iron with a shoulder against which a crooked bar is being driven with a hammer. The illustration was taken from the Brooklyn work, and represents properly the iron poling board there in use. Iron poling boards have been tried in South Fifth Street and failed; recently, also, in Baltimore they have been used and abandoned. The one used here

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was a success, and was designed by Superintendent Samuel Matson early in 1891, while this tunnel was under construction. It is a steel plate, one-quarter of an inch in thickness, 6 inches wide and 22 inches long, and having a wedge-shaped lug riveted to its under side, near the front end; the plate also has four holes through it at intervals of its length. The lug is for driving it with bar and hammer, and the holes are to straighten it or shift it sidewise. The poling boards are used in the heading for some 4 feet each side the center line, and for the pilot work as described, their purpose being to save loss of material in running sand, gravel, or cobble stone.

Mr. Beach has recently claimed that this iron poling board was a "needle" of his, patented in 1869. From a copy of his patent, to which he refers by number, furnished from Washington, the author finds that in connection with his shield patent, there were "sliding staves" which are staves over the end of the shield. There is no kinship in appearance between his "sliding staves" and his "needle," as illustrated. Prior to this comparison, the author thought that the poling board used was Mr. Beach's needle re-invented, a case of double discovery, which so often occurs. Engineers should insist that superintendents and foremen be given full credit for thoughtful ingenuity that bears fruit. The members of our profession are too apt to appropriate to themselves credit for much that some foreman or laborer really did when they themselves were at their wits' end. Who knows the name of the laborer who hit upon the cooper's draw-shave that was such a help in the pipe clay at St. Clair? Did any one ever hear how the shield on the Sarnia side was gotten down to the portal when presumably every means known had been tried to no purpose thus far? In the earlier work of the Hudson River Tunnel, years ago, an accident occurred and nearly a whole gang of men were buried alive. Men stood aghast; engineers were consulted, each with a different way of meeting the emergency, and the President of the company was distracted by the conflict of views. Who speedily hit upon a plan, which was successfully carried out, for rescuing the bodies and restoring the structure? He was not an engineer.

These iron poling boards are useless in heavy material without some way of driving them. The driving bar was designed with the poling board by the same man. He used a double hook to suspend it from the plates and so hold them up. The hook shown is better and replaced the first one, and was gotten up by foreman Charles

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Erricson. It hangs in two bolt holes with a bar between, all in one piece. In stiff clay no iron poling boards are needed; in heavy boulders requiring much blasting they are often removed temporarily. Some skill is needed to keep them in the position desired. At the completion of the tunnel the iron poling was in great favor among both foremen and heading men. We have failed and will fail with them, mainly because we make them too large, and do not have the needed special appliances with which to handle them.

Excavation and brickwork must next be mentioned. When the heading is at least 15 feet in from the shaft, or from the toothing of the last section, a 10-foot section of brickwork is put in. The roof of the heading, lined with plates, is carried by braces on the pilot. The pilot rests for some 20 or more feet in the muck, and for the 4 feet of its length nearest the shaft on braces or blocking. The heading men start down with the excavation on the sides of the pilot and for a length of 10 feet. When they get below the bottom of the iron heading plates, they put in planks longitudinally on either side and brace these planks from the sides of the pilot. The planks are put in one below the other on the line of the outer circle of the brickwork until they are below the pilot, or where the material will stand. At the front ends of the planks a bulkhead is driven vertically, extending from the pilot to the side planking. The excavation is carried down under the pilot, and braces put underneath on foot blocks. A bulkhead is also put in underneath the pilot on a line with the front ends of the side planking, braced up, and enough braces put under the pilot to carry roof and arch. All the excavation is done carefully to the outer line of the brickwork. A profile for the invert is moved forward, set to grade line and its top cross piece leveled horizontally and set on center line; planks are put in the bottom unless the bottom be very hard, and the brickwork begun. There is no back filling or ramming. A sub-drain was planned, but a wooden box above the brickwork of the invert was used instead, and any water in the section was pumped up into this box. This was better, for the box could be taken up and cleaned out.

The best hard-burnt North River bricks were used, the ring being four courses thick. They were of a uniform quality of rough brick throughout the rings of the section. None were laid without cement, but in some very wet sections the bottom course was started dry and then

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buckets of neat cement were dumped on it and spread over to fill the joints. This gave excellent results, showing no settling or percolation of water. Each course was laid to line, and the courses were tested frequently with level and straight-edge. No pains were spared to insure a strong, workmanlike job, and no time was given to effect. The cement was Rosendale, tested to resist a tensile strain of 60 pounds per square inch after 30 minutes in air and 24 hours in water. It was mixed in the proportion of one part of cement to two parts of sand. A slow-setting cement was sought, and the laboratory tests were tempered with judgment. These never showed as high tensile strength as published tests do. Much force was given to results from briquettes long made. The Department has a man who for some time has done all such work, and his deductions are comparable with themselves. Only one kind of cement was used for any ring of the brickwork.

With the same brick and the same cement every particle of space, from the neat lines of the inner surface of the sewer out to the muck, plank or plates, was built in solidly with brick laid in cement. There is not and never has been any crack in the brickwork, or any failure at any time. Where built in wet material it has settled, but it was built enough above grade at the leading end to allow for it. A brick tunnel in yielding material is sure to settle, nor is it water tight. Leaking pipes above sometimes caused a drip into the large sewer, and it is questionable whether a brick tunnel can be made water tight when built through dripping ground.

The invert of the brickwork completed, the ribs were set, and, to save room, iron ribs and not wooden centers were used, resting on planks carried by braces, one end of which rested in "put-lock" holes in the brickwork and the other against the pilot. Four ribs were used, each in two segments bolted together at the center; lagging strips were run in on these ribs, four at a time, as needed; the two bricklayers on either side, then worked up to within four courses of the center. The last lagging had a groove in its upper edge, a block lagging was placed in these two grooves next the toothing; one bricklayer started then at the toothing to put in the key, and built it out to the end of the brickwork of the section just laid. This completed a section of brickwork. After the cement had set, the ribs were struck and removed, the lagging carried back and 10 feet of the pilot were

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taken down and stacked up. All was then ready for starting down the sides to excavate for the next section of brickwork whenever the heading was far enough advanced to allow it. As two headings were worked from each shaft, and heading and pilot men never left their places of work, a section of brickwork would usually be put in at one heading, and then one at the other heading.

The chapter of accidents is never long on successful work. It also removes from the chronicler all opportunity for displaying his profundity in theorizing how each accident must have occurred. Owing to a disobedience of orders, a hoisting engineer, the friction gear of whose engine had been just overhauled, let down a cage load of men when he should have tried his friction with a loaded car. Part of the way down, the men dropped too fast and some were slightly hurt. At another time a new top man fell into a car, or fell somewhere else on the cage, his cries excited the engineer, and the latter dropped cage, car and man, disabling all three. There was a foreman on the spot, or near there, who had a theory just how it must have happened, and it was a detailed theory. It was a theory, too, that he worked whichever way he assumed the man fell, and the one opportunity for advancing a theory about an accident on that whole contract was fully embraced. There was no accident in the tunnel.

Difficulties met, and how met, is in papers of this sort a topic that will bear amplification. Early in the work, a water pipe running parallel to and over the tunnel on Fourth Avenue gave way; the heading men were less trained then and had lost material needlessly. The break occurred at night, and the city employees did not at once find the proper way to shut off the pressure. Much water escaped, but no damage of moment was done.

At Franklin Avenue the surface pipe was stripped, and, during an unusually heavy rain storm one Sunday night, the surface pipe on Greene Avenue gave way where stripped, over the end of the pilot. The pressure probably lifted up the exposed pipes, as the present sewers are badly overcharged there. The trench filled, and finally the water commenced running through the pilot, carrying considerable material with it, causing a cave-in in the street. Had men been at work, the pilot could have been bulkheaded against it. Had the filled trench given enough head to the water, it might, however, have broken into the heading. This was a difficulty, on the whole, that could scarcely have

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been foreseen or guarded against. The pressure upon overcharged sewers is enormous in some localities in Brooklyn. The test shaft on Hanson Place at St. Felix Street showed a small body of water not far above the tunnel roof as already stated. It was a water pocket, but was thought by the contractors to be from a leaky pipe. As the heading reached and passed under it there was a heavy drip from it, which lasted until the spot was passed. This finally ceased, probably through the water having found a way down the brick work. This water pocket could possibly have been bled, *i. e.*, a pipe driven down from the street to it and a pump used to remove the water. In Baltimore this has been done frequently. In open cut work in Boston, below tide, the foundation is kept dry owing to peculiar stratification, by a battery of connected pipes to which a pump is attached.

We have already seen that near Shaft No. 1 the entire brickwork was set high. The arch was found to settle in all the loose material. The leading iron rib was set always 1 inch high, and then the brickwork at the toothing would be found to be at true roof grade after centers were struck. In sand and gravel—in anything but silt—the invert never settled, but the arch would settle an average of 1 inch. In one wet section, with boulders and sand, a roof was known to settle 4 inches in the 10 feet. Perhaps one-half inch of this settlement should be attributed to the centers and their supports taking a bearing. Apropos to the Chicago Tunnel, under investigation at the present time, this settlement seems pertinent. In the same connection the author desires to say that we never succeeded even with Portland cement in making the roof water tight when the roof was dripping fast as the brickwork was put in. The iron plate as a cap for the rib braces was suggested by foreman Charles Lindau, and thereafter used, and it saved from one-fourth to one-half an inch of the settlement where the roof was heavy.

We now pass from evils incident to too much water, to one caused by too little. Reference is had to a very fine, dry sand that will flow like water and pour through the least crevice in a bulkhead. It was first encountered in South Fifth Street several years ago. It could not be tunneled unless made wet. Flooding it by means of a pipe driven down from the street and a hydrant attached to it, failed for some reason. Next, a hose was attached to the water-supply pipe used for cement mixing, near the heading; a short piece of water pipe

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was fastened to the far end of the hose, the pipe was thrust into the sand at the roof of the heading and the water turned on. The water could not force its way in nor even soak in. Finally, a jet of water, was played upon the sand and a hole washed, into which the pipe was slowly advanced, and the sand was wetted. This worked very nicely, and the water jet was thus used throughout this portion of the work. Rarely does this sand extend all over the section in this drift material.

The care needed for water and sewer pipes overlying the tunnel depended much on the condition of these pipes. Slinging was resorted to for water pipes where danger was apprehended. The greatest annoyance was from cement sewer pipes, some of which were put down years ago by property owners and afterward accepted by the city. Having been laid under the supervision of men unfamiliar with such work, they were now found settled, broken and cracked, often leaking before the pilot reached a point under them. They were too broken to sling even on plank. Stripping them, constant tamping, Portland cement bottoms, and even plaster of Paris to caulk cracks were employed.

Under the worst-laid pipes the blasting was heaviest. Care in the heading, and constant watching and tamping of the pipes availed most. Boulders containing often a cubic yard with others to fit in the spaces, fine sand overlying and heavy rains surcharging all with water, go to make a bad surrounding in which to tunnel under a broken sewer pipe in winter. A deep hole, a small charge of black powder hard tamped and splitting the boulder into about three pieces, was found the best way to blast in the case described. The lower piece was then worked out and an upper piece let down a little so as to get in one poling board; a little more for another poling board, and so on. These rocks must be removed from the heading before pop-shotting. This is not the best way in all cases, but is best for this special one. The most useful device found for carrying water along a bad surface pipe or sewer, was a strip of heavy canvas, wide enough to come up above the water line and long enough to bridge over the broken pipe or crumbling brick. This was made tight to the surface of the pipe at the upper end by plaster of paris.

Things are sometimes seen that we had best admit we cannot explain, but unless our senses deceive us, they are facts. The author

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once heard a member of the St. Louis Engineer Club, when confronted with the foot-a-day creeping of the rails across the Ead's Bridge there, throw overboard the whole puzzling problem with an "I deny the facts." It was surely facing the issue, and he was a man to do just that. The author would like to face a fact about these pilots in that way. They turn about their axes, and the line of their longitudinal flanges, which separate the plates from each other, was a spiral. Every three weeks the plates had to be shifted one hole to the right, *i. e.*, six inches. Every pilot moved in the same direction, from right to left, when looked at with the back to the shaft from which the pilot started. The direction and rate of this movement was the same for each pilot on the work. Would bad punching of the plates do this? But the pilot plates were not all made at the same place or at the same time. Each plate was put in each time just as picked up; the probabilities are that it was therefore turned around every other time it was used. Left-handed or right-handed men made no difference. All possible orders of succession of poling and bolting were tried, and no difference detected. As we worked each way from each shaft one-half the pilots rolled in one direction, the other half in an opposite direction. In South Fifth Street, it is stated, the same thing was observed, the heading plates traveling to the left when they moved at all. The movement of heading plates was very irregular—usually much less than those of the pilot. As some of you know, the shields at St. Clair and the shield in the Hudson River Tunnel revolve, and in neither case did the effort to stop the tendency or find the cause, succeed. Of course, there need be no analogy here, for shields are moved by hydraulic pressure. They do not always roll to the left, neither is the rotation always uniform.

The instrumental work of the alignment and grades was uniformly excellent. The line of levels and bench marks having been carefully established in the street, it only remained to measure vertically down the shafts with a steel tape to transfer the levels to the tunnel. Bench marks were established in the tunnel with a wye level, but grade elevations for the work were given by a combined transit and level, thus avoiding the use of two instruments in cramped quarters. A grade tack was put in the invert every 30 feet, from which the inspector tested his bottom grade, and a grade mark was placed on the center of the roof to guide him in setting up his leading rib

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and to show the foreman how his plates must be made to run for grade. As the several headings came together the differences in levels were trifling.

The alignment was carefully established on the street and is shown in connection with the profile. The curves shown were built upon and the brickwork now stands true to them. The shafts were of different sizes. Usually, there was an available base line of 14 feet to be used in transferring the center line to the bottom of the shaft. Fine piano wire and 5-pound plumb-bobs were used, the latter steadied in a bucket of water. The transit was set up in the tunnel a little way from the shaft and shifted until its line of collimation passed through each plumb wire. A point was then established beyond the shaft in the tunnel. No point of intersection of the curves was outside the tunnel brickwork. Center points were given by tacks in the roof about 50 feet apart, nails were driven a foot ahead of each tack, and from these a long cord and a plumb bob enabled the foreman to get a center point in pilot or heading, or the inspector to center his profile for each section. The author desires to say for the benefit of his professional brethren, that centers and grades were given without any appreciable interference with the workmen. The errors in alignment found to exist as the various headings met, were too small to be now seen in the completed work. The least error in alignment was one-fourth of an inch, between Shafts No. 5 and No. 6; the greatest was 4 inches between Shafts No. 3 and No. 4. From Shaft No. 3 the line extended around two curves and for a total length of 1 400 feet to the heading. From Shaft No. 4 the line extended 780 feet to the heading, where these two lines met. These shafts were farthest apart and the long heading was on the same heading as the curves. This error of 4 inches under these conditions is not large, but first-class transit work; it certainly needed to be no better for the occasion. Mr. Wm. T. Brourton, the engineer in charge of the field work, also had charge of that work on the Knickerbocker Avenue Extension Sewer in South Fifth Street, Brooklyn. It is his judgment that for shafts giving a 14-foot base line in the tunnel and with those shafts 1 600 feet apart the maximum error in alignment of 2 inches and the maximum error in levels of one-fourth of an inch are reasonable errors.

On the profile is shown the progress in months from each heading after the work was well under way. The average progress in a heading for each 24 hours was 4.4 feet.

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It only remains to speak of the special constructions employed. The sewer under consideration is a relief sewer, and therefore has no street or house connections. At Marcy, Nostrand and Bedford Avenues manholes were put in and a connection made with the surface sewers. One of these manholes and connections, the one at Bedford Avenue, is shown in detail (Plate LI); it is typical in idea with the others, although each is built on a special plan designed to apply to the case. The end sought is to avoid disturbing the house drainage of the small sewer, but to take from the small sewer all water in excess of that from house drainage and to do this with the least impeding of the flow. The connection is brought into the small sewer at as easy an angle as possible. A single slab of stone placed in the line of the down stream side of the inlet, completely cuts off all flow above its lower edge. The excess water is led to the manhole and drops vertically to the invert of the tunnel masonry. This invert has previously been paved with granite blocks such as are used on streets; the granite replaces the inner rings of brick and is laid in Portland cement to the same inner lines. It extends for 30 feet in length and up to the springing lines, and manholes were put in at every shaft except No. 7, and at Marcy, Nostrand and Grand Avenues as well. Where the diameter of the tunnel was changed from 12 to 14 feet, it was done in one 16-foot section of brickwork. Raising the ribs on the small section and lowering them on the large section started the brickwork in the changed direction and gave a gradual change of section when finished. The curve used in turning into Greene Avenue was a long curve, and put in with straight 10-foot sections of brickwork, each section of which was a chord to the circle. This was thought preferable in every way to a short circle and curved brickwork. It impedes flow less, is easier to lay brick to, and a pilot can very easily be turned around it by wedging only. It is better than a short curve, an angle, or a turn in the shaft.

The manholes on Section No. 1 are smaller and more numerous. The I-beams rest on a granite coping on the side walls of the flat roofed portion; between the beams are flat brick arches carrying the street, and all possible head room was economized. The trap basin is worthy of close study. The illustrations are so complete that it is necessary to touch only upon a few points not shown, but evident on reflection. The size of this basin, 60 feet by 84 feet and 16 feet high, is realized only

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when one is in it. Its duty is to receive the entire discharge; pass it through a battery of cast-iron pipes lying in a horizontal row; conduct it over a bridge wall, and then through a second and similar battery of pipes into tide water at the head of Gowanus Canal. To insure that the sewer outflow be trapped at any possible stage of the tide water is the object of the bridge described and this is apparently an unique method. The top of this bridge is 1 foot above mean low water, while the bottom of the sewer invert is at mean high water.

The Main Relief Sewer of the City of Brooklyn is an accomplished fact, well planned, successfully accomplished in a short time without a single accident or misunderstanding. In the securing of data for the preparation of this paper, the author is greatly indebted to Mr. John P. Adams, Commissioner of City Works, for the information kindly placed at his disposal through Mr. Robert Van Buren, Chief Engineer. His sincere thanks are also due to Mr. L. Russell Clapp, Assistant Engineer in charge, for kind assistance and time cheerfully granted.

DISCUSSION.

ALFRED F. SEARS, M. Am. Soc. C. E.—This paper is a description of the plans of the Main Relief Sewer of Brooklyn, with a statement of some of the expedients resorted to for overcoming difficulties in its construction. As an agreeably written, historical sketch it is pleasant reading. It is much to be regretted that the author did not enter more fully into the details of the work, some of which I venture to suggest. This would have added greatly to its value to all who are engaged in this class of Hydraulic operations.

First.—We are told "that the maximum rainfall per hour is 4 inches, and that, of this, 1 inch per hour reaches the sewer as a maximum." But the statement is also made that this 1 inch per hour "is assumed." After this, we are given an example of the application of Kutter's formula with Flynn's improvement. Now, my comment on this point is, that the application of the formula goes without saying; having a certain amount of water to accommodate, we know exactly where to go in our books to find the size of the conduit. But it would be extremely useful to have the course of reasoning by which an engineer determines that 1 inch out of a shower of 4 inches will reach certain points along the street gutters, and not 2 inches nor simply a half-inch. If Mr. Beahan will give us this argument he will do a great

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deal for the advocates of what is known as the "Combined System of Sewerage," provided, always, he can show anything more than the guess-work, which has so often brought to grief the construction of sewerage works otherwise sound.

Second.—We read that "the cost for timber would be great in Brooklyn." Now, this is a very wide country, and Brooklyn is on the extreme eastern edge of it. The papers of this society must be made as useful to me on the Pacific coast where I can have the best yellow fir delivered on my work at $9.00 per M. B. M., and iron costs me, when cheapest, $50 to $60 per ton, as to you who live right here and know, perhaps, what Mr. Beahan is talking about, when he says "cost is great," which I certainly do not; if he will give us a detailed estimate on carefully drawn plans, of the cost of constructing these different designs, then we can apply the figures according to the geography of the case; without such detail the value of the statement is very limited.

Third.—The sole value to this Society, of papers telling us the story of construction, lies in the minuteness and accuracy of their details. If Mr. Beahan will tell us the number of days' labor, the class and cost of the labor it took, to do certain excavations and back fillings; also the cost of skilled labor and of the materials employed, this paper will be of much greater value. I hope he will see his way clear to do this, for which he is so competent, and so place us under greater obligations to him.

A. Fteley, M. Am. Soc. C. E.—I have read the paper that you have heard, and I might add a few remarks. One thing that I see by this paper, with a great deal of pleasure, is that the cost of the work as it stands to-day has been within the limits of the original estimate. This result upon a work that has presented so much difficulty and difficulties of such various kinds, it seems to me is a result that we must all consider as very flattering to the engineer who planned the work.

I notice in the paper a few remarks made about the reliability of soundings. I believe that in the work under consideration there were some errors shown and there were some difficulties encountered thereby, but I may be permitted to say, that, having had under my observation a great many soundings, there are means of making soundings by which the ground can be explored with a great deal of accuracy, so as to give to the engineer a good idea about the work that is to be done.

I notice also that in the computations of flow, the Kutter coefficient n has been estimated as being .015. I believe that figure is altogether too high; from data I have of the work under consideration I do not doubt that the coefficient would certainly be as low as .014, and considering the important bearing of the coefficient I do not doubt that the flow ought to show more than represented in that calculation.

I notice also that the maximum error in the tunnel in alignment has

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been considered as being a maximum of 2 inches in a shaft presenting a base of 14 feet for a heading of 1 600 feet. I do not myself attach a great deal of importance to the absolute accuracy of alignments in work of this kind; sometimes a great deal of time is spent in getting absolute accuracy. I should say we have lately in tunnel building attained some results very much more exact. We have seen many results in which the error was a very small fraction of the maximum mentioned for a heading as long as 7 500 feet.

It may be permitted also, as I see there is not present any engineer who had anything to do with this work, to say, as it was my good fortune to be consulted some years ago when this scheme was planned, that the study of the present sewer required heroic treatment. The engineer who recommended this sewer had also recommended another portion which has not been accepted. All the portion of Brooklyn which adjoins the Wallabout is so very level that when a large amount of flow reaches that region, the streets are very much flooded, and it is very difficult to prevent flooding. It was suggested that the whole district might be raised a number of feet. It was not then very thickly built upon, and the expenditure would have been within some reasonable amount. As an engineer, I am sorry that the details of the matter were not more fully considered than they were. I don't doubt, however, that a portion of the trouble there would be relieved by the fact that a portion of the overflow water of the district will be taken care of by this Greene Avenue sewer.

On the whole, this work has been very satisfactorily conducted throughout. We have heard little of it because it was so well conducted that there was not any accident. It was a great work; it was carried on under the main street and under heavy buildings, and no trouble was experienced at all, and it does a great deal of credit to the people who have planned the work.

Mr. BEAHAN.—Do you consider that .014 would be about the right coefficient for North River brick in joints not struck?

Mr. FTELEY.—I must say that I have not seen the last part of the work. I saw it at the beginning, and, judging from other structures of the same kind where the joints were not struck, I believe that .015 is the very extreme coefficient that ought to adopted, and it should be very rough indeed to obtain that figure. On a very large work in which I am now measuring the water, I find, within the last two or three months, that from .0146 to .0142 is about the limit.

A. McC. PARKER, M. Am. Soc. C. E.—I once had to sink a shaft about 120 feet along the footwall, through a pile of loose material made up of earth, dirt and broken rock, some of which was in very large masses, in a pit at the Tilly Foster Iron Mine. A pit 8 x 10 feet at the bottom was dug in the top of the pile as deep as it could be safely carried down. In the bottom of this pit a shaft set, made of flatted

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round oak timber, 10 inches in diameter at the small end, was laid at an angle of about 80 degrees with the dip of the shaft. Corresponding sets placed 4 feet apart were placed above this, up to the level of the top of the pile of loose material and then 2-inch oak poling boards, not over 9 inches wide, were driven all around and the pit filled in and tamped thoroughly. The lower set of poling boards was then driven, one by one, until about 6 feet had been excavated, when another timber set was placed and a new set of poling boards started. When the poling boards were driven they bore against the dirt on the outside of the cap of a lower set (tending to crowd the cap toward the wall), and on the inside of the cap of an upper set (tending to pull it out), but this pull was resisted by the pressure against the boards already driven; consequently, the harder the boards were driven, the stronger the shaft became. The sets were partly supported by the friction of the poling boards, and partly by setting them, not normal to the dip of the shaft, but at an angle considerably above the normal, so that, as they tended to settle down at the cap end (the foot being supported by the foot wall), they became longer in a direction normal to the dip and consequently exerted a thrust against the overlying dirt; hence, the greater tendency to settle, the stronger the shaft-timbering became.

O. F. NICHOLS, M. Am. Soc. C. E.—This is, so far as I know, one of the first occasions in which this "pilot system" of tunneling has been presented before an engineering society. I regret that Mr. Beahan has not gone into the question of cost. It would have been extremely interesting if we could, at least, have had the average number of men employed at each heading. I am still in hopes that the engineers who designed and inspected this work may, in their reports or otherwise, give us many valuable details which are wanting in this paper.

The system seems to me to have very marked advantages in certain places, and Brooklyn seems to be one of the most suitable localities for its application. The same system was used by Mr. Anderson, I think, on the South Fifth Street sewer, which was one of the earlier sewer tunnels in Brooklyn. There had been great settlement on that work, and many difficulties in open work and tunnel. This system was adopted, property was saved from destruction, and the contractors avoided serious financial loss.

I have observed the work on the Greene Avenue sewer with great interest and mainly as one who would be seriously affected if it should fail during construction. The work has been pushed rapidly and with great care and skill. This Long Island material is, as we all know, a compact drift of fine sand, gravel and boulders. Where the material is sand, it is often clean, sharp and compact, and, under these conditions, this method of tunneling has been very satisfactory when the ground could be kept dry. Whenever boulders have been found, the compacting of these together in mass has arched the material over the tunnel

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in a way to avoid great settlement and loss of material. The settlement of the street surface has generally been very slight, no buildings have settled, no structures have been jeopardized. At Flatbush Avenue an elevated railroad foundation within the settlement area settled less than one-half inch, the moving load having been taken off during construction, at this point the general surface settlement had been considerable, due to the necessity of pumping from below the invert. At Grand Avenue, of six foundations of the same kind, nearly over the sewer, one has settled not more than an inch, and the others have not settled at all. The material here is compacted boulders, and pumping was not necessary. Altogether, I doubt if corresponding results over such a length of tunnel could have been secured by any other method.

A disadvantage of the system would seem to be that so few men can be worked in the headings, the pilot and braces fill up so much of the space for a considerable length of tunnel. A great advantage on the other hand, particularly for cases of this kind in crowded streets, is that so little constructive material had to be handled through the shafts, no heavy or long timbers being required. The iron segments are taken in and remain there in progressive use with a minimum amount of timber, until the headings meet. Messrs. Anderson & Barr have had long and varied experience in the use of this pilot system and have reduced the delay, danger and expense to a minimum. I am, on this account, the more desirous that some one should give us details of the cost of this particular work, that we may compare it with other systems with which we are better acquainted.

Mr. Beahan makes sweeping criticisms of the shield system. It must be remembered that shields have generally been introduced where the loss of material and lives have been considerable before they were introduced. Such was the case on the old Thames Tunnel, the Hudson River, the Mersey and the St. Clair Tunnels. The engineers were not justified from previous experience, in the St. Clair Tunnel, in using any other method, and the repeated failures to get beyond the bulkhead line on the New York side of the Hudson River Tunnel, certainly justified the proposed resort to a shield as the safest method of doing this work.

The use of a shield in compact rock, as proposed for the deep rapid-transit tunnel in New York City, is simply absurd. The various attempts to tie us down to some particular patented shield are ridiculous. Shields like everything else are subject to betterment based on continued use in various materials. A perfect shield is therefore more likely to be the result of future development than of past patent right. Mr. Beahan becomes unnecessarily sarcastic in his preference for the practical school, and in demanding recognition for the services rendered by skilled workmen. No sensible engineer will theorize over the execution of a work of this kind. When he plans and projects, he neces-

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sarily uses theories and formulas; when he meets material in the physical sense, he meets it in a practical way with every agency at his hand, to overcome its resistances, and from Brunel to Fteley no ingenious device for accomplishing rapid, economical and, hence, successful work, fails of prompt recognition; and no laborer, however humble, who can add a valuable suggestion to the sum of engineering experience, fails to find applause from the conscientious engineer. "Let us be honest," says Mr. Beahan, and in saying this he implies an unwarranted arrogance and iniquity to a profession which is, of all professions, the most honest and the quickest to detect and expose fraud. Two foreman find recognition in this paper. Unless the work is very different from other work, there are others whose quiet efforts may have deserved it more. Published recognition is what the best workers, as laborers or engineers, seek last; it is the lesson of all biography, that modesty is the crowning attribute of the greatest. Only the time-server, the self-seeker, the vain-glorious and the schemer fail of that which is the best and often the only recognition of the faithful worker—the consciousness of duty well done under every trial.

CHARLES B. BRUSH, Vice-President Am. Soc. C. E.—The pilot system has been referred to a number of times this evening and it has also been discussed at considerable length on other occasions. It may be interesting to review the history of this system.

In the commencement of the construction of the Hudson River Tunnel a shaft was sunk 30 feet in diameter and 60 feet deep. The idea was that opposite sides of the shaft could be removed and the tunnel started directly in the silt. Large rings were built in the sides of the shaft as it was sunk, and provision was made so that the brick work could be taken out from inside of these rings and the tunnel started from these points. The attempt resulted in failure. An opening was made through the shaft, the excavation was commenced in the silt and for one or two days it worked very well. The opening was excavated for a width in the top of the heading of perhaps 12 feet and the heading was kept in place partly by timber bracing. On the third day it was noticed that this was failing and on the fourth day it failed entirely. An air lock had been placed through the brick work in the side of the shaft, and air under pressure was forced into the opening made into the silt. It was hoped that, without any further protection, the air pressure would be sufficient to hold the silt in place. The timbering to which I referred was very slight.

A number of methods were then considered. A roof was built over that portion of the tunnel where the excavation had been made, and this roof was covered with timber and some canvas, and again they attempted to make the excavation on the same principle, that is, to sustain the roof and sides simply by forcing air into the excavation,

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but it was unsuccessful. This occurred and was described in a paper presented to this society, and read May 25th, 1880.* The failure took place, as recorded in that paper, on January 5th, 1880.

The next thing that was done is described on page 267, viz.: "Plates for an iron tube 8 feet long and 6 feet 4 inches in diameter were ordered, the plates to be ½ inch thick, 2 feet 6 inches wide and 4 feet long, with 3-inch angle iron flanges riveted to the four sides of the plates." They were made just large enough to surround the airlock as it projected through the side walls of the shaft. These plates were put in position exactly as described, and thereafter no difficulty was found in working the material.

The question then arose as to how much area of heading could be satisfactorily worked in this way. For the purpose of determining that point, a series of concentric rings were then constructed and placed in position, gradually enlarging in diameter until the full size of the tunnel was reached. It was found that these rings assisted in keeping the material in place, but that the size of the original tube was the most convenient for use in making the heading. This original iron tube was the beginning of the whole pilot system, and it was proposed and designed by me. It was used during the balance of the construction of the tunnel while Mr. Anderson remained as Superintendent. What I have stated are simply historic facts.

Mr. BEAHAN.—I am perfectly aware that I am not giving any history of the pilot, nor am I here to answer the gentleman as to who invented the pilot or as to whether any man ever invented it all by himself. I do not know. I have not called it the Anderson system at all, as you will find; nor do I mean to disclaim anything the gentleman said. Mr. Anderson is not present; Mr. Brush has the floor and has the advantage of me. In reading the paper that Mr. Brush refers to, I never noticed that that was a pilot. It is a fair section of that tunnel, the concentric rings run from a small to a large one, and, as it started from that shaft, I don't see that it was a pilot. The idea of the pilot is that of a small experimental tunnel in advance of the main tunnel. One of the reasons, I suppose the first reason for using it, was to find out the nature of the material in advance; if there is bad material, the pilot will run into it and it can be prepared for. The pilot can be bulkheaded easily, when a tunnel 20 feet in diameter could not.

As to this discussion to which Mr. Brush has referred I confess I am not equal to it in any way.

Mr. BRUSH.—A tube about 6 feet in diameter proved to be the most convenient size for working with the advance tunnel, and with such an advance tunnel it was found that the work could proceed much more rapidly and the proper section of the tunnel could be more accurately obtained. It was also valuable for the purpose of exploring.

* *Transactions* Am. Soc. C. E., Vol. IX, p. 259.

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T. H. McCann, M. Am. Soc. C. E.—Mr. Beahan states that the "pilot system" of tunneling was first introduced by Mr. J. F. Anderson, Superintendent of the Hudson River Tunnel, in 1880. At the beginning of the work of this tunnel, in 1880, I was in daily attendance with C. B. Brush, M. Am. Soc. C. E., the Chief Engineer, and generally present during the discussions of methods to overcome the various difficulties that presented themselves from time to time. I have always had the conviction that the suggestion of the "pilot" was first made by Mr. Brush.

It is true, as Mr. Beahan states, that Superintendent Anderson introduced the practical working of the "pilot system," and there is no doubt that his skill and courageous energy made the experiment a success from the start. Now, whether the germ of the idea originated in the brain of Mr. Brush or that of Mr. Anderson will, I suppose, to engineers a thousand years hence make little difference, but it is only just that this testimony be entered here in order to render honor to whom honor is due. As all of us very well know, during a discussion in which several original minds are engaged on some difficult and intricate problem, new and novel ideas will be suggested; but if any one of the participants in the discussion were to be asked some years afterwards to whom a certain idea in the solution should be accredited, it is ten chances to one that he would not be able to remember.

The principal use of the "pilot" in the Hudson River Tunnel heading was for doing away with long stud bracing, and for assisting the workmen in keeping the iron plates closer to the normal cross-section. The second purpose was to act as an explorer. These are virtually the same purposes for which it was employed in the Brooklyn sewer as described by Mr. Beahan. It was at the very first excavations made for the Hudson River Tunnel that the introduction of the "pilot system" may be considered as having been made. But it was not until after the work was well under way that the system as described by Mr. Beahan was fully developed.

A brief description of the tunnel work will probably not be out of place here, although a full account may be found in the paper of Messrs. Spielmann & Brush, read at the Twelfth Annual Convention of the Society, May 25th, 1880. A brick shaft, 30 feet in diameter, was sunk to a point 60 feet below high water on the Jersey shore of the Hudson River. This was refilled for a depth of 30 feet, and at that elevation a horizontal air lock 6 feet in diameter was placed; the intention being to use (and, so far as I know, for the first time) compressed air horizontally. After the air lock had been placed in position, iron plates 2 feet 6 inches wide and 4 feet long, were forced out into the silt at the heading, so as to form a tube 6 feet in diameter, which was carried on for a distance of 12 feet. This 6-foot explorer proved of valuable service, for the silt being overlaid with ashes and loose soil

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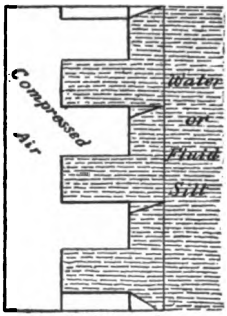
(with which the land forming the bulkhead was made) permitted the air to escape and a cave in was the result. An excavation to the depth of about 5 feet was then made on the surface of the ground and a sheet of canvas heavily weighted was placed over the heading. This was a success, and we were thus enabled to keep the air pretty well confined in the tube.

As each successive ring was constructed with the iron plates, the diameter was gradually increased—always keeping the top level—until a diameter of 18 feet was attained, and in this way a cone was gradually formed which at the end of 40 feet brought us to the grade of the bottom of the tunnel, about 60 feet below high water. The excavation was then continued with this diameter, keeping the tunnel to its proper grade, the brick lining 16 inches thick following up as close as practical to the heading. In this manner the tunnel was carried on for about 200 feet, but as it had been found almost impossible to keep the iron rings to their true cross-sectional form, even with the combination of compressed air and timber bracing, it was then that the "pilot" idea, as described by Mr. Beahan, suggested itself. As soon as the "pilot" was introduced, better workmanship and a truer cross-section was obtained, and was continued successfully in use during my stay on the work.

W. R. Hutton, M. Am. Soc. C. E.—The shield when used for tunneling has a double duty to perform. It must protect the face from the inflow of material, and it must support the perimeter while the lining is being built. Brunel's, the first shield, accomplished both these purposes. That of Barlow, in 1863, was needed for the second purpose only, as the workmen stood in front of the shield and excavated a way for its advance. The Beach shield was originally designed for sand, dividing up the natural slope of that in the face, into several lower and therefore shorter slopes by horizontal shelves. Evidently, therefore, the Beach shield was not used in the South 5th Street Tunnel (in Brooklyn) by Anderson & Barr.



Section of Baker's shield.

The St. Clair, and the Greathead (City and South London) shields have some further improvements, and a shield designed by Sir Benjamin Baker contains a "compressed-air-seal," as it may be called, between the double diaphragms. Although the material through which the St. Clair Tunnel is made—a soft blue clay—is very different from the silt in which the Hudson Tunnel lies, yet previous experience in attempts to tunnel the Detroit River (see *Transactions* Am. Soc. C. E., Vol. II, 233), fully justifies Sir Henry Tyler and Mr. Hobson in the use of a shield. They deserve great praise for the skillful design and management of it.

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Although compressed air has been used with the shield in the more recent works, the latter is not by any means a "pneumatic appliance." Except at special points, the air pressure was used in the St. Clair Tunnel, not to support the face, but to balance the pressure of the gas in the gravel with which the blue clay was underlaid, and which caused the failure of the previous attempts to tunnel under this river. The original Beach shield was not aided by air pressure. Presumably, that used in South Fifth Street was not, and air was not used normally in the City and South London Tunnel. In the Hudson Tunnel it is probable that an excess of material would flow in through the openings in the shield if air were not used, but no attempt was made to dispense with it, because the men—the older ones, trained under the Haskin system—thought the safety of the work and of themselves depended on a good pressure of air. If, as the author states, the shield is not indispensable in tunneling through the Hudson River silt, it would be interesting to know what method he would substitute. A return to the "pilot system" would be not only unwise, but criminal.

WILLARD BEAHAN, M. Am. Soc. C. E.—In reply, I will first answer the remarks made by Mr. Sears. As to the items of cost, I have given the progress each month from each heading, but have not given the number of days' work it would take to do it. Contractors do not usually give fuller information.

As for the assumption that one inch an hour reaches the sewer in a 4-inch rainfall, I cannot give the data to-night on which it was based; it was, of course, after a series of experiments. That was given me by Mr. Van Buren as what they had found through a long term of years, something like thirty-five, that 4 inches was the maximum rainfall. I should suppose Mr. Sears assumes that one-half of that rainfall goes off every hour. This sewer does not take all of the storm water of that portion of Brooklyn that it relieves. If you will look at the plan of the manhole you will find that considerable of the outflow of the older portion is still overflow of storm water. As to the cost of timber, it is variable according to the time and place where it is purchased.

The question has been asked whether there was any difficulty in keeping the poling boards close together. There was none. The difficulty with an iron poling board is that it is inclined to drop down on the side; in the roof they are quite as apt to drop down as to drop away from each other. The reason why I think they succeeded in this case, and yet have failed when used elsewhere, is that we have a smaller poling board, and it can be moved back to place. Boards 4 or 5 feet long have been used, but cannot be kept in place; they get out of position and drop down. At Baltimore they tried such poling boards and pronounced them failures, but the small ones used in Brooklyn were certainly a success.

Mr. Hutton, in his discussion, says, "to return to the pilot system

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in the Hudson River Tunnel would be criminal." Were any men killed in the Hudson River Tunnel when the pilot was used? Mr. Moier has stated to me that had they any men on the work now who were familiar with the pilot system they could get along all right without a shield. Still, I think it is proper to use a shield for the Hudson River Tunnel. The pilot was invented there for that form of material; still, I think if I was tunneling out under the Hudson River, with the possibility of coming out to clear water, I certainly should want a shield. Sir Benjamin Baker claimed for the water space between his two shields that it was perfectly practicable to tunnel through clear water with his system, but Mr. Moier assured me that when Sir Benjamin attempted to do so *he* would rather be on the top of the river in a boat.

The question is also asked whether much material slipped into the tunnel from above the top of the arch. There was some such slipping, as could be seen by the depression of the streets. Through the heavy cut between Shafts 4 and 5, we lost none that we know of; the amount depends on the material passed through. If the material will stand a half a minute, there will be but little material lost, but if there are boulders above the arch (and I have seen them so thick that nothing else could be seen), then the water would come in with the sand and keep carrying it down all the time. We could not protect ourselves when we had boulders with water, and naturally we would lose much material. This, it may be noticed, is not a machine method, but a method of manual labor, and the kind of men and the kind of foremen employed make a great deal of difference in the result.

No water pipes were broken from the settlement of the earth, but there were some sewer pipes broken; these were laid badly and dipped up and down and settled in various ways, and they were leaking before we got to them.

As to the nationality of the laborers which has been enquired about, we had more Swedes than anything else among the heading men, because the Scandinavians who come here are mostly sailors and of a more mechanical turn of mind than other laborers, and can be broken in better. The next nationality was Irish, some of them were the best sort of men, but they have the common failing of all Irishmen after pay day.

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(Vol. XXVI.—May, 1892.)

DISCUSSION ON PAPER No. 511 ON THE BRAZOS RIVER HARBOR IMPROVEMENT.

By WILLIAM P. CRAIGHILL, M. Am. Soc. C. E., and
E. L. CORTHELL, M. Am. Soc. C. E.

WILLIAM P. CRAIGHILL, M. Am. Soc. C. E.—In the interesting discussion of Mr. Wisner's paper on the Brazos River Harbor Improvement, I find Mr. LeBaron has fallen into an error, on page 554, in saying that certain mattresses there described were designed by General Gillmore. They had their origin on the Cape Fear River in the following way: We were trying to do what my predecessors had been working at occasionally for nearly a century, viz., to close the New Inlet, a mouth of the Cape Fear which had been open about a hundred years, to the great detriment of the original and proper mouth at the Baldhead. In that century it had become nearly as good a channel as the old one. It was a mile wide and quite deep, and there were the tides and waves of the ocean to contend with, as well as the currents of the river.

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It was decided to effect the closure by dumping stone and thus forming a dam. As usual in Government operations of this kind, it was necessary to work with limited means and as economically as possible. It was thought necessary to lay mattresses of some kind for the dumped stone to fall upon. There were no facilities for speedily constructing the regular mattress which had been so extensively used on the Mississippi River and elsewhere in this and other countries. North Carolina, as is known, abounds in pine, and of this wood there was a heavy growth on the banks of the creeks emptying into the Cape Fear near the New Inlet. The idea occurred to me to make rafts of these pine logs, by cutting them on the banks of the secondary streams, and rolling them into the water. Upon these rafts brush from the tops and branches of the trees was placed so that each raft carried a full load and had its top just level with the water. They were towed into position and sunk by dumping stone on them. The idea was adopted by one of the bidders and his bid was founded upon it.

Later, he became a contractor on the jetties at Charleston and the same kind of mattress was used there under the direction of General Gillmore. It is cheap and answers in some respects quite well, but it has, however, defects. It is rather too rigid except for very soft material. Sometimes, unless the mattress be entirely sunk in the material at the bottom, there is a hurtful escape of water through the raft between the bottom logs, producing scour where not wanted. Sometimes, in a heavy sea, such as frequently prevails on the bar at Charleston, a raft is not sunk flat or becomes partly broken, and detached logs rise to or near the surface like snags or sawyers and become dangerous to passing vessels.

This improvement was quite a successful one. An interesting account of it may be found in the Annual Report of the Chief of Engineers of the Army for 1886, pages 1004–11. When the new inlet was closed, the old mouth, six miles distant, increased in depth from 9 to 15 feet at low water.

A curious piece of the history of this form of mattress is, that the contractor referred to had the brass to take out a patent on it, and demanded damages for the use of his patent. My impression is he never received anything from General Gillmore, and I am quite sure he never did from me.

This economical operation in the line of producing a cheap mat-

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tress, reminds me of another practiced years before on the Susquehanna River, near Havre-de-Grace, where are now the fine bridges of the Pennsylvania and Baltimore and Ohio Railroads. In order to maintain the navigation of the main channel to Havre-de-Grace, it was necessary to close a secondary channel. There was but little money available. It occurred to me to utilize the products of that country also, as was later done on the Cape Fear. An old canal enters the Susquehanna a little distance above Havre-de-Grace. In that vicinity were a number of old canal boats of little value, that could be purchased for little money. A number of them were bought and towed to the quarries of Port Deposit, which is quite near. They were cheaply loaded with as much stone (quarry groat) as they would carry. They were then towed into position and, by the addition of a little more stone carried on auxiliary scows, were sunk in one or more tiers. The operation was very cheap and the result very good.

In this same vicinity there was a question of improving the wide stretch of the Susquehanna below Havre-de-Grace, by regulating works, although the fishermen objected very much to anything of that kind that would interfere with nets. A movable deflector or training wall was built with the view of determining the best position for a permanent one when more money should be available. The experiment gave valuable information, and the materials of the movable training wall were later used in a fixed one.

We are now wrestling with the problem of preventing the ice gorges which occasionally form at and near Havre-de-Grace on this same river —Susquehanna—of which very disagreeable effects are felt at Port Deposit, several miles above.

E. L. Corthell, M. Am. Soc. C. E.—Under date of June 7th, 1892, Mr. Corthell writes: "I have recently examined the Brazos River jetties, and although incomplete and the last 2 000 feet under water from 3 to 6 feet, there is now a channel 20 to 25 feet deep entirely to the end of the jetties, with only one sounding of 18½ feet, with 19 feet each side and 25 feet within 400 feet of the ends of the jetties.

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(Vol. XXVI.—May, 1892.)

BRIDGING CAÑONS LENGTHWISE.

By HOWARD V. HINCKLEY, M. Am. Soc. C. E.

It is not my purpose to present a treatise on how to bridge cañons lengthwise, but simply to show the peculiar features of bridges that have been used in two instances in the solution of this problem.

Case 1 is in the Apache Cañon of the Rio Galisteo, on the main line of the New Mexico and Southern Pacific Railroad (Santa Fé route), just east of Lamy, in Santa Fé County, New Mexico. This point is at mile 832 west of Atchison and 1 307 west of Chicago.

Fig. 1 is a map of alignment and river crossings.

Fig. 2 is a profile of river crossings.

Fig. 3 is a plan of the bridge.

Fig. 4 is a cross-section of the cañon showing center support of bridge.

The ends of the bridge rest on abutments on opposite sides of the cañon. The center line of the track is on a ten-degree curve, and is 14½ inchés off the center line of the plate girder bridge at the center and at both ends of the span.

Case 2 is in the Royal Gorge (of the Grand Cañon) of the Arkansas

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River, 2 miles west of Cañon City, in Fremont County, Colo., at mile 1 135 west of Chicago. This bridge is popularly known as the hanging bridge.

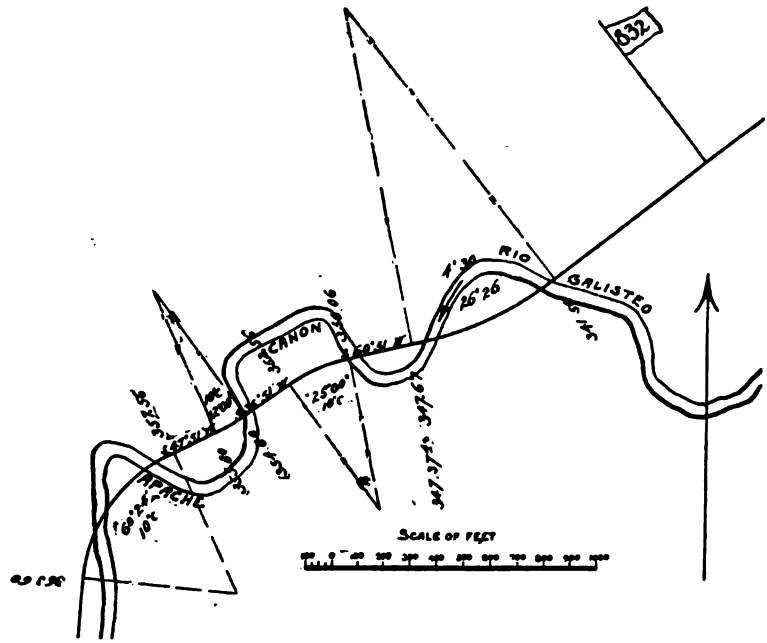

FIG. 1.

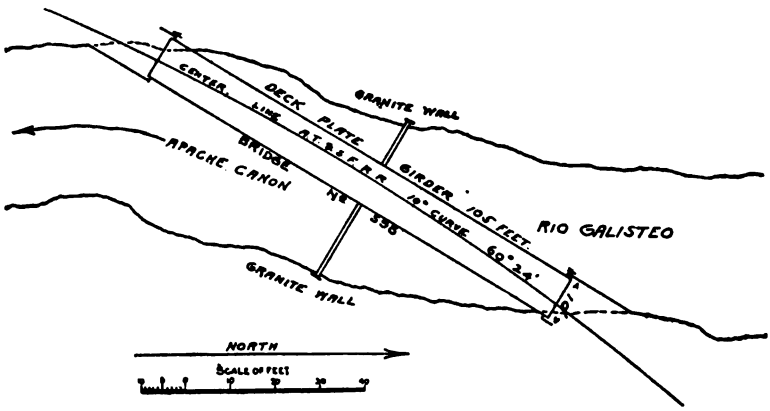

FIG. 3.

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Fig. 5 is a cross-section of the cañon showing the east "arch," looking westward.

Fig. 6 is a photograph of the bridge looking eastward.

Possibly some of the members who were in the excursion party following the Denver Convention of 1886, will remember that they rose

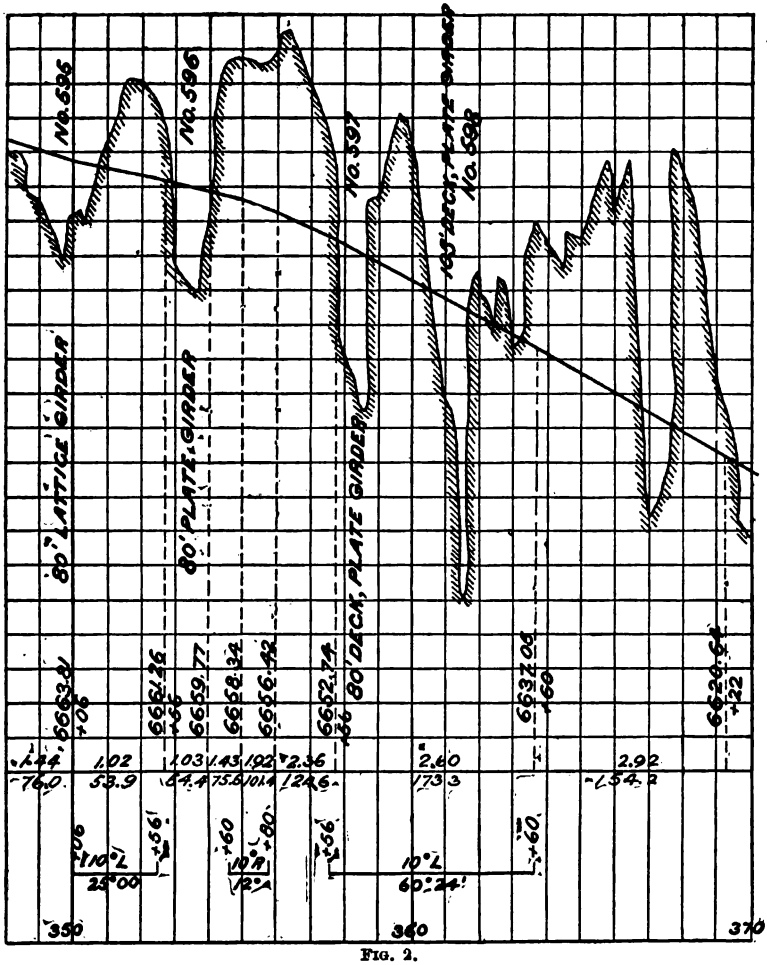

Fig. 2.

to their feet and waved their hats enthusiastically at this point. This railroad was built during the cañon war by the Santa Fé, but is now operated by the Rio Grande.

Fig. 6 shows the girders at the west end resting on an abutment;

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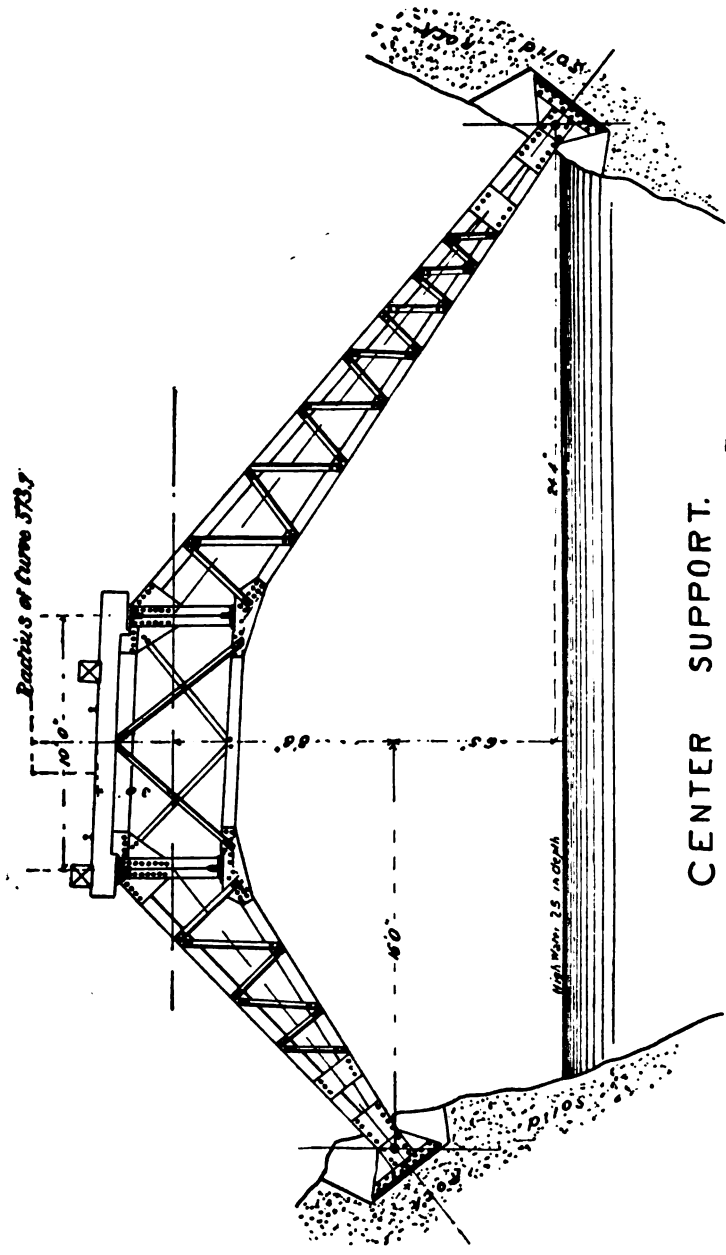

CENTER SUPPORT.
Bridge 598, over Rio Gallisteo, Apache Cañon.
FIG. 4.

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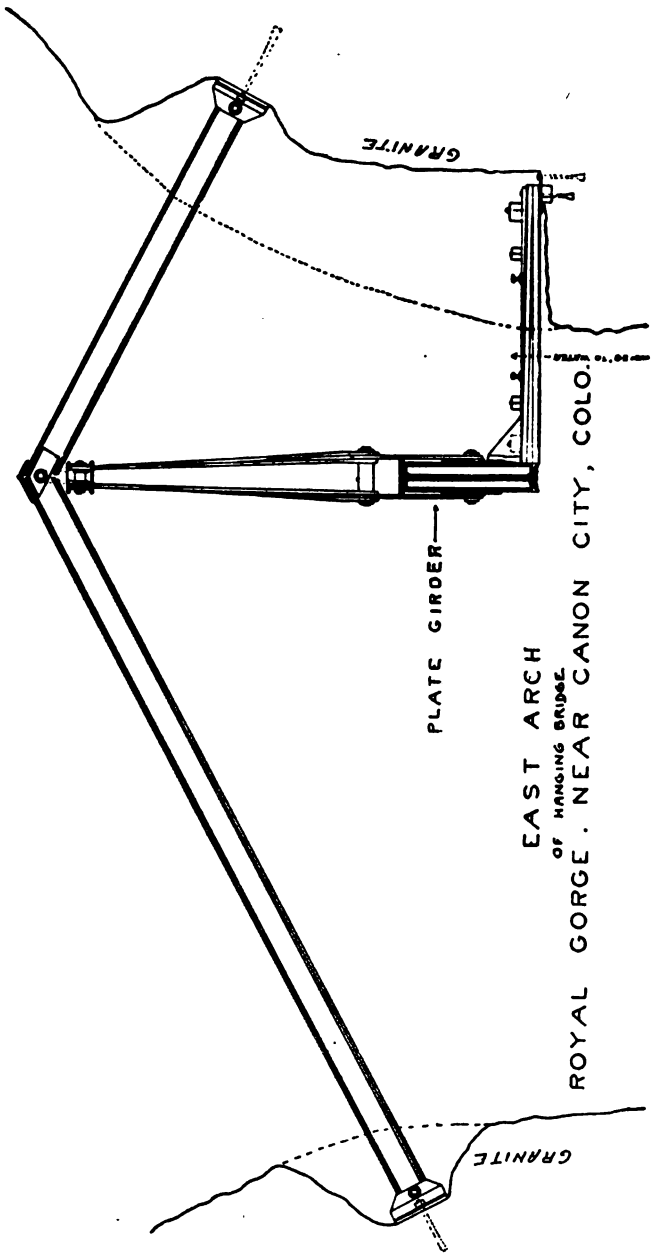

FIG. 5.

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the crevice or side cañon entering the gorge from the north immediately east of the west end of the bridge; the termination of the north girder after crossing the side cañon; the arches that support the south ends of the floor beams, of which the north ends rest upon the granite wall of the cañon; and the telegraph "poles" above the arches on the left.

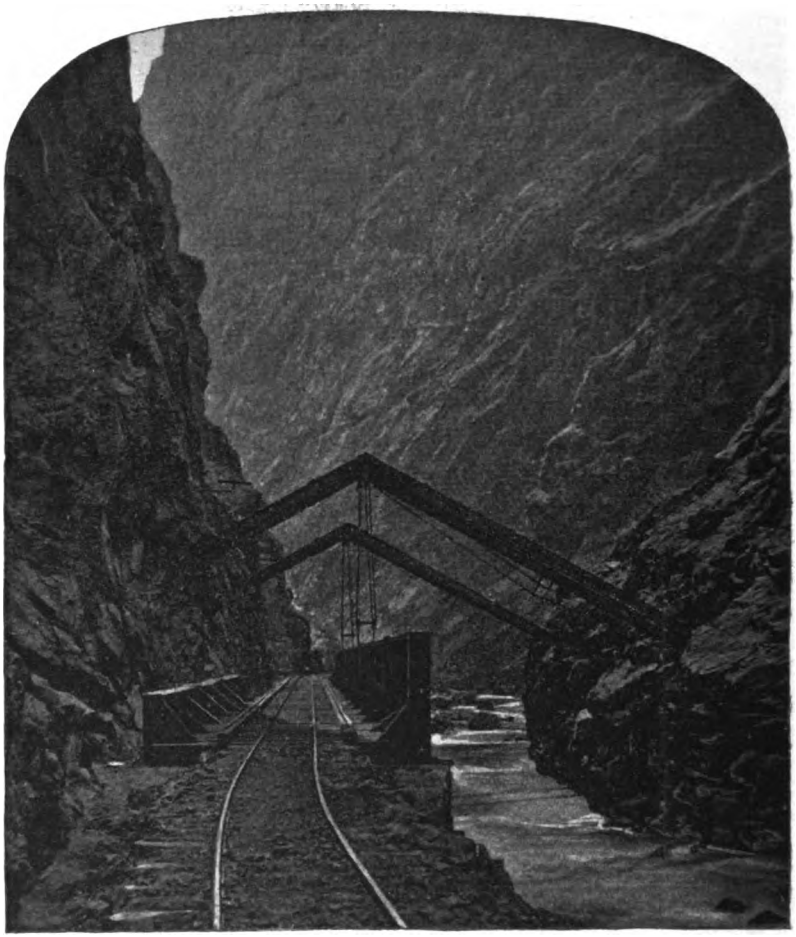

FIG. 6.

Plate LII is a plan and elevation of the bridge and its braces, and Fig. 7 shows how the bridge is hung.

Figs. 5 and 6 will give the reader some idea of the difficulty with which a transit line was first secured through the Grand Cañon. Men

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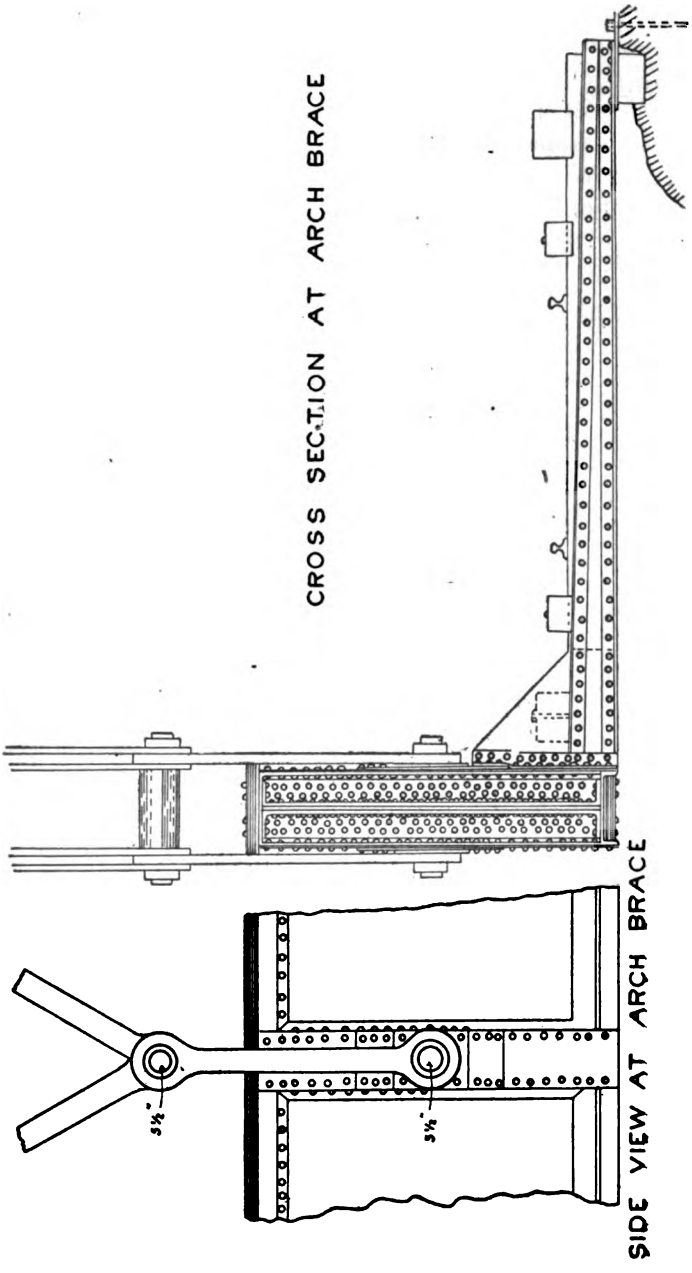

FIG. 7.

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were lowered by ropes; mules, burros, cook stoves and transits all found the walls of the cañon too steep for them, and they went down stream.

The prominent features of these two bridges (which were built under the direction of A. A. Robinson, chief engineer) the writer has given, with the hope that some of our members may have some "oddities" to add to the collection.

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(Vol. XXVI.—May, 1892.)

THE PHILADELPHIA TUNNEL OF THE BALTIMORE AND OHIO RAILROAD, ITS CONSTRUCTION AND COST.

By W. W. THAYER, Assoc. M. Am. Soc. C. E.

In November, 1885, the work was started of constructing the connection of the Baltimore and Ohio system with that of the Philadelphia and Reading.* By this connection the Baltimore and Ohio Railroad Company established for itself a south-bound leader from the coal fields of Schuylkill, Carbon, Northumberland, Luzerne and other coal counties of Pennsylvania to the southern market, and opened for itself a through-line from the West to the North and East.

The tunnel problem was this: With an exceedingly low limit of head room, made necessary by the low elevation of the city grade of

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the street system, and with a view to the retention of such grades as well as the care of the city reservoir, water supply and sewerage system, to rise from an elevation of 11.0 to that of the Philadelphia and Reading tracks (± 54.0) in a distance of 4 155 feet; and at the same time adopt a gradient which would have a sufficiently low limit of tractive force to be in keeping with the 8-degree curve required by the alignment. The character of the soil was, for the most part, of a shelving, seamy rock, hardening, at a depth of 10 or 12 feet, to a quartz, shelving transversely to the cut, and varying in firmness according to the location of its seam below the surface, at one point cropping out at the surface and again disappearing entirely and being replaced by soft clay.

By reference to the partial sketch plan, Plate LIII, it will be seen that the alignment was as follows:

An 8-degree curve led up to the tunnel portal, joining there to a tangent 987 feet long; this ran to an 8-degree curve to the left with a length of 698 feet, and was followed by a tangent of 2 325 feet, to where a compound curve of 300 or 400 feet connected it with the Philadelphia and Reading Railroad tracks.

The section of the tunnel was varied according to circumstances. Where sufficient head room was available, the brick arch was used; but where the head room was insufficient to allow this, the girder-top tunnel was adopted. The original section of the tunnel arch, as approved and intended to be used, was a 15-foot half circle of brick, supported on a bench wall with its springing line 7 feet 8½ inches above sub-grade; but where the rock showed good and full, a two-thirds circle of brick springing from a skew-back was used, the inside edge of which was $1\frac{85}{10}$ above sub-grade. By this two-thirds circle section, 2½ feet of rock excavation was saved on the back, at a height of 7.71 above sub-grade, and all intervening spaces were filled in with solidly rammed concrete or masonry. (See section of brick arch.) The brick ring throughout was composed of six bricks, which, with their joints, made a thickness of ring of $2\frac{3}{10}$ feet. The bench walls were without coping and were, by plan, to be carried up 5 feet thick and straight; but this, too, was varied according to the character of the rock backing, in some cases all the interstices being made solid with masonry. From the top of the bench wall the haunching was carried up to a height of $6\frac{4}{10}$ feet below the elevation of top of the extrados of the arch at the

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center, and the space between this and the cutting was in most cases filled compactly with dry masonry.

Centers were struck not less than a week after the keying of arches, and accurate levels taken to show the depression, if any, of the crown. This depression or settling of the crown was found to average between three-fourths and one-eighth of an inch, except in one instance, where the centers were drawn before the haunching had been carried up to a sufficient height to prevent buckling. In this case the crown settled 2¼ inches. Here, the centers were replaced and tightly wedged, the arch unkeyed and the haunching finished. Observations showed the arch lifted to its correct position and only one-quarter-inch depression at the end of three weeks.

Where the girder-top tunnel was used, the walls varied from 6 to 11 feet in thickness at the bottom, according to their height, and had a batter of one-half inch to the foot on the face. In most cases where rock was found, the walls were filled out solid on the back to the rock cutting, letting original plan sections go, and making final measurements as the work progressed. The girder used on the iron-top tunnel was a single-web girder 2 feet high on the center and 1½ feet at the ends, with 3-inch top and bottom angle iron braces, with 3-inch stiffeners, giving a bearing on top for iron buckle plates $\frac{5}{16}$ of an inch thick and 2 feet square. Girders were laid 3 feet 2 inches from center to center, to which the plates were riveted, and angles were run over all joints and riveted to both girder and plates. On the top of these plates 4 inches of sheet asphaltum was laid, forming the bed for the block paving composing the finished street, a crown of $\frac{5}{16}$ of a foot being given for the sub-grade of the street.

An idea of the arrangement of the grades in the tunnel proper can be best understood by a glance at the annexed sketch profile. In the tunnel proper and open way were six changes of grade and two vertical curves, and all masonry was constructed on the true alignment, that is to say, vertical curves were carried through all walls, inverts and skew-backs in exact conformity with, and parallel to, the sub-grade.

From the south portal the iron covered way ran 345 feet; here the arch began, which ran 1 700 feet with frequent changes of section as before described, when, through lack of head room, the iron covered way was again adopted for a length of 250 feet. The remaining distance, 1 860 feet, was built as a walled cut or open tunnel until the

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elevation of the Reading tracks was reached and the retaining walls ran themselves out on grade.

Length of south covered way	345.3	
Length of brick arch	1 700.0	
Length of north covered way	250.	
Length of covered way	2 295.3	tunnel proper.
Length of open tunnel	1 860.	
	4 155.3	feet.

The opening of such a trench in the heart of a large city, entailed the necessity of the care and maintenance of the water and gas supply and the sewerage system. A large 40-inch water supply main leading from the reservoir, was supplanted by extra pipes hung by girders across the cut until the making firm of the existing one was completed. Sewers were broken into and their contents conducted through trenches in the cut, pending the construction of new ones.

The writer submits the following final estimate of the completed work:

	Cubic yards.	
Earth and rock excavation	205 833.2	
Filling back of spandrels	66 278.4	
Rock filling	251.0	
Dry wall	281.4	
Bridge masonry, 1st class	100.2	
" " 2d "	5 455.	
Brick work	6 678.7	
Coping	647.1	
Concrete	156.0	
Rip-rap	142.0	
Rubble masonry	30 015.4	
Ballast laid	5 748.5	
Total cost		$429 564.77
Iron fence along coping of open tunnel		5 330.38
Re-throwing and laying Philadelphia and Reading tracks		4 867.85
		$439 763.00

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This total of nearly $440 000 is the cost of the completed tunnel exclusive of cement, ties, rails, plumbing, sewer connection and iron-work of the covered way. The cement, which was the Improved Union brand, was supplied by the railroad company after careful tests, and under its inspectors was not stinted. The writer has endeavored to give above the accurate cost of this work irrespective of the attendant expenses incurred for damages by awards to properties, by loss of life or limb, or those expenses necessarily incurred in complying with city ordinances or records, and in the widening of streets and changes of grades consequent upon the undertaking.

The deaths during the work, as far as learned, were five, two of which were of spectators whose extreme carelessness was alone the cause of their deaths. Three employees were killed by falling derricks and slides of embankments.

Within one year from the starting of the work the rails were laid in the tunnel. During construction the work was pushed day and night, and in one instance the walls were put in before the center excavation was started, by trenching to the widths of the walls, filling the holes with masonry and dressing the front face. The blasting was at the same time carefully carried on without any detriment to the walls or their foundations.

Five years after construction, trouble was experienced by the gases of locomotives eating through the web of the iron girders, and greatly weakening the structure; but this has been corrected by throwing flat arches in between the girders, which completely protects the iron from rust and the detrimental influences of locomotive gases.

COST PER FOOT OF TUNNEL ONE-HALF CIRCLE-SECTION WITH BENCH-WALLS.

Two bench-walls	$23.60
Brick ring	32.40
Haunching	21.20
Parging	1.52
Foundations	2.25
Cost of masonry	$80.97
Excavation, 30 feet deep	39.73
Top filling	.80
Total cost of tunnel complete (1 lin. foot long).	$121.50

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Cost of Two-third Section per Linear Foot.

Brick ring	$41.76
Backing	7.05
Foundations	2.25
Parging	1.52
Haunching	21.20
Skew-backs	5.50
Total cost of masonry	$79.28
Excavation, 30 feet deep	35.92
Top filling	.80
Cost per foot of two-third section	$116.00

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VOL. XXVI. JUNE, 1892. No. 6.

TRANSACTIONS

OF THE

AMERICAN SOCIETY OF CIVIL ENGINEERS.

Edited by the Secretary, under the direction of the Committee on Publication.

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Regular Meetings, on the first and third Wednesdays of the month, at 20 o'clock, except during July and August.

The rooms of the Society are open from 9 to 17 o'clock each business day except Saturday (when they are closed at 15 o'clock), and at other times on application to the Janitor.

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TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

533.

(Vol. XXVI.—June, 1892.)

ADDRESS AT THE ANNUAL CONVENTION AT HYGEIA HOTEL, FORTRESS MONROE, VA., JUNE 8TH, 1892.

By MENDES COHEN, President, Am. Soc. C. E.

A provision of the Constitution of the Society requires that the President shall deliver an address at the Annual Convention, and, in compliance therewith, I have the honor to appear before you this evening. As I have not been of late years actively engaged in the practice of our profession, and am, for that reason, unable to give from personal experience notes of recent works and improvements marking the strides of current progress in the science of engineering, it has been suggested to me that you would be interested in a historical narration of some of the events and problems of a much earlier period, and the manner of meeting them in the construction, development and operation of the Baltimore and Ohio Railroad. It must not be understood from my assuming to act upon this suggestion, that I was one of those upon whom these early duties and responsibilities devolved. I was not born until some years after the 4th of July,

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1828, when the venerable Charles Carroll, of Carrollton, then the only survivor of those who signed, in 1776, the charter of our liberties, turned the first sod and laid the first stone of the Baltimore and Ohio Railroad. But many of the original actors and participants in these events were still upon the stage when, as a youth, I entered the locomotive shops of the late Ross Winans, with a view to preparation for the profession of a civil engineer.

Mr. Winans, a native of New Jersey, was himself a most prolific inventor, and had produced, previous to 1829, an improved journal and box which he thought suited to railroad use, and which he went to England to test on the Liverpool and Manchester Railroad, then under construction. He was there present when the celebrated trial of locomotives took place in 1829, and the opportunity thus afforded him for a study of the locomotive was not lost, and was subsequently of much value both to himself and the railroad company. His journal and box were adopted on the Baltimore and Ohio Railroad, and he, taking up his permanent residence in Baltimore, had been continuously thereafter engaged in designing and constructing cars and locomotives adapted to use on the railroad.

In and about the establishment of Mr. Winans and the railroad works adjoining were many of the men who had been employed at the very commencement of the undertaking, the actual participants in which have now nearly all passed away. These men I used to meet constantly, and entering myself into the service of the railroad company a few years later, while many of the original features of the work were still in existence, I had opportunities of acquiring facts and of observing details not so readily gathered or noted to-day. Being in this sense only a survivor of that early period, I venture to bespeak your interest and attention to some of the more prominent details of the work in which there was much originality. Many of these facts are known to some of you, many of them are here gathered from the original reports and other records of the time, some from a history of the railroad prepared in 1853 by the late William Prescott Smith, and others from the personal reminiscences of survivors, but I am not aware that they have ever before been brought together.

In 1825 Baltimore was a city of about seventy thousand inhabitants. Its people were enterprising, and had developed with the country beyond the Alleghanies a large trade, transported in wagons over good

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turnpike roads, while the foreign commerce of the port conducted in its home-built, fast sailing vessels, known everywhere as "Baltimore Clippers," extended to all parts of the world. Its geographical position, giving it a saving of from 100 to 200 miles in distance to the Ohio River at Wheeling over that from Philadelphia or New York to the same river at Pittsburgh, was sufficient, when land transportation was effected only by horse and wagon, to give it an enormous advantage. Philadelphia and New York were making strenuous efforts to retain their trade, and to this end were pressing forward their respective lines of canal, the future possibilities of which, aided by steam navigation then rapidly developing, no imagination would limit.

Baltimore had not failed to realize early that action was necessary. It was believed that the valleys of the Potomac and Youghiogheny rivers would afford a route for a canal quite as advantageous as any to the north of it. Washington, himself, had taken much interest in such a project, and had presided in December, 1784, at a meeting in Alexandria, at which the Potomac Company was formed for improving the navigation of that river. In 1824, a charter for the Chesapeake and Ohio canal was granted by Virginia and Maryland. Into this corporation was merged the Potomac Company, which had thus far only accomplished the opening of a partial slack water navigation. Preliminary surveys for the canal had been made and some definitive location effected, when the hopes of Baltimore as to its commercial success were completely upset by the report of General Bernard, of the Board of Engineers on Internal Improvements, who in 1826 showed not only the very great cost of the work, but that the scarcity of water on the Alleghanies at the high elevation over which the canal must pass, made its successful accomplishment very problematical; besides, its proposed eastern terminus was Georgetown on the Potomac, and to reach Baltimore from that point would involve an additional cross-cut canal some 50 miles in length.

The opening of the Erie Canal in 1825 aroused the merchants of Baltimore to a full sense of impending decadence, and discussions of the subject were constant between the more thoughtful of their number. If the projected canal was out of the question, what were they to do? Competition by the best of turnpike roads was hopeless! At this time attention was being directed in England to the possibilities

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of railroads for general transportation purposes. Numerous short lines were already in existence there, and a very few had been laid even in this country, but so far they were designed chiefly, if not altogether, for the purpose of moving mineral products from mines or quarries to a convenient point where water transportation would be available, and were, in fact, what we would term to-day mere tramways, and only operated reliably by horses. Might not this new system be made available on a larger and much more extended scale for connecting Baltimore with the western waters? So thought Philip E. Thomas and George Brown, two of these Baltimore merchants. Having facilities for obtaining information from England in regard to what had been done there, these gentlemen took pains to procure all that was available from that source, and with it in hand they invited a few of Baltimore's most influential citizens to meet them for the purpose, as their call stated, "To take into consideration the best means of restoring to the City of Baltimore that portion of the western trade which has been lately diverted from it by the introduction of steam navigation and by other causes."

This meeting was held at the residence of Mr. Brown on the 12th of February, 1827. It was well attended. After hearing read and discussing the various documents and statements illustrating the efficiency of railroads for the conveying of articles of heavy carriage at a small expense, and the superior advantage of this mode of transportation over turnpike roads or canals, a committee was appointed to consider the subject, and was directed to report to an adjourned meeting one week later. At this adjourned meeting it was determined to apply to the Legislature, then in session, for a charter for the Baltimore and Ohio Railroad. This was obtained in the following month in such a comprehensive form that it remains to this day a monument to the forethought of those who prepared it. This was the commencement of the Baltimore and Ohio Railroad Company, the first railroad enterprise undertaken for general commercial purposes certainly in the United States, and far exceeding in magnitude anything then being promoted in England.

Organizing at once under this charter, Phillip E. Thomas became the first president of the company, and, with an efficient Board of Directors, lost no time in prosecuting the work. Application was made to the United States Government for a detail of officers to conduct

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reconnaissances and surveys, to be at once undertaken, while the company would take time for the careful selection of a suitable chief engineer. In response to this application there were detailed to the railroad service, S. H. Long, Brevet Lieut.-Col. U. S. Topographical Engineers; William Gibbs McNeill, Captain U. S. Topographical Engineers; Dr. William Howard, U. S. Assistant Engineer, assisted by Mr. F. Harrison, Jr., U. S. Assistant Engineer; Lieutenants Joshua Barny, U. S. Artillery; Isaac Trimble, U. S. Artillery; John L. Dellahunty, U. S. A.; Walter Gwynn, U. S. A.; William Cook, U. S. A.; Richard E. Hazzard, U. S. A.; George W. Whistler, U. S. A.; Fessenden, U. S. A.; Thompson, U. S. A., and Mr. Guion.

The duty assigned to these officers was performed with so much assiduity that they were enabled to present on the 5th of April, 1828, a report which covered a general examination of the whole intervening region between the Patapsco and Ohio rivers, and with so intelligent a discussion of many of the alternative lines that it is a matter of no little wonder how so much could be accomplished in so short a time, through so rough a wilderness as the Alleghanies must have been in that day. The study of the route seems to have been based on the theory that the gradient of the road, to be worked probably by horses, possibly by locomotives, must be moderate, not exceeding 30 feet per mile, as nearly uniform as possible, never undulating, that a very liberal amount of lateral curvature was admissible in furtherance of this, and for the avoidance of heavy cuts and fills; and that where summits were to be crossed and great differences of level to be overcome, recourse should be had to inclined planes, to be operated preferably by steam or water power.

The company had, in the meantime, secured the services of Jonathan Knight, Esq., of Pennsylvania, a civil engineer of experience, who had been engaged upon the National Road between Cumberland and Wheeling, and who had served more recently under appointment of the U. S. Government as a commissioner to extend that road from Wheeling through the States of Ohio and Indiana. A few days after the filing of the report of reconnaissance and survey, the engineering department was organized by placing it in charge of a Board of Engineers, consisting of the president of the company, Mr. Philip E. Thomas, Colonel S. H. Long, U. S. Topographical Engineers, and Jonathan Knight. The president had no knowledge of engineering, so there were two

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professional men with a layman to vote with one or the other and make a majority whenever a difference of opinion supervened. Captain McNeill was subsequently, on October 6th, 1828, appointed a fourth member of the Board.

On May 5th, 1828, the Board of Engineers made a formal report recommending to the company the adoption of a route by the valley of the Potomac, as affording by far the best passage through the South Mountain, involving too the crossing by inclined planes of but one summit, that of Parr's Ridge, before reaching the Potomac. A few days later, parties were started in the field to make definitive location of the line. It is curious to note that in the midst of their labors in the location of a great line through a difficult country, the Board of Engineers found they had quite as difficult a problem to solve in meeting the views of the citizens of Baltimore as to the city terminus of the line. Every merchant wanted it at his own door, and it was many years before the question was put to rest.

On July 4th, 1828, formal commencement of the work was made by the laying of the first stone, by Charles Carroll, of Carrollton, attended by a great procession and trades' display, badges, music, etc., and on the 14th of the same month the first letting of the work took place. That there was no time being then lost is clear, for, at the annual meeting, less than ninety days later, the President reports that the grading of 1½ miles was complete.

Simultaneously with the location of the line at Baltimore a party had been sent to locate the road through the Narrows of the Potomac, at and beyond the Point of Rocks, where it was feared there might be contest with the Chesapeake and Ohio Canal Company. This was promptly effected and was followed by the acquisition of the coveted ground in fee simple, though to no purpose, as it presently appeared, for in the same annual report of October 1st, 1828, the President announced that the Canal Company laid claim to a pre-emption right to a location along the Potomac River through succession to the rights of the old Potomac Company. Injunction immediately stopped the work of the railroad company along the Potomac, and the litigation thus commenced was prolonged for several years, ending finally in the triumph of the Canal Company.

The same report announced the intention of the President and Board of Directors to send to England a deputation of engineers to

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make observations there, and especially to study the application of moving power upon the roads there existing. Mr. Knight, Captain McNeill and Lieutenant Whistler were accordingly sent abroad in November, 1828. They made a very thorough examination of all the railroads then existing in England, and returned to Baltimore May, 1829. The report of their observations, though promised, was deferred, and, if prepared, seems never to have been formally presented and never printed.

At their very first meeting on the 12th of April, 1828, the Board of Engineers appointed Mr. Casper W. Weaver, Superintendent of Construction, "whose duty it should be to aid in arranging contracts for the execution of work upon the railroad and to see that all contracts for the construction of said road be faithfully carried into effect, and also to perform such other duties connected with the service as the Board of Engineers may direct." Mr. Weaver had been engaged in a somewhat similar capacity on the construction of the National Road from Cumberland to Wheeling and its continuation in Ohio, and, although probably not equipped with a scientific education, had acquired considerable experience and practical skill in dealing with contractors and pushing public works. The Board of Engineers, on the contrary, with a most cumbrous organization, was dealing with entirely new problems where precedents and past experience were of little avail. The Superintendent seems to have thought he possessed far more knowledge of the subject than his engineers and would brook no control. He seems to have recognized their general lines of location, but modified and changed them occasionally as he thought fit.

The Board of Engineers adopted in its printed regulations a thorough system in accordance with the military training and habits of a majority of its members, and not differing from the methods observed in the Government work. Mr. Knight, united with his colleagues in the adoption of these regulations, all of which were doubtless right in themselves, but they were not elastic enough for the views of Superintendent Weaver, who did pretty much as he pleased, without the slightest attention to regulations when he chose to ignore them. It is very evident to one who studies the record to-day, that the Superintendent had the ear of the President and was sustained by him and the Board of Directors. They wanted the work driven with energy in order to impress the public with confidence in the success of a new and

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untried enterprise, for the company was seeking support and subscriptions to its stock from both the State and the United States Governments.

Mr. Weaver seems to have rushed things, which was what they wanted, and the man who, without drawings or prepared plans, would assume to put contractors to work on dressed stone viaducts of 80, 55 and 20-foot spans, at an estimated cost but little exceeding that which the Board of Engineers had named as the cost of wooden superstructures, adopted by them for economy, was the man for the day. Colonel Long seems to have had much trouble from this cause during the absence of his colleagues in England. On their return in May, 1829, there was an investigation; charges were preferred against the Superintendent, such as changing location without authority, injudicious dealings with contractors, entire disregard of certain specific orders of the Board of Engineers, etc. A hearing was had before a committee of the Board of Directors which acquitted the Superintendent of all charges affecting his integrity and practically sustained him throughout.

The Board of Directors soon thereafter, on the 4th of January, 1830, abolished the Board of Engineers and appointed Jonathan Knight as Chief Engineer. This action gave rise to much acrimonious discussion in the newspapers of the day. A pamphlet of some five hundred pages was published, giving minute details of the work of the Board of Engineers from the first organization of the Board to its final dissolution, and this, in connection with other pamphlets and newspaper articles, give a pretty clear insight as to the actual condition of affairs. A study of it is interesting as showing how the engineers were led, principally by local jealousies, as to the point of entrance into the city, combined with a supposed necessity for avoiding undulations in the grade which had been located as a level for the first six or seven miles, to the adoption of a most expensive line within the first three miles from the city, involving a costly viaduct, and one thorough cut of 310 000 cubic yards. It was thought by this location "a line of road would be secured which, by some extensive embankments and sharp cutting near the city, could be located with but a single summit for a distance of 180 miles, and with but two summits requiring stationary power along the entire distance to the Ohio River."

It enables us further to appreciate how thoroughly novel and un-

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tried were the problems involved. We find an elaborate defense of the proposition that the flanges of the wheels should be on the outside of the wheel and rail, an arrangement which the Board of Engineers had adopted, and in accordance with which one and a half miles of track had been actually laid, when shortly after the abolition of the Board of Engineers, the Chief Engineer changed it to that which was then used abroad, and which has since everywhere prevailed. Mr. Knight, in his annual report of October, 1830, explains the reasons for this change of plan and discusses the question very fully; the result being that, while his analysis would seem to justify the conclusion that the flange was better on the outside, and in that position peculiarly well suited to Mr. Winans' patent car, which it was proposed to use, yet as experiments on the Liverpool and Manchester Railroad had shown that this car would work well with the flanges inside, it was desirable that a railroad which must be a general thoroughfare and probably intersected by numerous branches, should be suited to all approved kinds of carriages, of which all, so far, had inside flanges. Hence the conclusion to adopt upon this road the form already in use elsewhere. The same report contains an elaborate discussion and analysis of the size, weight and form of the chilled cast-iron wheel to be used on the road with the proper amount of cone to be given to the thread, resulting in a pattern which was standard for the next twenty years or more. Of cars, a number were constructed at this time with case-hardened or steeled journals and chilled bearings, a system which also continued in use for over twenty years.

Here, I must mention the name of John Elgar, Mr. Knight's mechanical assistant, who was employed in designing and constructing wheels, chairs and turn-outs or switches. The latter were still used in and about the yards and street tracks of New York some twenty-five years later, where they were known as Baltimore switches. When I knew Mr. Elgar forty-five years ago, he was an old man of about seventy years, gentle, and minutely painstaking in all he did. He was then interested in water-rams, which were constructed for him in Winans' shops. I was frequently assigned to his work and learned to know him well. In fact, the first time I ever held a target was for him in running a line of levels for setting one of his rams to supply water to the Baltimore House of Refuge. The level he used was peculiar. He was too poor to buy an ordinary telescope level, and much too

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independent to borrow one, so he made his own. Its use did involve the necessity of being reasonably careful about equal sights, but it served his purpose well, and the four silver quarters which he insisted on my accepting for my afternoon's duty as rodman, was the first pay I ever received for engineering work.

The dissolution of the Board of Engineers was soon followed by the retirement from the company's service of all the United States officers heretofore attached to it. They were all men of ability, and each and all subsequently made a record elsewhere; but it is very evident that the President and Board of Directors had effected a most impracticable organization at the outstart, under which it was well nigh impossible to produce good results.

The track was to have been laid throughout on stone sills, but owing to the scarcity of stone until the valley of the Patapsco could be reached, and for the further reason that the embankments constructed without regard to the directions of the engineers, who had ordered them built in compact layers of full width, as you would build an earthen dam, were not sufficiently settled to afford a proper bed for the stone, it was deemed best to lay the first seven and a half miles with wood. Sleepers, or cross-ties of 7 feet to 8 feet in length, and of from 5 inches to 10 inches in thickness were laid transversely on the road at a distance of 4 feet from center to center. Notches were formed in each sleeper at proper distances to receive stringed pieces 6 inches square, and from 12 feet to 40 feet in length. These were held in place by wooden keys. Under each notch of each sleeper a cavity was excavated in the graded bed to receive 1¼ cubic feet of broken stone of a size to pass through a 2-inch ring. This cavity was arranged 18 inches lengthwise of the sleepers, and 12 inches lengthwise of the road, and 12 inches deep. Each sleeper was laid so as to rest firmly on these supports of broken stone.

The iron rails 15 feet long by 2¼ inches wide and $\frac{5}{8}$ths of an inch thick were laid on the stringers, about three quarters of an inch from the edge. Iron plates having been let into the wood immediately under the joinings of the iron rails, the ends of each two adjoining rails were fastened to the plate by a screw bolt or nut, or by a nail or spike. The rails were laid a quarter of an inch apart with mitred joints, and were nailed to the wood throughout, through holes made in the rail for the purpose. The projecting corner of the stringer was then

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adzed off, that the flange of the wheel might not come into contact with the wood. On the unsettled embankment sub-sills were used under the sleepers or cross-ties. For the first seven miles a double track was constructed of this description, and on the next six miles which carried the work to Ellicott's Mills, one track was made with the wooden stringers on stone blocks, about 16 inches square by 12 inches long, bedded in broken stone, instead of on wooden sleepers or cross-ties. The rails were secured to the block by cast-iron knees. The other track consisted of stone sills, about 16 inches wide by 12 inches deep, bedded in broken stone, forming a continuous base for the iron strap rail. The latter was the standard track, and it is recorded in the reports as believed to be the best known, and superior to the iron rail in use in Europe. Its cost was $2 000 per mile for each track more than the wooden stringer, but its supposed permanency recommended it to the company wherever the proper material could be had.

It was not long, however, before experience began to enlighten the officers to a fact which the President announced a little later; that of the several systems of superstructure just described the first intended to be but temporary was decidedly the best, and the last as decidedly the worst of all. The track was finished with a horse path of gravel or broken stone, and was opened for use to Ellicott's Mills on the 21st of May, 1830, being operated by horses. Among the assistant engineers in charge of the track-laying are found the names of J. Dutton Steele and Squire Whipple.

So much extra expense had been involved in the laying of new track with the stone sills, owing to the distance from the quarry and the difficulty of distributing them ahead of the track, together with the evident inexpediency of using them on unsettled embankments, that, as the work proceeded west of Ellicott's Mills, the Chief Engineer suggested whether the extension could not be more advantageously laid with wood, leaving the stone, or other approved methods, to be availed of for renewals. This policy was not adopted, however, until about 40 miles of single track had been laid with the stone.

In his annual report of October 1st, 1830, Mr. Knight speaks with much satisfaction of the readiness with which a speed of 10 miles per hour has been maintained with horses in the working of the 13 miles of road then in use for four or five months. He refers, too, to the recent eminently successful demonstration of the applicability of the loco-

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motive to a railway with curvatures of 400 feet radius, as proved by the trial upon the road of an experimental engine built by Peter Cooper, of New York. This engine was rudely constructed, had but a single working cylinder of 3½ inches diameter and was mounted on the ordinary 30-inch car wheels, which were driven from the engine shaft through the medium of gearing. This, *the first trip of the first locomotive built in America*, took place on August 28th, 1830. A most graphic description of the trial trip was prepared many years ago by the late John H. B. Latrobe. No doubt many of you may recall it, and I will not detain you with further reference to it than to say, that the machine was a mere experiment to prove to the skeptic that the sharp curves of 400 feet radius, or even less, could be readily worked by steam applied through the adhesion of the wheel to the rail. Peter Cooper was interested in proving this, for he was the largest owner of shares in the Canton Company of Baltimore, a real-estate corporation that all, who know anything of the New York stock market, have heard about. Peter Cooper believed that his company's lands were only to be made valuable by the successful development of the railroad project, and he was right. The full recognition of their value is, however, only now being realized after an interval of sixty years.

During this and the following year experiments were made to test the resistance of cars with the Winans' anti-friction box and those with hardened steel journals and chilled bearings. With the former the friction on a level straight road was found to be one four-hundredth part of the load and with the latter one two-hundred and fortieth part of the same. Experiments were also made to test the practicability of turning the street corners of the city with the railroad and cars. It was found that a radius of 60 feet could be readily turned by permitting the wheels on the outside of the curve to roll on the edge of the flange, which was guided by the flange running in a groove in the rail, while the inside or shorter rail remained of the usual form and carried the tread of the opposite wheel.

In his annual report of October 1st, 1831, Chief Engineer Knight acknowledges the aid he has received in the arrangement of the machinery from his assistants John Elgar and Ross Winans, the latter of whom was then engaged in planning the machinery and fixtures for the inclined planes, which were never, however, brought into use.

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The directors being desirous of procuring their steam machinery, as far as practicable of American workmanship and anxious to direct the mechanical genius of the country to its further improvement, published on the 4th of January, 1831, an advertisement for locomotives. In this advertisement $4 000 is offered for the most approved engine and $3 500 for the next best, the engine to burn coke or coal and not to exceed, in running order, the weight of 3½ tons, not inclusive of tender. Three locomotives were offered to the company in response to this advertisement. Of these only one was found to come up to the requirements of the specification. This engine was the "York," built by Davis & Gartner, of York, Penn. It was designed by Phineas Davis, an ingenious watchmaker of that borough, under the stimulus of the company's advertisement, and who, probably for the purpose of building it, associated himself with Gartner, a machinist of the same place. The weight of the engine, limited by the terms of the specification to 3½ tons, was not exceeded. It had a vertical boiler, and was carried on four of the ordinary 30-inch car wheels. The size of its vertical cylinders I find no where recorded. After undergoing some modifications by which the functions of the cylinders were transmitted to the driving wheels and velocity attained through spur gearing, the engine was found capable of conveying 15 tons at 15 miles per hour on a level.

It was put in regular service between Baltimore and Ellicott's Mills, making the distance of 13 miles in an hour with four cars weighing 18 tons gross. About 5 miles of this distance had a grade of 17 feet to the mile. It was found to travel curves of 400 feet radius with facility, even at 15 miles per hour, and on straight parts of the line sometimes attained a velocity, for a short time, of 30 miles per hour. The fuel used was anthracite coal, and answered the purpose well; but the boiler was deficient in size. This engine was at once improved upon by the same builders in their engine "Atlantic," which weighed 6½ tons, of which 4 tons were upon one pair of 36-inch driving-wheels. The boiler was much improved. The upright cylinders of 10 x 20 inches were connected to one pair of driving-wheels by spur gearing, doubling the velocity. The new feature was introduced of cutting off steam at two-thirds of the stroke.

This engine did good work. It was followed from the same shops by the "Indian Chief," soon afterward altered and remodeled as the

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"Traveler," and then by the "Arabian," which latter machine became the type of the so-called "Grasshopper engine." This engine weighed 7½ tons. The boiler was vertical, on the model of that of Peter Cooper, with iron tubes. The cylinders, 12 x 22 inches, were connected through spur gearing, not as in the "Atlantic," directly to the axle of the driving-wheel, but to a shaft or axle, about 3 feet in front of the front pair of wheels, parallel therewith and in the same horizontal plane. The ends of this shaft carried cranks which were connected by the usual side rods to cranks on the ends of the driving-wheel shafts, thus transmitting the motion to the driving-wheels. This arrangement, first introduced into the "Traveler," secured the spur gearing from shocks due to the irregularities of the road and disposed finally of a most troublesome cause of breakages. Chief Engineer Knight, in his annual report for 1833, states that the "Atlantic" has run 13 280 miles, burning anthracite coal, of which it had, in that distance, consumed 190 tons without the failure of a single tube.

The building of these engines was now transferred to the company's shops at Mount Clair, under Phineas Davis as contractor, and the work was there conducted by him until his untimely death by an accident to a new engine he was testing, September 27th, 1835. Mr. Davis was succeeded by Gillingham and Winans, who lent their skill to the further perfecting of the machinery and brought these engines to a degree of efficiency which is surprising. So well and so thoroughly was the detail of this engine worked out, and so well adapted was it to hauling heavy loads at slow speeds through curves of the shortest radius, that it has survived to a very recent day. Twenty years ago, when the late Benjamin H. Latrobe prepared for the *Railroad Gazette* a very full and complete description of this engine, it is noted by the editor that four of the engines were still in use. Whether any are in service to-day I am not fully informed, but only a few years ago they were used for yard work at Mount Clair and were very efficient.

While the improvements we have noted in the locomotives, and which we have traced down to 1836, were being effected, other departments of the work were progressing. On the 1st of April, 1832, the line was opened to the Potomac River at the Point of Rocks, a distance of 70 miles from Baltimore, and was regularly worked throughout by horse-power. As already stated, it had been contemplated to effect,

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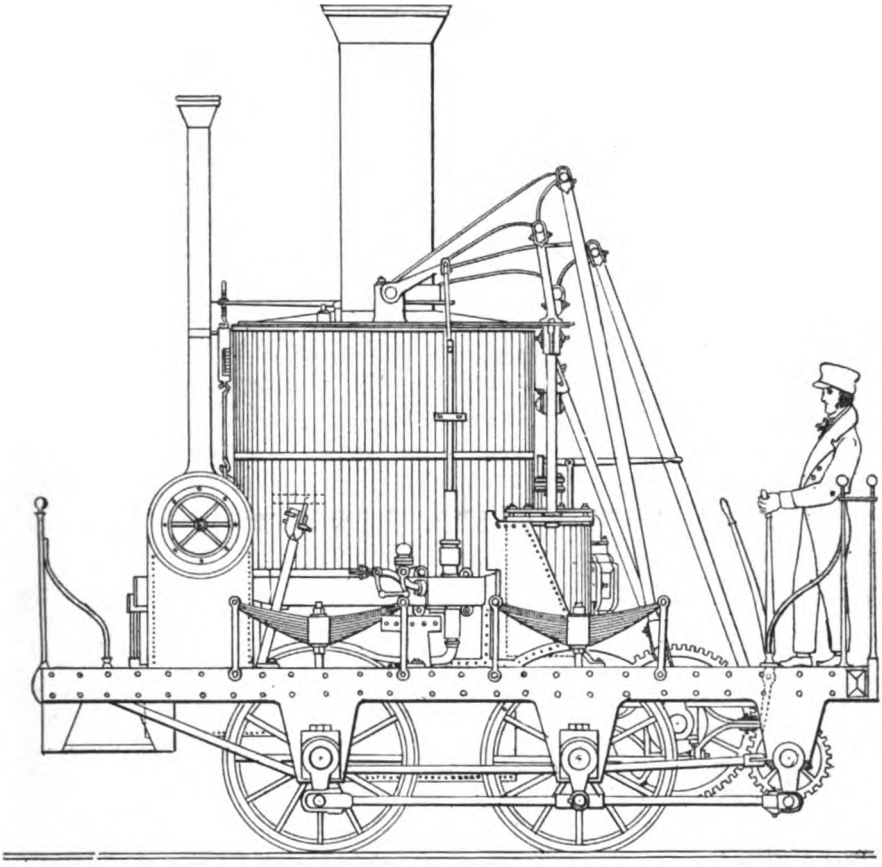

Locomotive Engine "Arabian," used on the Baltimore and Ohio Railroad, designed and constructed for the combustion of Anthracite Coal by Phineas Davis, 1834.

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with inclined planes, the crossing of Parr's Ridge, the summit of which was 800 feet above tide and the eastern foot of which was reached at about 40 miles from Baltimore. The placing of the machinery for working of the planes was, however, deferred from year to year, and the traffic carried across the ridge by horse-power, until the development of the engines already described led to their being tested on the heavy grade of the intended planes.

I have already mentioned that, at the very outset, work along the Potomac was stopped by litigation, resulting in favor of the Canal Company; a compromise was sought and finally effected in the spring of 1833. The immediate difficulty was the passage of the several spurs of the Blue Ridge which are cleft by the river with bold and rocky slopes and first encountered by the road in the Catoctin Mountain at the Point of Rocks, and continue at intervals for 12 miles to Harper's Ferry. The compromise arranged need not be considered here. It was onerous enough, but could not be avoided, and under it the work was completed to a point opposite Harper's Ferry by December, 1834.

During these years of obstruction, authority had been obtained to construct a branch road to Washington, and in 1831 Mr. B. H. Latrobe was appointed an assistant and assigned to the reconnaissance. This was followed up without delay by a more definite and careful examination. Mr. Knight takes a most comprehensive view of what he says must become a great national highway. He urges that no grades exceeding 20 feet per mile, nor any curve of less radius than 1 500 feet, or in extreme cases, 1 000 feet, be tolerated if avoidable at any reasonable expense, so that light locomotives may make the run regularly in two hours and indicates the possibility of a "message being made to pass the whole distance from Washington to Baltimore in one hour." The surveys were continued with great minuteness so that, as the President expresses it, "the route finally adopted should leave no better one available to a rival corporation."

In July, 1833, Mr. Knight submits his report and analyses of twelve alternative lines with a degree of elaboration and care that I venture to say has rarely been equaled. The line recommended by him was at once placed under contract, and under Mr. B. H. Latrobe, as the engineer in charge, was completed and opened August 25th, 1835. At its point of divergence from the main stem 7 miles from Baltimore it crosses the Patapsco by a masonry viaduct of eight arches, each of 58-foot chord, some 70 feet in height above the water and of a total length

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of 707 feet. This was designed by Mr. Latrobe and stands to-day a monument to his taste and professional skill.

The superstructure of the branch road was intended to be a great improvement on the various forms used thus far on the main stem. It consisted of longitudinal sills 6 inches square, and from 12 to 40 feet in length, laid in trenches, so cut for their reception that the upper surface of the sills will be from 2 to 5 inches below the graded surface of the roadbed. On this were laid crossties 4 feet from center to center, and cut out to receive stringed pieces of yellow pine 6 inches square, on which were laid the first heavy T-rails used on the road. This rail weighed 40 pounds per yard, as proposed by the Chief Engineer and modified in the shape of its face or surface by Mr. Ross Winans.

I must now say something of the progress made in coaches, cars and car wheels. The coaches were at the outstart made about as comfortable as the stage coach of the day, and were built on much the same pattern; but, of course, with cast-iron wheels. The "Ohio," of which I am able to show you a lithograph, was an improvement on those which had preceded it. You will see that it is provided with Mr. Winans' anti-friction boxes. So long as horse-power alone was used, nothing better was required. As soon, however, as the locomotive appeared upon the road, there came with it the necessity for modification in the carriages. The wheels, at first light to save weight, were made heavier to give needed strength with increased speed. Mr. Knight improved the shape of the tread and flange, while John Elgar and Ross Winans developed its chilled features, and Phineas Davis further improved and perfected the whole by altering the disposition of the metal in the tread and angle of the flange, and by introducing within the wheel a wrought-iron ring of $\frac{5}{8}$ or $\frac{3}{4}$-inch round iron, which not only perfected the chill, but increased the strength of the wheel.

So satisfied was Mr. Knight with the result, that he observes at quite an early stage, "the cast-iron wheel has probably attained the utmost perfection of which it is capable." Thousands of these wheels were made at Mr. Winans' shops, not only for use in various parts of this country, but for some of the German and Swiss roads which used them extensively up to 1851. Mr. Knight in his report for 1836 states, that "the chilled wheels have run 30,000 miles and more at the high speed used on the Washington branch without the failure of a single wheel," and he further states that he had every reason to believe that 50,000 miles might be accomplished by them.

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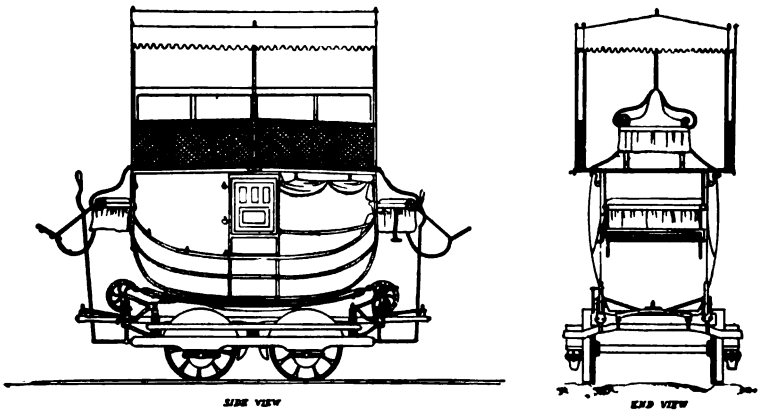

The Passenger Car "Ohio," designed and built by Imlay, for the Baltimore and Ohio Railroad Company.

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The increase of speed due to the use of steam made greater steadiness desirable in the coach than was possible in the four-wheeled car. The long stringers and sills for the track, some of them as long as 40 feet, had been heretofore carried over the finished road on two of the ordinary four-wheel platform cars, coupled by a long pole. On the platform of each of these cars there rested transversely a bolster with a vertical pin, or king-bolt, passing through its middle point and so connected to the platform of the car as to permit the bolster to swivel freely. On these bolsters of the two connected platform cars, and stretching from one to the other, the track timbers referred to were laid. Vertical stakes secured to the ends of the bolsters prevented the timber from rolling off. This contrivance, a very familiar and natural one, and not even then used for the first time, answered its purpose perfectly, and from its steadiness of motion suggested to the active mind of Ross Winans the carrying of passengers on a car constructed on the same principle. His experiments soon followed, and in the annual report of October 1st, 1833, the superintendent of machinery notes that he is building three car bodies to form one coach on eight wheels to carry sixty passengers.

This was the birth of the eight-wheel car which was patented by Mr. Winans and, though the courts, after long litigation, decided adversely to his claim on the ground, I believe, of prior use, as in the case of the timber car and a similar one for transporting large blocks of granite at Quincy, Mass., yet I have always believed that to Ross Winans we are indebted for this great improvement. Very soon thereafter eight-wheel cars were used throughout the line for both passenger and freight traffic, and special cars were provided for baggage, which had theretofore been carried on top of the coaches.

In 1835, James Stimpson, who claimed to have invented a jointed axle, permitting the wheels at each end thereof to revolve independently, applied to President Thomas to have his invention adopted on the road. The question was submitted to Chief Engineer Knight, who reported unfavorably, in a letter to the President, dated May 23d, 1836. In this he discusses at much length the nature of the resistances to which the wheels, axles and rails are exposed. The following passage is worth quoting:

"While upon this particular part of the subject, permit me to advance the belief, that in regard to assigning the relative resistances on different parts of a railroad laid out in straight lines and curves, and in the perfecting of the cast-iron wheel to traverse such a road, not

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to mention improvements in the locomotive-engine, more has been done at Baltimore upon the railways of this Company than in all the world beside."

I have stated that the road was opened to Harper's Ferry in December, 1834. The portion of the line crossing Parr's Ridge, located for inclined planes with stationary engines at a maximum grade of 270 feet per mile, was still worked by horses, as were also the 12 miles between Point of Rocks and Harper's Ferry. The latter section was so worked on account of the opposition of the Canal Company to the use of locomotives, because they frightened the canal horses. Trials of the new Davis engine, already described, having been made on the planes of Parr's Ridge, and the ability of the engine to overcome even a grade of 270 feet being fully demonstrated, the road across the ridge was re-located to adapt it for locomotive power. This was done with grades of about 80 feet per mile, increasing the distance less than one mile, and was put in operation in June, 1839, when a pecuniary consideration to the Canal Company having meanwhile steadied the nerves of their horses, the whole line, as far as completed, was in operation with locomotives.

The increasing demand for these machines was partly met by replacing the engines at work on the Washington branch by others built by Mr. William Norris, of Philadelphia, having horizontal boilers and one pair of driving wheels of 4 feet diameter, with a truck in usual form, cylinder of 10½ inches diameter and 18 inches stroke, and weighing 9 tons with 5½ tons on the drivers. It was a form of engine much better adapted to the fast passenger service of that line, and was in its day a most efficient machine. These engines used wood fuel and were followed by others on the same general plan, using the same fuel, which was found less costly than the anthracite coal supplied to the Davis engines.

The crossing of the Potomac at Harper's Ferry was effected by a timber bridge, designed by B. H. Latrobe, and built by Wernwag. It was a noted structure in its time, being modeled somewhat upon the plan of a celebrated bridge over the Rhine at Schaafhausen. A peculiarity of the line at this point necessitated a bifurcation of the bridge at its western end, forming two branches of a Y; the one arm connecting by a single span to the south with the Winchester and Potomac Railroad, then just completed, to meet it; the other, by two spans to the west, being the prolongation of the main stem of the railroad.

This arrangement, involving the use of switches midway of the bridge, was necessarily somewhat complicated, but it has continued to

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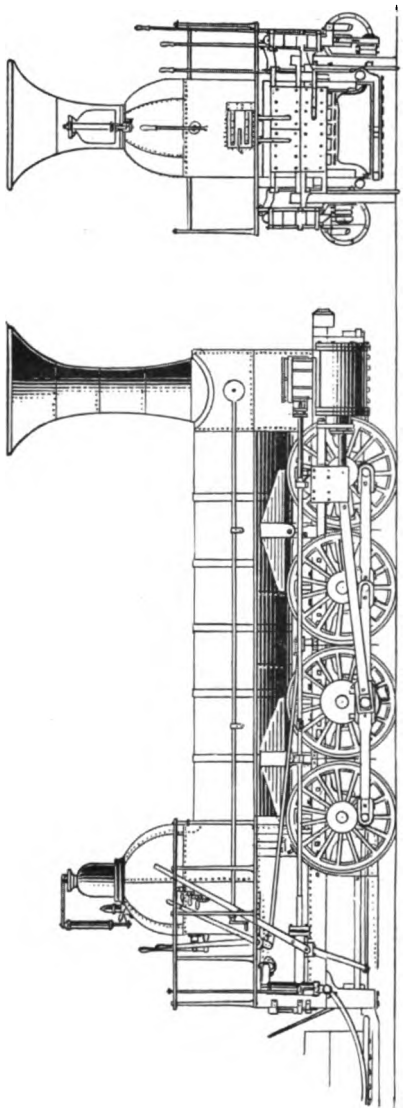

Anthracite Coal-Burning Locomotive "Delaware," built for Philadelphia and Reading Railroad by Ross Winans, Baltimore, 1846.

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this day, although the rapidly increasing weight of the locomotives and trains caused the replacing of the timber bridge at a later date, 1852, by an iron truss patented by Wendell Bollman, the details of which were arranged by Mr. J. H. Tegmeyer, and which has been claimed as the first iron truss railroad bridge.

The extension of the line from Harper's Ferry westward to Cumberland, a distance of 96 miles, was now being pressed forward with his characteristic energy by Mr. Latrobe, the engineer of location and construction, and was opened to Cumberland in November, 1842. The engineering features do not call for special comment here, beyond reference to the track superstructure which still adhered to the sub-sill. It consisted of a wooden under-sill and stringed piece with cross-ties and blocks between them, the whole fastened by wooden pins. The iron rail was of the bridge form, weighing 51 pounds to the yard, with cast-iron chairs at the ends and in the middle of the bars, which were held firmly down to the stringed piece by screw bolts at the ends and hook-headed spikes at intermediate distances. The whole resting on a bed of broken stone 1 foot in depth.

A marked stage in the development of the engines for this extension must now be noticed. In 1836 Ross Winans constructed two engines for the road with the Cooper vertical boiler and with the cylinders placed horizontally at the rear end of the frames and connecting by the spur wheel and pinion through the independent shaft to four driving-wheels as in the "Grasshopper" engines. This arrangement enabled the boiler to be depressed and lowered the center of gravity of the whole machine some 12 inches, thereby giving greater stability at high velocity. This plan was further developed in 1842 in seven engines, built for the Western Railroad of Massachusetts, which had eight drivers instead of four. These were powerful machines, but were not a success. They were entirely unsuited to the use of wood; and as anthracite could not be furnished on that line at any reasonable price, the engines were soon laid aside.

On reaching Cumberland, the Baltimore and Ohio Railroad tapped for the first time the bituminous coal region, and a locomotive was wanted to burn that fuel. Mr. Winans, to meet this demand, designed an engine with horizontal boiler and horizontal cylinders of 16 x 22 inches, connecting with eight driving-wheels 33 inches in diameter, through a spur wheel and pinion as heretofore, and weighing 23½ tons. Twelve of these engines were placed in service between 1844 and 1846,

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and were a great advance upon anything which had preceded them. They burned bituminous coal and handled the coal trade in its incipiency. They led the way in the development of the coal-burning engine, the next step in which was the building in 1846, by Mr. Winans, of four engines for the Philadelphia and Reading Railroad with horizontal boilers, horizontal cylinders and eight driving-wheels of 46 inches, diameter, the use of spur gearing being dispensed with.

I mention these Reading engines, of which I am fortunately able to show you a lithograph, for they had no equivalent on the Baltimore and Ohio Railroad, and yet, as the first built with large fire-boxes and horizontal cylinders, connecting directly to the four pairs of driving wheels, they mark an important stage of progress to the next engine, built for the Baltimore and Ohio Railroad, and known as the "Camel." The first of this class of engines was placed on the road in 1848, in response to specifications prepared by Mr. Latrobe and limiting the weight to 22 tons. It had 17-inch cylinders of 22-inch stroke placed horizontally, and eight 43-inch drivers; the fire-box was larger and wider than ever before. In distributing the weight of this engine on its four pairs of driving-wheels, it became necessary to remove the large dome from over the fire-box to a point well forward of the middle of the boiler, where also the throttle valve and engine-man were placed. This humped feature suggested the name "Camel," which was given to the first engine of this form, and the name has since adhered to the type. These engines were developed in the next few years by increase in size of boiler, fire-box and cylinders, which latter were 19 x 22 inches, with a total weight of engine of 28 tons. They were most efficient and enabled the heavy grades of the new road west of the Cumberland to be worked successfully. In fact, the engine was designed for these grades, and there was no other then available for that purpose.

This engine has been further developed and improved upon since, largely in the shops of the Philadelphia and Reading Railroad, to which road many of them were supplied, where its crudities have been removed and its deficiencies supplied by the skill of the several engineers of machinery of that great corporation, notably by the late James Milholland and by Mr. J. E. Wootten; but many of its characteristics and important features can readily be traced—the horizontal cylinders, the large and wide fire-box, the flat connecting and side rods; the latter arranged with fixed bushings incapable of adjustment, a feature much reviled at the time, but now recognized and adoped by the best builders.

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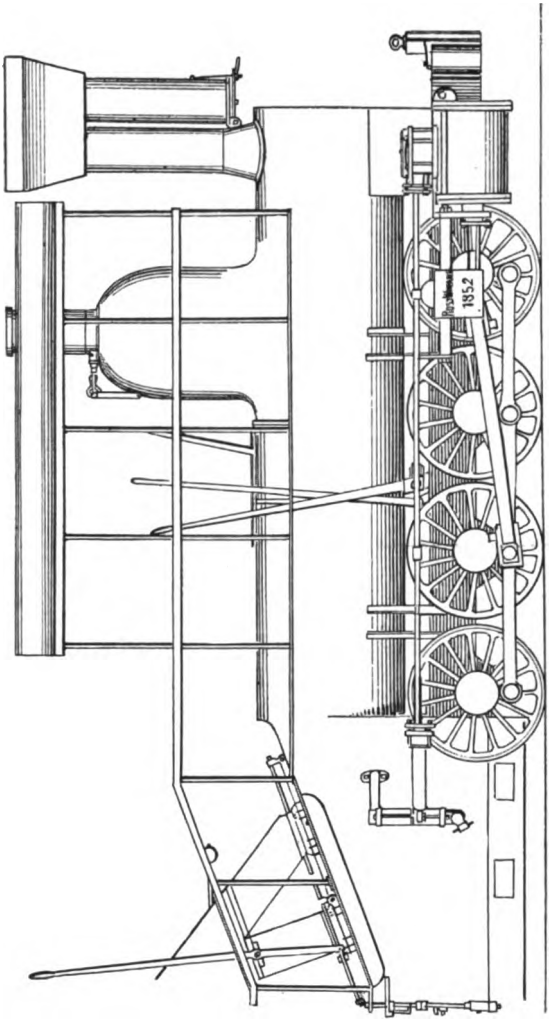

The Improved "Camel" Engine, as supplied to the Baltimore and Ohio Railroad, by Ross Winans, Baltimore, Md., 1852.

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The progress of the road beyond Cumberland was delayed for five or six years by many and peculiar causes. While the original charters granted by Maryland and Virginia were perpetual, yet there were limitations as to time of completion which were already exceeded, and which now operated to require further legislation; while a limitation in the Pennsylvania charter operated to annul it altogether.

Maryland had a very large amount of money invested in the canal which had not yet reached Cumberland where it now rests, and still cherished the hope of its further extension, so that the railroad location must still be subservient thereto. Virginia endeavored to couple with her grants of extended time, onerous stipulations limiting the terminal point to Wheeling.

The railroad company would have preferred to locate its line to a more southern point on the Ohio, preferably to Parkersburg at the mouth of the Kanawha, and was not insensible to the advantage of a terminus at Pittsburgh, but was evidently averse to Wheeling.

The well-known diplomatic skill of President Louis McLane had full scope in reconciling the difficulties, and with some measure of success, for though the Act of Virginia, of March, 1847, stipulated Wheeling as the terminus, yet the conditions were less onerous than certain earlier acts, under which the company declined to proceed. It contained a section which seems to have been designed to facilitate connection with a branch to Parkersburg. This was so promptly availed of, that the Northwestern Virginia Railroad, now the Parkersburg Branch Railroad, was well under way before the road was opened to Wheeling, and was completed a few years thereafter.

The definitive location of the whole line from Cumberland west was now pushed forward by numerous parties. At the west end three alternative lines of approach to the Ohio presented themselves after avoiding the southwest corner of Pennsylvania, distant from Wheeling some 40 to 50 miles. The first reached the Ohio at the mouth of Fish Creek, the most southern point permitted by the Act of 1847, and then followed the river 25 miles to Wheeling. The next touched the river at the mouth of Grave Creek, 12 miles below Wheeling. The third *via* Wheeling Creek only reached the river at the City of Wheeling. The first, although the longer, had light grades and no heavy work, and was the choice of Mr. Latrobe. The second and third crossed several divides involving much heavy work, each of the two including Board Tree Tunnel of 2 300 feet, with 80-foot grades on either side.

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Wheeling, fearing an attempt to deprive her of the benefits of the actual terminus, protested against the adoption of the Fish Creek line as contrary to the spirit of the Act of 1847, and of a subsequent agreement between the city and the company. She called to her aid Jonathan Knight and Charles Ellet, Jr., as engineer experts. Mr. Knight had resigned from the Baltimore and Ohio Railroad, and had been succeeded by Mr. Latrobe, in 1842, about the time of the completion of the road to Cumberland, and now brought all the knowledge and skill acquired in the company's service during his fifteen years of connection with it to bear in opposition to its plans and to the views of its chief engineer, his former assistant. The situation may well have been embarrassing to Mr. Latrobe, whose notions of professional ethics were of a different order.

It will be too long a story to enter here into the details of the controversy. It was skilfully managed on both sides. Reams of paper were filled with the calculations and discussions of the engineers and with the arguments of counsel, among whom were Daniel Webster, Reverdy Johnson and J. H. B. Latrobe. The matter was finally adjusted by arbitration in the autumn of 1850, resulting in the adoption of the Grave Creek line, having caused a further delay of three years.

The questions of location having been settled, the construction of the 200 miles from Cumberland to Wheeling was pressed at all points with energy by able assistants. William H. Small, George Hoffman, Thomas Rowles, James L. Randolph, George McLeod, Charles P. Manning, Benjamin D. Frost, Alfred F. Sears and Albert Fink, the latter a Past President of the Society, are names most of which are familiar to you.

The track from Cumberland westward was laid throughout with 60-pound rails on cross-ties, with a wrought-iron H or lip chair of boiler plate. The latter, though better than anything which had preceded it, was very unreliable. The graded road was ballasted ahead of the track with broken stone, sand or gravel, as might be obtainable. The track was not yet perfect, but a great step in advance was made, when there was no longer a sub-sill to churn away in the mud beneath a real or pretended ballast.

Mr. Latrobe's adoption of the 116-foot grades in crossing the mountains was deemed bold and hazardous, but he had been trained in a school which was self-reliant. He made his calculations of what he required and what he felt sure could be accomplished, and found in Ross

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Winans the man who recognized the occasion, and undertook the task of designing an engine that should perform the work. Well do I remember accompanying Mr. Winans on his first test of the new engine on the grade for which it was designed. The occasion was the opening of the new road in July, 1851, to Piedmont station, 28 miles west of Cumberland. Here begins the 116-foot grade which continues for 17 miles to the top of the Alleghany Mountains. The track-laying force was busily engaged some 3 or 4 miles beyond, and this short distance was all that was available to see what the engine could do. As a test of its full power it did not amount to much, but there were attached to the engine, in addition to the passenger cars of the excursionists, a number of car-loads of iron rails, all that were available, and with these the engine started at the foot of the grade and made its run at the rate of 10 or 12 miles per hour with a large surplus of steam, which was cut off at half stroke. On arriving at the end of the track with steam blowing off, there was much congratulation. It was realized that the question of ability to work this grade, of which great doubt had been expressed, was clearly demonstrated with a large reserve of power in the engine, and from that time forward no more was said of impossibilities.

In the following winter and spring the track was carried to and across the slopes of Cheat River. Here, the deep ravines of Tray Run and Buck Eye Hollow were crossed on grades of about 105 feet to the mile at an elevation of some 160 feet above the bed of the ravine by trestle work, soon replaced by permanent structures, the detail of which was executed by our Past President, Albert Fink, then office assistant of Mr. Latrobe.

The next great feature was Kingwood Tunnel of 4,100 feet in length. This work not being ready in time to pass the track-layers through without much delay, Mr. Latrobe determined to grade a track over the tunnel and, passing the iron over it, to continue the work of track-laying beyond. The maximum grade involved in this was 10 feet per hundred, which, though intended for horse-power, was worked by the camel engine, which pushed over it one car of rails without any serious difficulty. At a later period, after the opening of the road, when the arching of the tunnel was in progress, this temporary line was revised with a maximum grade of 5 per cent., and the whole traffic of the road was carried over it for some months. The crossing of this tunnel effected, the progress of the road lay on easier lines for some 80 miles. The Monongahela River was crossed by a bridge of three spans of 200

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feet each. The masonry of the piers and abutments of this bridge were entrusted to our late member, Mr. James L. Randolph, who was the division engineer, the superstructure being designed by Mr. Fink.

At Board Tree Tunnel, about 164 miles west of Cumberland, was encountered another temporary delay by reason of the failure or inefficiency of the contractors. Here, Mr. Latrobe again determined to avail of a temporary crossing over the top of the hill. This time the maximum gradient was 6 per cent. The crossing was effected by a series of zigzags, or switchbacks, two on one side of the hill and five on the other. The topography was such that there were two curves on the line of 180 degrees and more of continuous curvature in one direction; the limiting radius was 300 feet. Mr. Latrobe has himself so well described this crossing that I will not repeat it here at length. This temporary road was most successfully worked for some five or six months. Beyond this point for 36 miles to its terminus at Wheeling there are no features of special interest which I now recall. The last rail, completing the track to that point, was laid on the eve of Christmas, 1852, and early in the next month, January, 1853, the formal opening of the road to Wheeling took place.

And now, ere closing an address which has, I fear, already taxed your patience too severely, I will say a word or two of the man who brought this great work to a conclusion. Mr. Latrobe was identified with the Baltimore and Ohio Railroad almost from the very beginning. He had been intended for the law, but after completing his course of study, and being admitted to the Maryland bar, concluded that railway engineering was more to his taste. He entered the service as an assistant to Mr. Knight, and I am quite satisfied that Mr. Knight in all his career never had a more faithful one. None could have been more loyal than he, and when he was no longer an assistant, but himself the chief, his modesty was as marked as before.

While his was the mind which planned and directed all, one might have supposed that the ideas emanated from the assistants, so ready was he to give them credit for their well-executed details. He took pride in the work with which he had been so long identified. He never could have accepted a retainer to oppose its interests whether he was on its pay-roll or not. He left behind him in that portion of the Baltimore and Ohio Railroad which he built, a monument to his professional skill; and in the hearts of his assistants, a loving remembrance not soon to be effaced.

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SOME NOTES ON THE HOLLAND DIKES.

By WILLIAM STARLING, M. Am. Soc. C. E.

WITH DISCUSSION.

In 1889 the author spent a few days in Holland, and was much impressed by the engineering marvels that he saw. The time was too short and his acquaintance with the country too superficial to make his visit of any profit. He determined, however, to avail himself of the first opportunity for another visit. With this view, after acquiring a little knowledge of the Dutch language, he had some correspondence with acquaintances in Holland, and read a few books that were recommended to him. That which he most desired to study was the dike system. Information was sought especially on the following points:

Sinking and permeable foundations; bad material; prevention of leakage under the dike; dimensions compatible with safety; use of cul-

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verts through dikes; protection against storms; causes of breaks; specifications of work; experience with outlets; methods of administration.

A second visit was paid during the summer of 1891. All these subjects were examined to some extent, and more or less information will be found about them in the following pages. The author found the Dutch engineers both intelligent and courteous. He is under particular obligations to Mr. W. F. Leemans, The Hague, Chief Engineer in charge of the Great Rivers; C. F. M. H. Schnebbelie, Bois-le-Duc, Chief Engineer in charge of the New Maas Mouth; Mr. Wisboom, his assistant; Mr. P. H. Kemper, Utrecht, Engineer of the Merwede Canal; Mr. Koole, Resident Engineer of the Westkapelle Dike; Mr. H. E. Bruyn, of the Waterstaat office, at The Hague, and Mr. J. J. L. Bourdrez, of Middelburg, Zeeland, student of Engineering at the Polytechnic College at Delft. All these gentlemen took particular pains to supply him with information, at the cost of some trouble to themselves.

The author visited some of the most remarkable seaworks, which filled him with admiration, but also with despair, as there seemed hardly a possibility of ever being able to imitate them. What interested him particularly were the river dikes, from their close analogy to those of our own country, and it was those, accordingly, that received most of his attention. The Dutch have had the same problems to solve as we, and a study of their methods, adopted after a long experience, ought to be of some profit to us.

The country which we call Holland is denominated by its inhabitants the Netherlands or the Low Countries—the name Holland being restricted properly to the two provinces of North and South Holland. While, on the whole, a flat region, it is not uniformly so. It consists of two separate divisions, of different geological periods, and, of course, diverse in their types. One is strictly alluvial, that is, it was formed while man was on the earth; the other is pre-alluvial, though still belonging to the Quaternary epoch. Briefly, the two divisions may be called uplands and lowlands. The uplands comprise about 41 per cent. of the whole surface of the Netherlands.

The lowlands are not themselves of uniform constitution. They were formed in different ways, are composed of different ingredients, and their history, up to the present time, is altogether dissimilar. Part was formed by deposit from the sea, part from the great fresh-water rivers, and

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part from the decay of forests and swamp-growths. The consequence is that the lowlands are composed of strata of clay, sand and peat, the proportions and relative situation of each varying according to locality. Sand and clay are found everywhere, sometimes the one and sometimes the other being the predominant constituent. Peat is often absent altogether, generally appearing at the surface, and sometimes below the surface, at variable distances.

Peat is a rotten, spongy and very compressible soil, which retains and transmits water, and in its natural state is not suitable for cultivation. An artificial sod may be created on the top of it by continued labor, planting, pasturing and manuring, and some very good lands have been formed in this way. In the greater number of cases, however, the peat has been cut away altogether, to the depth of perhaps 6 feet or more, until the solid clay was reached. This generally underlies the peat, forming a stratum of variable thickness. It is very rich and not very stiff, and when treated in the thorough manner which the Dutch farmers practice, it makes a soil of incomparable fertility.

The alluvial land, being produced by deposit from existing rivers and existing seas, the level of which, relatively to the land, has not materially changed, cannot be built up by such deposit to the highest mark of these waters, and must, hence, be liable to overflow in time of flood or storm. Still more must this be the case when the surface strata have been removed to a considerable depth. In order, therefore, that cultivation may be continuous and safe, and that improvements may be fearlessly undertaken and habitations constructed, the land must be protected from inundation. It is well known that this has been effected in Holland by its famous system of dikes.

Dikes, in one form or another, have existed in Holland from a very early time, but the beginnings of the present system are said to date back to about the thirteenth and fourteenth centuries. In the fifteenth century (1421) a terrible storm carried away the sea-dikes for a great distance, destroyed seventy-two towns and villages, inundated 200 000 acres of land, and is said to have cost the lives of 100 000 people. It gave an entirely new conformation to the Lower Rhine and Maas, converting an inhabited district into a great swamp, which has only lately been partially reclaimed. The "Saint Elizabeth's Flood," as it is called from the day of its occurrence (the 18th of November), is thus an important date in the hydrographic history of Holland.

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In taking measures for defense against the external waters, it is clear that the most dangerous and pressing enemy is the sea. A great part of the Netherlands is directly exposed to its attacks. North Holland is simply a narrow peninsula, with the sea on both sides. Friesland, Groningen, Gelderland and Utrecht, border on the Zuider Zee. South Holland and Zeeland abut directly on the North Sea. Much of the country is below low tide, nearly all below mean flood. Storms would cause the waves to sweep over the whole land, and the damage that they would do is best shown by the frightful ravages which they have committed in the past. The first and all-essential provision is then to be made against the sea.

Holland is intersected by five large rivers—three branches of the Rhine, the Maas and the Schelde. Of these, the last is hardly a Dutch river at all, as it barely enters the territory of the Netherlands with its estuaries. The others have almost all of their alluvial part in that kingdom. It is impracticable to shut off these rivers from the sea. They are too large. It would be practicable to dam them, but so great is the volume of water which they discharge that it would be well-nigh impossible to keep them drained. Sluices could hardly be built large enough to carry off their waters into the ocean at low tide, and even if this were possible, they would have to be drained by pumping at high tide. Now, the greatest draining machines in Holland have a capacity of about 100 000 cubic meters per hour. The high-water discharge of the smallest of the great rivers, the Maas, is more than eighty times that much. In time of storm, the sea sometimes remains at an extreme height for several days, and the drainage from the river valleys would accumulate in that time to an amount which would be almost as formidable as the sea itself. Therefore, the mouths of the rivers must be left open. But if the mouths are left open, the floods of the ocean will rise in them and overwhelm the lowlands just the same as if they were directly on the sea-coast. Not only so, but the rivers themselves are subject to formidable freshets, rising nearly as high as the floods of the sea. On all these accounts the rivers must be diked, or they will overflow the land.

Owing to the flat slope of the Rhine flood-plain and the close proximity to tide-water, the rivers show a strong tendency toward the delta formation—a tendency to split and wander. Therefore, especially in the lower part of their course, they inter-osculate and are united with

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one another by a network of small watercourses. The Lek and the Waal are united by the Noord and the Botlek. The Waal and the Maas are so close together at Fort St. Andries that they have been allowed to merge together in time of flood, and they unite at Woudrichem. By these larger and smaller streams the fluvial part of the Netherlands is divided into a number of islands, each of which has to be completely enclosed by a ring dike. Of these islands some are large and some small. The portion of North Holland lying north of the Y—now the North Sea Canal—is an island. So is the still greater territory composed of portions of North Holland, South Holland, Utrecht and Gelderland, bounded on the north by the Y and the Zuider Zee, on the east by the Yssel, on the south by the Lek and its continuations, and on the west by the German Ocean. A very important island is included between the Lek and the Waal, from their point of separation, and bounded on the west by the Noord, and comprehending the districts called the Over Betuwe, the Neder Betuwe, the Land van Buren, the Land van Culemborg, the Tielerwaard, the Vijfheerenlanden and the Alblasserwaard (see Plate LIV). Being completely inclosed by dikes, these tracts can have no natural drainage. They must either discharge their rain water through sluices into the rivers or the sea, if the latter be low, or be pumped out if the external water be higher than the internal. In point of fact, both these systems have to be resorted to. As the several portions of each inclosed island are of different elevations, some being one foot below mean flood and some fifteen or twenty, the lowest tracts are usually themselves diked in. Moreover, as precautions against possible breaches in the river or sea dikes, there are occasional interior lines of embankment, dividing some of the islands into subordinate districts, and serving, like the water-tight compartments of a ship, to limit the extent of the injury.

Dikes may therefore with propriety be classified as sea dikes, river dikes and internal dikes.

First.—The sea dikes are distinguished by their great strength. As they have to stand the shock of very heavy waves, they must be built with a very long slope—as this has been found one of the most effectual safeguards against damage by breakers. A "sea-beach slope" of 40 or 50 to 1 is of itself usually a sufficient protection against further cutting. Of course, it is not practicable to give so flat a slope as this at all places, or perhaps at any. The front of the dike must then be protected by

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artificial means, usually by stone, giving, however, to the dike as flat a slope as possible, consistently with economy—else the stone revetment would have to be too heavy, and would be too precarious. The result of endeavoring to reconcile these two independent principles has been the evolution of a cross-section for the sea dikes which combines some features of each. Of course, mere hydrostatic considerations are entirely lost sight of—the necessity of providing for wave wash making it obligatory to adopt dimensions several times greater than the most liberal allowance for mere pressure would prescribe. Furthermore, as the sea is raised by storms sometimes to a very great height, there must be an ample margin in this respect, the dikes, for complete safety, being 18 or 20 feet higher than mean flood. These extraordinary dimensions are not of universal application. On the Zuider Zee, especially on the western shores, the exposure to storm and tide is not nearly so great as on the North Sea, and the dikes are reduced in size accordingly, though the smallest of them are far larger than would be required to stand the mere head of water to which they are subjected.

Mr. Caland, a well-known Dutch engineer, divides the sea dikes into three classes, as follows : Dikes of the first class, situated immediately on the North Sea, in unfavorable locations, with little or no foreshore, with steep banks, and with a direction perpendicular to the prevailing (westerly) winds. Such embankments have a crown of at least 4 meters, and a front slope of at least 10 to 1, a little convex, with the lower portion somewhat steeper, about 6 to 1, protected by a heavy stone pitching. Dikes of the second class, lying further inland (the shores of Friesland and Groningen), or at the mouths of great rivers. The crown of these is at least 3.5 meters, and the front slope 6 to 1. Dikes of the third class, still less exposed, not on the North Sea, with a crown of at least 2 or 2.5 meters, and a front slope of 3 or 4 to 1. All these dikes have a back or land slope of at least 2 to 1, an inner berm or banquette of 4 to 10 meters, and an outer berm somewhat sloping in profile, and generally carried up one or two feet above ordinary flood tide. Dikes of the first and second classes are often built higher on the outside. The road is then placed lower down, on the back slope. Sometimes the outer slope is made concave, instead of convex, so as to give the stone pitching a flatter slope.

These remarks of Mr. Caland were made many years ago, and it is believed the dimensions now in vogue greatly exceed those given above. The author is sure that this is the case with all seen by him.

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PLATE LIV.

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Not all of the sea-coast needs artificial protection against inundation. The western shores are naturally defended, in a great measure, by a chain of dunes or sand-hills extending, with little interruption, from the Helder, the northern extremity of the peninsula of North Holland, to the mouth of the Maas, near Rotterdam. Dunes also exist on the western coasts of the islands of South Holland and Zeeland. They are heaped up by the action of the winds, like snow-drifts, and are subject to gradual and continual migration inland, from the same cause which created them. Similar phenomena are common on other sea-coasts, notably on the eastern shore of our own continent, where the landward movement, apparently almost irresistible, has done serious damage and threatens more. In Holland the natural mobility of the sand has been counteracted, to a great extent, by planting the seaward slopes of the dunes with a species of reed grass (*Arundo arenacea*), locally called helm. This takes root even in pure sand, and holds the material in place. It is planted in tufts about 2 feet apart. The few points on the western coast where the dunes are missing, the western shores, and most of the eastern, of the Zuider Zee (the exceptions being where the uplands abut directly upon the water) are protected by dikes.

It may be mentioned, in this connection, that the term dike is generally applied by the Dutch only to works of primary importance, intended to keep out sea or river water, or which have at some time served such a purpose. Embankments of inferior grade usually go by the name of kaden or quays. The subject of protection against winds and waves will be discussed at greater length hereafter.

Second.—In considering the rivers of Holland, the reader will probably have to reconstruct his map of the Netherlands. Take an ordinary atlas and see the delineation of the water courses of this kingdom. You see that at Pannerden the Rhine splits into two branches, the Waal and the Neder-Ryn. Near Arnhem the Neder-Ryn is again divided, the Yssel of Gelderland (so called to distinguish it from another water-course further down, the Yssel of Holland) diverging toward the north and emptying into the Zuider Zee—the other branch pursuing its way, under different names, toward the west, past Rotterdam. It is still the Neder-Ryn until it reaches Wyk-by-Duurstede, where (on the map) it puts forth another branch called the Kromme Ryn, which seems to run past Utrecht and Leyden to the sea at Katwyk, becoming successively the Oude Ryn and the Leidsche Ryn in its course. At Vreeswyk, the

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Neder-Ryn again sends forth the Vaartsche Ryn, meeting the Kromme Ryn at Utrecht, and the Hollandsche Yssel, which seems to run past Gouda, and to re-enter the parent stream just above Rotterdam, thus forming an island. At Utrecht, the Kromme Ryn loses a part of its water to the Vecht, discharging into the Zuider Zee. In the same basin appear the Amstel and its tributaries, also emptying into the Zuider Zee, and in North Holland the Y, which appears to be the drain of a chain of lakes lying north of Amsterdam.

Now, this does not in the least convey a correct idea of the present situation. Of the streams named above, three alone are living rivers, having an unobstructed course to the sea. These are the Yssel of Gelderland, the Lek and the Waal. All the rest have long since been cut off from all connection with the main river by dams, and have also been dammed at their lower ends, converting them into pools or canals. Some of them have been dammed several times, dividing them into different levels. They are used, in this condition, for several purposes. The dams are pierced by locks, thus still allowing them to be used for navigation, and, in fact, making them more serviceable for that end. The water is maintained at a high level, enabling them to be employed as reservoirs for irrigation and for the supply of water for stock and for drinking. By means of powerful pumping apparatus, water is brought into the elevated pools from the lower lying adjacent fields, and they thus serve for drainage. To accomplish these different purposes, their beds have been enlarged in two different ways. They have been deepened by dredging and the banks have been raised by dikes. Their storage capacity has thus been greatly increased, and their usefulness for navigation multiplied many times. They have been connected with one another by canals, forming a vast network of water-courses, all under perfect control, and all, except the main rivers, perfectly stagnant. The usefulness of the high-water level will be more plainly seen hereafter. Suffice it to say now that it serves the purposes, first, of giving greater capacity; second, of furnishing water to the higher lying lands; and third, of affording drainage, in a natural way, into the sea at ebb-tide, or into the main rivers.

The three rivers just named, arms of the Rhine, are of very different sizes. The Yssel carries about one-ninth of the total discharge, the Lek two-ninths, and the Waal two-thirds. They are all diked on both sides during their course through alluvial ground, which is about

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equivalent, roughly speaking, to their course from the first point of separation, namely Pannerden, below Emmerich, to their mouths. The Maas is diked, with some exceptions to be noted, from about Mook, a little above Grave, to near Woudrichem, where it empties into the Waal. (See Plate LIV.)* The Schelde is diked from above Antwerp. Very little of this river, however, is in Holland.

The river dikes are much less massive than the sea dikes, and are of very variable dimensions, according to the material of which they are built, the foundation on which they rest, the extent and value of the country which they protect, the resources of the local organizations which maintain them, and many other considerations. They, also, like the sea dikes, are exposed to wave action. The rivers are wide and have long bends, through which the winds have full sweep. The destructive force of the wind is found to be specially great when it blows up stream. It is necessary then to make provision against this danger. River dikes are exposed to other perils, some general to all dikes, some peculiar to rivers, some incident to particular rivers, which cause them to be built in forms very different from those which might have been expected.

The sea dikes are constructed of such superfluous strength that they are not generally instructive as a study. The river dikes have not been built with the same lavish expenditure. They bear the marks of having been evolved by a gradual process from small beginnings, and it is evident that economy has been studied in their construction and maintenance. For these reasons and also because they were closely analogous to those which the author had to look after at home, he gave them much more attention, and inspected considerable stretches of them personally.

Before proceeding to the consideration of the river dikes as they actually exist, it will be proper to examine a little the principles of dike building. In Holland, all elevations are referred to "Amsterdam Datum," expressed by the abbreviation "A. P." (Amsterdamsch Peil). It is supposed to have indicated mean flood at Amsterdam when the Y was open. The Y is now a part of the North Sea Canal and is dammed

* This plate is a copy of the map of the "Rivers of the Netherlands," appended to the pamphlet entitled "Les Voies de Navigation dans le Royaume des Pays-Bas," published under the auspices of the Dutch Government, for the Manchester Congress of Internal Navigation, in 1890. The figures indicate elevations above Amsterdam Datum. The letters *d* and *k*, prefixed to figures, mean dike and *kade*. Where no letters are prefixed, the elevation is that of the natural surface.

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off at both ends. This mean flood height is shown by a certain bolt in the front wall of the Bourse at Amsterdam, and also by two other bolts in different parts of the city. Other data are high water (H. W.), low water (L. W.) and "Mean River" (M. R.). This last signifies the mean of the stages from May 1st to October 31st (the six summer months), for the last decennium that ended in a multiple of 10—for instance, from 1880 to 1890.

It might be supposed that the rules prescribed for the construction of earthen reservoir walls would be applicable to dikes, but it is not so. There are local and economical considerations which forbid the use of the precautions usually taken with reservoir embankments and necessitate the employment of others in their stead. Dikes, in short, are cheap, low and very long reservoir-walls, designed to contain water for a limited time. They cannot generally be built on firm or impervious foundations and must be made of the material which is closest at hand, which may be good or bad, but which is usually abundant. They are almost always placed directly upon the surface of the ground. In other words, both the embankment itself and its foundation must depend upon the constantly varying nature of the soil. These are the ruling conditions to which the construction must conform. The alluvial soil of Holland, like any other alluvium, is composed of strata of various qualities of earth, differing in density, in tenacity, and in their capability of being made water-tight. All natural soils are more or less porous and transmit water pretty freely; but some still retain their quasi-fibrous texture, and have their tenacity not impaired, or at least destroyed, by being infiltrated. Others become entirely disintegrated, and some almost lose their solid quality and run like a liquid. In consequence of their porosity, natural soils adjacent to water-courses become saturated to a long distance and nearly to the very level of the water itself.

Now, it is perfectly possible to construct a good and tight dam or dike out of poor material, by devices familiar to engineers, such as building in thin layers, rolling, watering, ramming or tamping, puddling, and the like. But if the natural soil be porous to a high degree, there will be leakage under the embankment to an extent which may be very inconvenient and even extremely dangerous. The water which thus transpires through the soil is called by the Dutch "leak-water," and by the Americans of the Mississippi Valley "sipe-water" (pronounced *seep*). It is sometimes enormous in quantity. If the soil be of a fibrous

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and tenacious kind, so that a certain continuity and coherency are preserved even in a moist or wet state, the evil will probably be confined to mere leakage, which will be embarrassing or damaging in proportion to its quantity, its duration and the means of getting rid of it by drainage. But if the soil be composed of loose and independent particles of small size, it may become exceedingly unstable and even mobile when saturated with water. A fibrous or honeycombed soil, though never so porous, yet presents, as it were, a network of capillary tubes, which afford a strong frictional resistance to the flow of water and diminish the hydraulic head by so much. But earths of the second kind permit their particles to be perfectly enveloped by the liquid and held in suspension by it, in proportion to their smallness and lightness, perhaps also to the cohesive union between the water and the particles, which is different in different earths. Hence result a variety of treacherous and semi-fluid soils, commonly classified together as "quicksands," which instead of remaining in place and resisting the passage of water, are actually carried by it and are, as it were, incorporated with it. It follows that such soils will transmit the pressure of the external water with comparatively small loss. It is evident that if the water stand very high against the embankment, the pressure transmitted under the bank will be exerted upon the layer of strong soil, if such there be, overlying the quicksand, and if this be thin, will "blow it up," as it is called, thus knocking the foundation from under the dike or levee, and causing the destruction of the latter, as shown in the sketch. This is no chimerical

FIG. 1.

or merely speculative danger, but a very real and threatening one. The five breaks which took place in the Mississippi Levee District in 1890, are believed all to have been due to this cause, and in "water-turning" constructions of all kinds in Holland, extraordinary precautions are taken against it.

There are other considerations connected with the existence, under the dike, of a semi-fluid mass, that deserve attention. "If a sufficient quantity of water finds its way into the center of a bank that has been put together in a comparatively dry state, it will rise and soak into the

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earth until at length what was a solid mass becomes semi-fluid, settles into a smaller space than it before occupied, and as a consequence will leave a vacuity above it. The inevitable result is the subsidence of the superincumbent earth; but instead of resting, as at first, on a resisting material, it floats, so to speak, on the semi-fluid mass underneath, and having little or no friction to overcome, slips away to a lower angle than it before stood at."* In the construction of great dams for reservoirs and the like, provision is made against this danger by carrying the work down to bed-rock or at least to impermeable strata. This is not possible in dikework, because dikes are nearly always built in alluvial soils, where rock is at an infinite depth, and even impermeable strata may lie so far beneath the surface as to be practically inaccessible when economical conditions are considered. Nevertheless, if such precautions are indispensable to safety, they must be taken, at whatever expense, or the dike system abandoned. Fortunately, means have been found of dispensing with deep foundations, considering that dikes are usually of very moderate height, and the head consequently not excessive and of only temporary duration.

For, first, it is not by any means universally the case that sandy strata exist at a short distance below the surface. It is obvious that if the top stratum of clay or strong loam be of tolerable thickness, say 6 feet, it would balance a hydrostatic pressure of double that height, and by so much the more a hydraulic pressure, diminished by friction and fall. If, therefore, the water-bearing stratum be deep and the dike not very high, an embankment strong enough, in itself, may also be safe from beneath.

Secondly, if the stratum from which danger is apprehended lie close to the surface, it is clear that the thinness of the strong top soil may be supplemented by artificial means. To this end, a layer of several feet of earth may be placed upon the natural surface on the inner or land side of the dike, to serve as a counterpoise to the column of external water. As to the dimensions of this banquette, as it is usually called,† it is hard to lay down any rule. It is a matter, in fact, that has been regulated wholly by experience. It will be seen that the width, no

* "Jacobs on Storage Reservoirs," page 84, Van Nostrand's Science Series.

† The Dutch call it merely a *binnenberm*, or inner berm, but it seems desirable to make a distinction between a berm, which is properly a portion of the natural ground left untouched, and a piece that is artificially raised above the surface.

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less than the height, has to be considered. The upward pressure of the water in the soil extends to an indefinite distance, but, when far removed from its source, is so diminished by friction and fall that it ceases to be dangerous. It will also be evident that a banquette, or an extension of the base, on the front or water side of the dike, will be serviceable to this end, by removing the source to a greater distance. Thus, by adding the exterior banquette *A*, *B*, *C*, *D*, we have transferred the maximum head from *A* to *B*, and have thus given the subterranean current a

FIG. 2

longer distance to traverse. Accordingly, we find numerous instances of both interior and exterior banquettes on the Holland dikes, of all possible dimensions, from 3 to 10 feet high, and from 20 to 100 feet in extreme width. The inner banquette is by far the predominant form, and, in fact, if there is only one, it is almost always on the land side—the outer, if any, being added as a supplement or additional safeguard. The new South Linge dike contains several banquettes of the largest dimensions. Their cross-section is usually a long convex segment, the extreme altitude being, perhaps, from one-fifth to one-tenth of the base.

In place of a banquette, a somewhat ingenious device is occasionally employed, evolved by necessity or emergency, doubtless, during the high-water season, as it has been on the Mississippi. A low interior bank is thrown up, at a distance of 20 or 30 feet or more from the main dike, and connected with the latter by wings, thus forming an inclosed space, which is allowed to fill with sipe water. The latter exerts a certain counter-pressure against the external river water, thus relieving the main dike of a part of the lateral strain and also the weak interior berm of a part of the upward pressure. This is a very common device in America, especially for the temporary cure of bad leaks, and several places were seen where it had been employed in Holland. The author was told that it was a well-known expedient there.

It may be a matter of surprise to the reader that no direct means have been mentioned hitherto of cutting off the leakage under the dike, or at least a portion of it. Such means have been tried, but their efficacy is not universally admitted. In the Valley of the Mississippi it is the almost invariable practice among the levee engineers to put what they

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call a "muck ditch" under their embankments, that is, to excavate a portion of the natural soil and to replace it with strong well-tamped earth, clay if possible, thus causing a projection, as it were, of the embankment into the earth, after the manner of a tongue and groove joint. The office of this adjunct evidently is to supply, as far as may be, the want of a foundation extending to an impermeable stratum, and to stop at least a part of the leakage, by substituting a tight for a porous soil for a part of the way. It also gives the water a somewhat longer distance to traverse. It is thought to be especially "indicated," as the physicians say, in sandy soils, and in those permeated by roots and the like. Occasionally, sheet piling has been used to a limited extent, and it has been proposed to put in longitudinal diaphragms or vertical walls or cores of cement or concrete. These latter precautions have the fault of being very expensive, and they have never been given a fair trial in this country. No more have they in Holland. Even muck ditches, though known to the Dutch engineers under the name of *Klei-kisten*, clay boxings as it were, are not much favored by them. They think that they do not go deep enough to do any material good, and cannot be made to do so without great expense. What is the use, they say, of going 6 or 8 feet into a sand stratum which is 20 or 30 feet thick? Nevertheless, the experience of the American engineers is that muck ditches, when of considerable dimensions, well filled and tamped, perform an important part in arresting sipe water, at least in the immediate neighborhood of the dikes, and that is the most dangerous point. In Mississippi, the new levees, built since the flood of 1890, have muck ditches 12 feet wide at the top, 6 feet wide at the bottom, and 6 feet deep. They were unusually dry during the flood of 1891, and so was the land behind them, though previously well known to be very much affected by sipage. The Dutch engineers themselves show that they attach some consequence to this principle, by adopting and insisting on very similar precautions in the case of culvert and lock floors, as will be seen hereafter. It may be noted here that the Dutch never use a puddle wall or any similar device, or any diaphragm of timber, concrete or other material in their dikes. The immense dikes now building for the new mouth of the Maas are furnished for part of their course with muck ditches of considerable dimensions, averaging about 4.5 meters wide at top, 1.5 at bottom, and 3 meters deep. For about two-thirds of the line, however, they are not used.

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In America our worst soil is sand. In Holland, what they most distrust is peat, or what they call veen. This has already been spoken of as a very light, rotten, compressible soil, which absorbs water like a sponge, and doubtless transmits hydrostatic pressure pretty freely. This material is found in strata of considerable thickness, as will appear from the following borings at Vreeswyk, on the River Lek (one of the branches of the Rhine), furnished by the courtesy of Mr. Kemper, one of the engineers of the Amsterdam-Merwede Canal. (See next page.)

It will be seen that under 6.5 feet or so of clay and mould, with a little peat, there exist successively strata of 3 feet of pure peat, 6.5 feet of peat mixed with clay and sand, and 60 feet of sand, alternately coarse and fine, occasionally mixed with a little mud or gravel. Accordingly, the dikes of the Lek are among the most dangerous in Holland.

In Baedeker's "Guide Book to Belgium and Holland,"* it is stated that "the first care of the constructor of dikes is to lay a secure and massive foundation, as a preliminary to which the ground is stamped or compressed in order to increase its solidity." The author was not able to verify this observation, though he questioned every engineer he met—among them several of acknowledged eminence. On the contrary, so far as could be learned, no special pains are taken with the foundation beyond the ordinary precautions to secure an intimate union between the natural soil and the new bank—and the possible preliminary operation of displacing a stratum of peat by throwing in sand, as will hereafter be noticed. As to the construction of the dikes themselves, there are pretty minute directions in the "General Regulations" (Algemeene Voorschriften) which are published for the guidance of contractors of all kinds of work. There are two sets of these, one published by the Waterstaat and the other by the corps of military engineers. Each is a volume of 250 pages or so, and contains standing regulations for the performance of work of every sort. Each is made a part of all contracts entered into with the respective organizations by whom they are issued. There are also many very practical and useful precepts contained in the "Manual" of Storm-Buysing.† From these sources principally, some

* Page xxviii.

† D. J. Storm-Buysing, Handleiding tot de Kennis der Waterbouwkunde, voor de Kadetten van den Waterstaat en der Genie. Breda, 1844-45. Two volumes, with atlas. A later edition was published in 1868. This work has not been superseded, though it appeared so long ago, and is still the recognized authority. There is a vast encyclopedic work in course of publication which will, however, eclipse everything else—the "Waterbouwkunde" of Professor Henket and others. Eight volumes of text and eight of plates have already appeared, and there are ten more of each to follow. The price will be about $200. The Dutch Waterstaat corresponds to the French "Ponts et Chaussées," and the term Waterbouwkunde embraces not only canals, dikes, etc., but roads, bridges, railways and land drainage. Henket's collection has not yet got so far as dikes—or had not last August.

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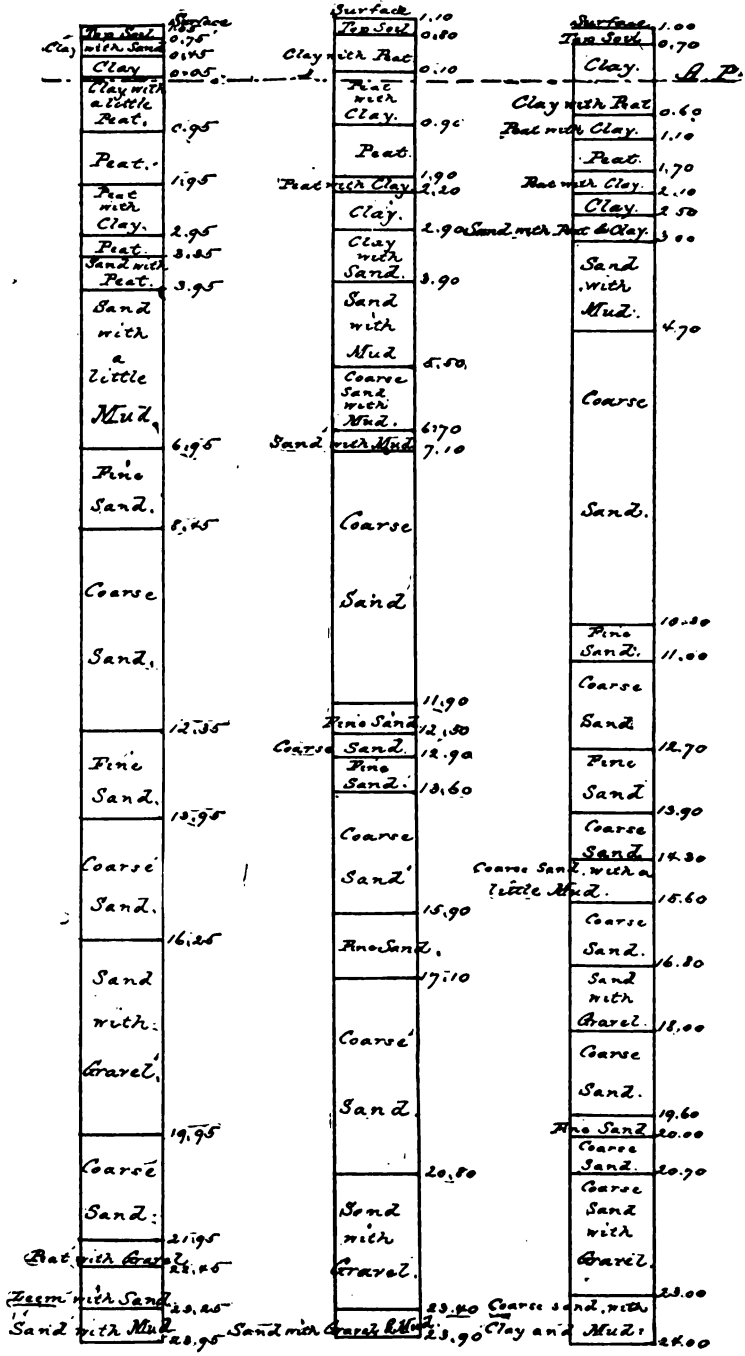

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remarks have been compiled on the construction, maintenance and defense of dikes which it is hoped will be found instructive. Where they are not consistent with the present practice, so far as this could be ascertained, they have been corrected.

Dikes should first be high enough, that is, 0.5 to 0.6 meter above the highest known water mark. (This margin has been increased to 1 meter.) They must be strong enough to resist the utmost force which the water can exert, and must to this end have the section of a trapezium, and must be composed of good, tenacious earth, which shall cohere, not only with itself, but with the ground whereon the dike rests. The crown may be from 0.5 meter to 8 meters wide, the inner (or land or back) slope 1½ or 2 to 1, the outer (water or front) slope from 2 to 8 or 10 to 1, though sometimes the back slope may itself be flat. There should be berms both inside and outside, which should usually be simply the natural surface, but which may be partly artificial, to compensate for inequalities of the ground. There should be a foreshore of varying breadth, according to the dimensions of the dike and the nature and degree of its exposure. For small rivers, the minimum may be 75 meters; for great rivers and for sea dikes, from 150 to 200 meters, which may in unfavorable cases be increased to 375 meters. Traverses 6 meters wide at the crown should be left across the borrow-pits 100 meters apart, to promote the silting up of the pits, and to serve in case of future enlargement and repair.

The trace of the new dike should of course have regard to economy of material and capacity for protection. Its general direction should run nearly parallel to that of the stream, but should avoid, as much as possible, exposure to the most destructive and prevailing winds. But especially the fitness of the foundation should be regarded, and good, high ground selected, free from creeks or sloughs. Sharp angles are to be condemned—especially re-entering angles, because they make basins in which the wind has full play to raise the water. Where changes of direction occur, they should be made by a curve, uniting the two tangents.

The criterion of height must be the recorded elevations attained by great floods. These are usually given as in still water, and freed from the disturbance of wave action. An allowance must therefore be made for this, varying according to the exposure, which of course is greatest for sea dikes and least for rivers. On some of the Zeeland islands it amounts to 6.5 meters. At Westkapelle it is 5.5. The

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height of waves is less when there is a broad foreshore and where the water is shallow, and it is less with a flat than with a steep slope.

A wide crown is an advantage, even though the dike be not habitually used as a roadway. In time of danger, it may serve as a means of transport for material, and especially it may be of incalculable benefit in case of inundation when the ordinary roads are submerged. We should, therefore, give to the crown of dikes not generally used as roads a breadth of at least 3 meters, and to those which are so used a breadth of 5 or 6 meters. (Add to this, that a wide crown affords a base whereon to put a temporary topping in case of extraordinary floods, and also may furnish the material for such a work. It gives a secure and convenient foothold for men engaged in high-water work, in protecting against storms and the like—and affords space for the storage of brush, sacks, wheelbarrows and other high water material.)

For the inner or land slope no greater inclination is necessary than that which is demanded by the natural angle of repose of the soil. (*Per contra*, such slopes sometimes slough, if saturated with water, or if the ground beneath them be porous and wet.) Nevertheless, experience teaches that slopes of less than 2 to 1 are unfavorable to the growth of grass. This ratio then is prescribed.

If dikes had nothing but hydrostatic pressure to stand, and if the material were uniformly tenacious and sound, the dikes might have a triangular cross-section. But first, earthy materials are never homogeneous, and the method of construction does not insure uniform density or compactness; and, secondly, the work has to resist the shock of waves, which is the most common cause of destruction.* The force of the shock of water or of floating bodies, as of ice, against the slopes is in proportion to the sine of the angle which the slope makes with the horizon. Therefore, a flat slope withstands such action better than a steep—which is consistent with experience.† Ice, however, is never brought by low water, but always by the flood. Therefore, the lower portion of the slope may be steeper than the upper. So, also, the lower part, being much under water, cannot be protected by sod, but must be faced with stone or other costly material. Hence, on the score of economy, we arrive at the same conclusion.

This precept has been carried into execution in most of the great sea

* Storm-Buysing, Waterbouwkunde, i, 359-60.

† Beekman says that flat slopes are a comparatively modern invention.—De Strijd om het Bestaan, p. 282.

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dikes that were inspected—the lower slope being generally 6 to 1 and the upper slope broken into portions that became gradually flatter, till, in the case of the Westkapelle dike, they reach 17 to 1. Economy in the use of basalt protection has led to the adoption of broken slopes also in the case of the river dikes, but here the cross-section is concave and not convex, thus:

FIG. 3.

The reason of this is that by reason of the rapidity of the rise at moderate flood stages, and of the shallowness of the water at such a stage (owing to the foreshore), the long sodded slope *A B* is sufficient up to the point *B*. Briefly, it is found by experience, on the Mississippi, that wave-wash is mostly confined to 2 or 3 feet below the high-water line. Above mean flood the damage would be severe, so here the basalt revetment is placed; but to save cost it is made very steep. Many cases of this kind were met. There is generally a row of small oaken piles, called *perkoenpalen*, at the bottom of these steep pitchings (and even when they are not so steep), to keep the stone from sliding. Sometimes there are several rows of *perkoenpalen* at intervals between the stones. As the Dutch rivers remain at extreme high-water mark only a day or two, the basalt revetment is not usually carried to the top of the slope.

The material of the dike should, if possible, be clay, and should be taken from the outside. If clay cannot be had in that situation, then resort must be had to the inside. Sand has little cohesion and does not make a strong and water-tight dike. Peat has too little specific gravity, often less than water, and will not answer well. Mould or arable ground, though not so good as clay, is far better than sand or peat, packs closely, and is especially suitable for dressing slopes that are to be sodded, as grass grows finely in such soil. Clay cannot usually be had in sufficient quantity to build the whole dike. It must therefore be combined with other material. This may be safely done, provided the precaution be taken to put the best and purest clay in and next to the front slope, while the poorer earth is placed in the body of the dike.* There are examples of dikes that consist of very sandy material, with a dressing of only 1 meter of clay, yet they turn water excellently. But it is easy

*Algemeene Voorschriften (ed. 1882), p. 6.

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to be seen that such a covering must be carefully treated and protected from injury, as, if the poor material be exposed, it cannot be trusted to exclude the water.

In the putting up of embankments, as well as in excavations, the grass must be taken out in regular sods and piled on one side. The base of all earthworks must be cleared of all mud, filth, straw, sedge, brush, rubbish, etc.—either all at once or in sections, as the execution of the work may require. When the embankment is designed to keep out water, the earth must be broken with a spade or plow to the depth of 0.2 meter (say 8 inches), and chopped fine. This applies also to existing earthworks to which the new work is to be joined. The embankment must be provided, in addition, with V-shaped ditches, at least 1 foot deep and wide, at the foot of each of the slopes, and with trenches of the same width and depth, at a distance apart of at least 1 meter, to the satisfaction of the Direction.* The new earth will not settle at once to the density which it will ultimately acquire, but will undergo a process of shrinking from the pressure of its own weight and the evaporation of the water which it contains. Further, the natural soil under the dike will undergo a greater or less compression, until an equilibrium is reached. Therefore the dike must be built higher than the required grade by an amount, varying according to the material and the mode of construction, from one-seventh to one-twentieth of the height (one-fifth to one-tenth in the Mississippi practice).

To obviate too great shrinkage, the embankment must be carried up in layers, level longitudinally and rather sloping transversely, of a thickness of 0.3 to 0.4 meter if carts are used, and 0.2 to 0.3 meter if built with wheelbarrows, according to the quality of the earth. If the work be done with locomotives the thickness of the layers is to be determined by the Direction.† (Scrapers, either drag or wheeled, seem to be unknown in Holland—at least, great difficulty was experienced in explaining what they were.) Horses and carts are greatly to be preferred to wheelbarrows, and should be used, if possible, at least in part. The teams must not all travel the same road, but must change their tracks constantly. The earth must be freed from foreign matters, and properly broken fine, and if the Direction requires it, tamped with heavy rammers. The number of tampers is regulated by the Direction according to the quality of

*Algemeene Voorschriften, p. 4.
†*Ibid*, p. 5.

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soil. As a general rule, there must be one tamper to four shovelers. If the nature of the ground requires more tampers, it will be so provided in the specifications. If the earth is tamped by horses, they must be managed by grown persons. (Old cavalry horses are generally used.) The embankment must not be begun until all creeks, ditches and gullies falling within the work are divested of mud and vegetable matter, their sides dug away to a slope of 2 to 1, and they filled with well-tamped earth, with a convexity of at least one-eighth of their depth.

Unless otherwise specified, borrow-pits must not be less than 10 meters from the foot of the slope, and their sides must have such an inclination as is satisfactory. At intervals of 100 meters, cross-berms or traverses must be left, 6 meters broad and of sufficient slope. Borrow-pits on the land side must be dug regularly and under the orders of the Direction. The slopes must be flat, and the small dams or balks left after completion removed. Private property must never be trenched upon, unless by written permission or by order of the Direction. Slopes must be dressed to the line and deficiencies properly made good. These works must be entrusted only to experienced earthworkers or slope dressers.*

The sods which have been removed from the base and piled at the sides are now placed upon the slopes regularly in rows, beginning at the bottom. They are closely fitted, struck in place with heavy wooden paddles and covered with fine earth. If sods cannot be had, the dike is sown with clover seed. A good close sod is the best protection for dikes which have any considerable foreshore, and it is important to preserve it in good condition. Judicious pasturing is the best means to this end. Calves and sheep should not be allowed, because they graze too closely, nor horses because their shoes cut the sod. Pasturing should not be begun until the second or third year, and not continued after the first of October. The dike will become more compact, the cattle will tramp together all mouse or mole holes, and by manuring will promote the growth of the better kinds of grass.† Low places and cracks should be filled in the spring with fine earth.

Trees on dikes are highly prejudicial. They prevent the dikes from becoming thoroughly dry. Grass does not grow well in their shade or under their influence. They are shaken even to their roots by heavy

* Algemeene Voorschriften, pp. 6–7.

† Storm-Buysing, Waterbouwkunde, i, 365–6.

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winds and the ground loosened accordingly. (Add to this, that there are frequently cavities under the arch of the roots of large and especially old trees, and that considerable streams of water may be conducted through the dike by means of the lateral roots. The latter are subject to decay and then constitute formidable channels. This salutary precept is often violated. Many important dikes have double rows of trees for long distances, for the purpose, apparently, of giving shade to travelers. Impunity has brought about recklessness which may possibly result in disaster in time of some great flood.) The planting of trees outside a dike at a safe distance, is to be encouraged by all means, as a protection against waves and ice. (A thick growth of small timber outside a dike is undoubtedly an effectual safeguard against even heavy storms. A thin and scattered growth, even though of considerable width, is not of much value.)

Ramps for road-crossings should have a slope of at least 12 or 14 to 1, and a crown of 8 feet. For important roads the slopes should be still flatter and the crown 12 or 14 feet. In weak soils the sod should not be broken, so as to preserve as much as possible the toughness of the upper stratum. By increasing the width of the base we may distribute the pressure over a greater surface and decrease the danger of sinking. To this end the lightest soil should be placed under the crown, where the mass is greatest. To obviate partially the defect of an imperfect union between the old and new earth, we may break the ground for 2 or 3 meters from the foot of each slope. The dike should be put up slowly and regularly, so that the settling may be as uniform as possible without cracks. Should these means be insufficient, mattresses must be used. (Such is not now generally the practice. See *post.*) Flat slopes on the land side are prescribed in three cases: first, when it is necessary to give a very broad base; second, when the nature of the ground compels a steep slope on the front, and third, in the case of overlaten, of which more hereafter.

Bank protection is frequently necessary for the safety of dikes. It should be resorted to before the foreshore is too far gone; for though the protection of the bank be expensive, that of the dike and the bank together is still more so, and it is dangerous besides. There should be no undue haste about it, however, for the river may take a more favorable turn and save us the trouble. Damage to a bank may result from two causes: from winds and from currents, or from both combined.

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Winds and the waves which they produce affect the upper portion of the bank alone, without injury to the lower portion or beach—nay, the latter may increase at the expense of the former. The current, however, always works in deep water, scours out the beach, and sometimes undermines those dikes which have but little foreshore and causes them to fall in. The repair of such accidents is always costly and often precarious, and it frequently happens that after all our trouble we have to build a new line. For defense of the upper bank against waves, doubtless direct revetment by stone or other suitable means is the best. For the prevention of erosion by currents, hoofden have been found efficient. (Hoofden, literally heads, are of very general use in Dutch engineering. The term hoofd is applied to any spur dike, but especially to the elaborate constructions which are designed to resist the action of the winds and currents of the sea. They are found at the Helder, at the Pettemer and Hondsbossche sea-works, at the Hoek van Holland, at Westkapelle (where there are miles of them) and elsewhere. They are built of clay, faced with stone and with or without piles driven into them. They are mostly intended (the stone part at least) to be submerged at high water. They have a very long seaward slope (at the Helder about 40 to 1). The strongest and most elaborate that were seen were at Westkapelle, and details of them will be given when that place is particularly described. For bank protection in rivers hoofden should extend to the deepest part of the stream, or as near thereto as practicable. To this end zinkstukken* should be constructed, from 0.6 to 1 meter thick, and ballasted with stiff clay and stone. A length of 180 meters can be given to such a mattress, which length, however, though measured from the low-water margin, may yet not reach the deepest part of the channel. Yet even though it fall a little short, it will still be serviceable. Should it be undermined at the foot and settle or slide a short distance, this can be made good by a shore-mat at the landward end.†

Zinkstukken cannot well be made more than 12 to 16 meters wide. To cover the whole bank by such means would undoubtedly be a perfectly effective plan for preventing erosion, but it would be very expensive. For this reason, instead of placing the mats in actual juxtaposition, we lay them at intervals of about half their length and then carefully observe the effect. Should more erosion ensue, the spaces

* See *post*, p. 644.

† Storm-Buysing, Waterbouwkunde, i, 372.

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must be shortened or intermediate mats sunk. From the low-water mark up, in the prolongation of the axis of the mat, the spur proper is built up to high-water mark. A greater height would be expensive and very seldom useful. The direction of the spur should be at right angles to the dike and to the direction of the stream. If the stream and the dike are not parallel, there should be an elbow in the spur. Spurs should not be put in independently, but according to a regular and well-considered system. Concave curves are to be avoided, as affording more points of attack to the current. The ends of the spurs should therefore lie in a right line or a convex curve. (This is not according to our practice.) The upper-bank spurs may be spaced further apart than the sub-aqueous mats, at twice or even four times the distance. It is better to have too few than too many, as the deficiency may easily be supplied if necessary. The cross-section of the spur is not a matter of great consequence. It should be flat rather than steep, as experience shows that in the latter case the shock of the water causes holes of greater or less depth, while the flat spurs become buried in the sand and thus give no trouble in the maintenance. Spurs are usually constructed of clay, protected against the action of the water by a facing of straw-mat,* brush or stone, or simply by brush and broken brick, upon which heavier stone is sometimes placed.

Some banks do not suffer erosion from the water, but, on the contrary, grow by accretion. They are raised gradually to the height of slack tide, when vegetation begins to start. After that time they are covered only by higher and mean floods. Consequently, they grow more slowly, and can never, from the nature of the case, exceed mean flood height. When they reach this level they can generally be reclaimed profitably by dikes. Such reclamation is common at this day in Zeeland, North Brabant, Friesland and Groningen. The process can be greatly expedited by artificial means. These accretions are of much moment to the safety of the dikes, and the higher they are, the more valuable are they on that score. Therefore, every encouragement should be given to such endeavors. In Friesland, they begin the work by placing small screens or hurdles, to turn aside the current, and prevent it from running across the territory to be reclaimed. As soon as the mud is sufficiently dry at ebb, the tract is divided into small lots, some 5 meters broad, by ditches cut perpendicular to the bank, 1.5

* See *post*, p. 593.

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meters wide at top, 0.6 meter deep and 0.3 meter wide at bottom. These ditches are given a length not exceeding 250 to 300 meters, and they are shallower next the bank than inland, so that the muddiest dregs of the water do not run off, but are retained. The accumulated silt is then thrown out into the middle of the lot, and distributed over it. Generally, the first year after this operation, young sprouts begin to show themselves upon the lots, and in two years the ditches are filled with silt. They are then cut out again, and the earth thrown on the land. After a few years two lots are united into one, and a cross-ditch dug for drainage. The same process is then repeated on the side next the sea.

In Zeeland, for a like purpose, so-called silt-catchers ("slik-vangers") are thrown up, being small levees with a height of 0.2 to 0.5 meter above the ground, with a slight slope to the front. They are placed at a distance of 20 to 30 meters apart, and have broad flat slopes of 3 or 4 to 1. They are covered over their whole width with a straw mat, to resist the flow of water. They are joined to one another, near the ends, by a so-called puddle-berm ("plasberm"), which is a layer of brush with the tops turned landward, fastened with three rows of wattled stakes. This puddle-berm holds the silt without confining the ebb water. This process is equally effective with that of the Frieslanders, but is more expensive.

A good foreshore is a great protection to a dike, but it is not always sufficient to reduce the size and force of the waves within such a limit that a good sodding will withstand them. If there be no foreshore, so much the worse; for the violence of the waves increases with the depth. Now, there are many localities where the dikes abut directly upon the sea—the foreshore having been scoured away or beaten away by storms. In these situations the dikes would speedily follow the example of the shore, were not artificial means taken to prevent it. It has already been said that against wave action direct protection is the proper remedy, though hoofden are sometimes powerful auxiliaries. (They seem indeed to be considered essential, in the present practice, in cases of severe exposure.) In the first place, the most destructive part of the storm shocks is directed against that portion of the bank which lies above low water. It is therefore easily got at, and revetment or other suitable device can be applied without difficulty. Secondly, the winds are variable in direction and the hoofden are fixed. They may, there-

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fore, be enfiladed, as it were, and will then be useless. (As now used, they are placed perpendicular to the most destructive winds. For instance, at Westkapelle the northerly winds are the most dreaded. The hoofden, therefore, run nearly east and west. The same is approximately true of the Hondsbosch hoofden and of the Delfland hoofden, which protect the Hoek van Holland and the mouth of the new waterway from Rotterdam to the sea. They extend far enough and are spaced far enough apart to mask one another. They probably serve also another important purpose in modifying destructive littoral currents engendered by the north winds or the tides.)

Various methods of direct protection have been used, and are still actively practiced. In severe cases pilework and stone are the means most depended on; in others, recourse may be had to straw and brush, especially for temporary purposes. Pilework, when employed, as it usually is, for the defense of important dikes, is elaborate and thorough, and exhibits considerable variety in its several types. The principle which seems to have met with the most acceptance is that of a double row of piles, parallel to the trace of the dike, properly braced back, and the interval between the rows filled with brush, broken brick, and finally with large and heavy blocks of stone. The piles are driven as close in the row as possible, and spiked to waling-pieces. The objections to this type seem to be the vertical or nearly vertical position of the piles, which invites the greatest amount of shock and causes the greatest possible undertow, whereby the foot of the pilework is apt to be scoured out, unless there be a considerable rip-rap of stone there—the liability of wood to decay—and, in salt water, the exposure to the attacks of the teredo. The first objection may be obviated by spiking inclined timbers to the piles, or by rip-rap as aforesaid. In Holland wood appears to last very well. It is said that the average lifetime of an oak or fir pile is at least thirty years. As for the teredo, it is only bad on the sea-coast proper, and then only below mean tide-water. It hardly frequents the Zuider Zee at all, and is not found in brackish waters. So pilework is still much in vogue in various parts of the Netherlands. It is generally placed in front of a dike and not on it—so that the slope may be sodded, and the force of the waves so broken that the sod may suffer no detriment.

Stone, however, is the main resource, and in some situations it is the only one—for instance, where the effect of tidal scour is added to the

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force of the winds, and the damage is not alone above water but also below, so that deep water is found at a very short distance from the foot of the dike, and continually encroaches upon it. Such a situation is found at the Helder, the extreme northern point of the main land of North Holland. A strong current flows through the strait between the Helder and the island of Texel, which, with the assistance of storms, has scoured and beaten away the bank until, at a distance of 40 meters from the toe of the dike at flood-tide, a depth of 25 to 28 meters was found in many places, and at 50 meters a depth of nearly 30 meters,* indicating a mean slope of about 1.5 to 1. The only remedy that was thought available was a rip-rapping of heavy stone, combined with hoofden where necessary. The former part of the design was effected by throwing in massive blocks of Norwegian granite, which lodged upon the slope and were renewed as fast as they were displaced by the waves and the currents, combined with their own gravity, until an equilibrium was at length reached.† Nevertheless, for a long time continual repairs were necessary, and careful soundings were made at close intervals, to determine the exact localities where deficiencies existed. It was estimated that 800 or 1 000 tons of stone were used annually for this purpose. The hoofden also seem to be entirely of stone, and if so must have consumed an immense quantity of material. They are not placed along the whole reach, but only in the most exposed spots. They are spaced at variable distances, which the author estimated at an average of about 100 meters, and have a seaward slope of about 40 to 1. They are not all of the same height above the sea level, those most exposed (the western) being the highest, and projecting some 3 feet above high water at the shore end, tapering thence to about high water grade at the seaward end. Going eastward each hoofd is a little lower than the preceding. Above water the stone are regnlarly laid, though in natural blocks, from 1 to 2.5 feet in diameter, and say 1.5 feet thick. They are laid on a foundation of *puin* or broken brick. The dike itself is not of uniform dimensions. The crown is about 30 feet, the back slope about 2.5 to 1, the front slope broken, convex, the upper part about 10 to 1, the lower part generally about 6 to 1, though sometimes the long slope is continued unbroken. (See Plate LV.)

Large and heavy brick, burned specially for the purpose, have been used instead of stone for revetting exposed fronts, but it is believed they

* Storm-Buysing, i, 377. † See Plate LV.

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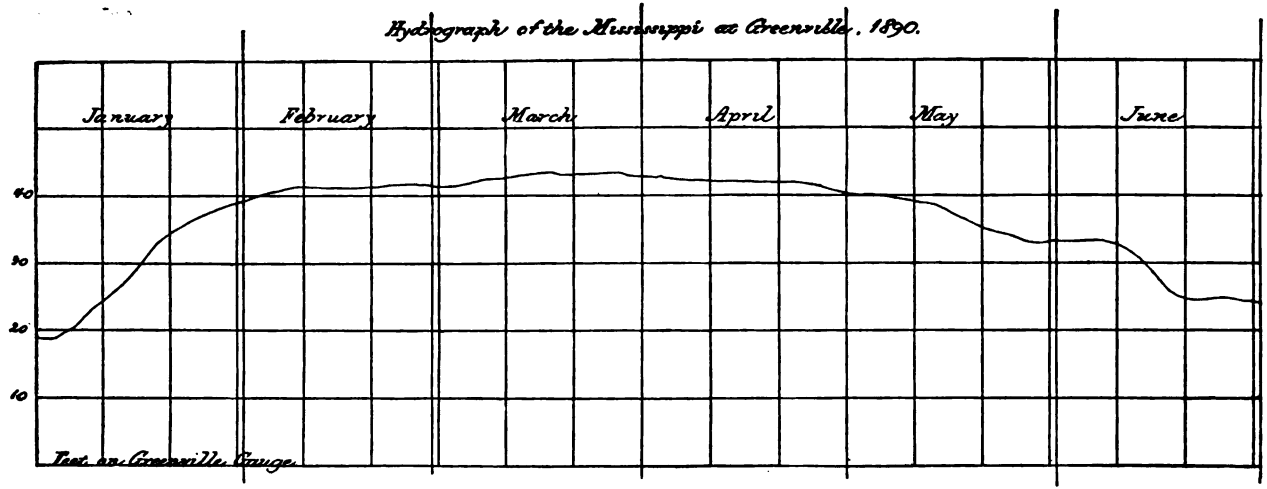
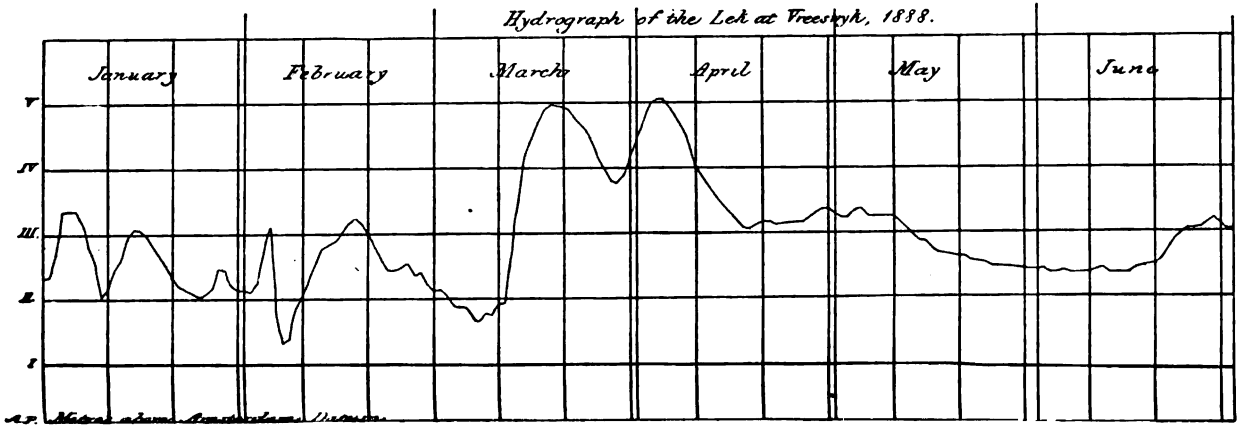
PLATE LIX.
TRANS. AM. SOC. CIV. ENGS.
VOL. XXVI, NO. 534.
STARLING ON HOLLAND DIKES.





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any place, the defect may show itself in a leak or "well" on the inside, or it may remain latent until the last moment, and then result in a slough, a sinking or an absolute break, without warning.

Experience confirms this reasoning. The engineers and riparian dwellers of the Mississippi Valley do not so much dread a sharp and short flood as a prolonged one. As the people say, "the levees get rotten," and the foundations still more so. After several weeks of high water, it is found that in places, and sometimes for miles, the natural surface on the land side of the levee becomes so soft that it will not bear the weight of a horse or of a vehicle, and a sharp rod may be thrust into the ground for 8 or 10 feet or more. Even in the body of the levee such spots sometimes are met with, though they are not now so frequent as they once were. These places are always regarded with alarm by the people, though they are not uniformly or generally precursors of disaster—being formidable or not, according to the head of water against them, the dimensions of the bank and the nature of the soil. In point of fact, disasters have been more common where the ground near the base has been comparatively dry. Nevertheless, the soft spots are always given attention, the remedy applied being usually a banquette, resting on a layer of brush, to prevent sinking and the contamination of the new earth by the leak water.

The cavities developed in a levee by gradual and prolonged scour through sandy material are occasionally met with in the Mississippi levees and are popularly called "cisterns." They are not common, at least so far as discovered, being generally found out only by accident. Disasters of unknown origin have frequently been attributed to this cause, but for the most part on very imperfect evidence of the negative kind, that is, there was no other way of accounting for the break. Very frequently alarming holes break out on the land side, throwing out large quantities of sand—several cubic yards perhaps—which distributes itself in the shape of a regular conical crater. After the first outburst, the discharge of sand generally ceases, or becomes imperceptible, the hole still pouring out considerable volumes of water. The conduit, when sounded, is found to be vertical for a considerable depth. It is evident that the sand which was discharged has left a cavity somewhere, but probably pretty deep under the surface. These holes are always

FIG. 6.

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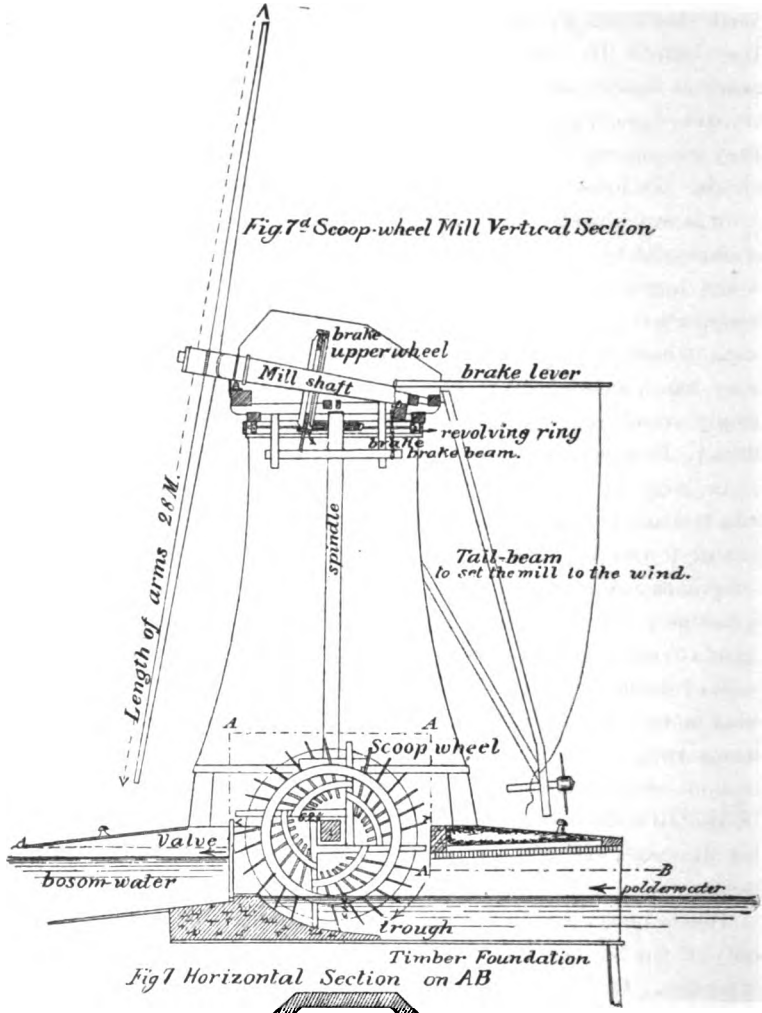

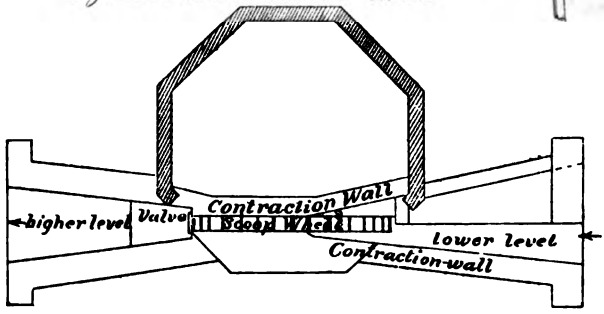

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sixty or seventy.* With steam may be used wheels or screws, as with wind power, and also pumps. Centrifugal pumps especially are coming much into vogue. Nevertheless, nearly all the great draining machines that were seen were wheels. There are wheels at Spaarndam, Halfweg, Katwyk, Amsterdam; centrifugal pumps, it was said, at Schellingwoude; lifting pumps at Haarlem Lake.

The following memoranda are from notes taken on the spot:

The apparatus at Katywk consists of six wheels, each 9 meters in diameter, with twenty-four arms, each carrying a bucket 2.5 meters broad and 2.25 meters radial depth, working in a trough with a self-acting valve opening outward. There are eight large boilers, of which only six are used at a time, the others being held in reserve. There are two gangs of wheels, three in a gang, each set worked by an engine—that is, there are two engines in all. The lift is 1.3 meters with all six wheels, or 2 meters with four wheels. The engine cranks move pinions, making thirty-six revolutions per minute, which work into large cog-wheels keyed to the main shaft, giving the water wheels four revolutions. Capacity, 80 000 to 120 000 cubic meters per hour.

The draining machines for the city water of Amsterdam (that is, the water of the canals, etc.) have eight boilers, four engines, eight wheels, each of sixteen arms; buckets 3 meters wide; capacity, 100 000 cubic meters per hour; lift 1.2 meters with eight wheels, 2 meters with two (?) wheels, which last height, however, is never needed, because the head to be overcome never amounts to so much. There is a sluice by which the city water may be shut off from the North Sea Canal when necessary. Formerly there was direct communication with the Zuider Zee by a sluice. Now, since the construction of the Merwede Canal, resort must be had to other means. The canal itself could not be used as an intermediary, because of the great disturbance of its level that would ensue from the introduction of a vast quantity of drainage water after a rain for instance, or the withdrawal of an equally large quantity to freshen the city water, as is often necessary. Therefore, three great siphons have been sunk under the canal, giving the required communication. The siphons are of iron, square in section, and are supported only at the ends. They can be hoisted for cleaning or other purposes.

The "mill" at Halfweg has six wheels, arms 7 meters long, twenty-four in number, 2 meters broad, four large boilers. The engines make

* Beekman, op. cit., p. 116.

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sixteen revolutions per minute, the wheels four. Capacity, 90 000 cubic meters per hour. Lift with all six wheels 1 meter, with four wheels said to be 1.5. Sluice valves, "tankard" pattern. The Spaarndam works have ten wheels, each with twenty arms; eight of the wheels have buckets 2 meters wide, two have them 2.5 meters. Two engines, four very large boilers, valves small double gates. There was no attendant there, hence no details could be learned as to capacity, etc.

The pumping apparatus of the Haarlem Lake polder is different from any other in the Netherlands, being the same that was originally used to pump out the lake. There are three pumping stations. The machinery of all is nearly similar. Briefly, it consists of a powerful Cornish engine, working several pumps. The machines are named the Leeghwater, Cruquius and Lynden, of which the Leeghwater was the first constructed. There is a full description of it in "Appleton's Dictionary of Mechanics," article "Pump, Leeghwater Steam" (Vol. II, pp. 506–510). It has eleven pumps, each 63 inches in diameter, of which only nine are used at a time.* The Lynden and Cruquius machines have eight pumps of 73 inches in diameter, of which seven are used at once. The extreme lift is 17 feet.

During the months from April to August, when the evaporation exceeds the rainfall, it may be necessary to let the water from the bosom into the polder. This may often be done without special appliances, as, for instance, by opening the outlet valve by a crowbar. Sometimes there are sluices in the outlet valve. Sometimes there are special conduits, running outside of and around the mills, closed by sliding gates, which need only to be raised to admit the outer water. Sometimes there are inlet culverts or pipes, as at Haarlem Lake. If there be not sufficient water in the bosom, then more must be admitted from the outer water. The Rhineland district has three points of inletting, at Gouda, at Leidschendam and at Bodegraven. In 1868 there were admitted at these places 167 000 000 cubic meters. In nine years there were admitted, in the average, 49 000 000 cubic meters annually.†

The dikes of many of the older polders were constructed, not only to keep out the bosom water, but even the sea or river water in the event of a break in the main dikes. Such accidents used to occur quite frequently. While they work great injury to all alluvial lands, they are

* Statement of attendant at the Lynden pump.

† Beekman, "Nederland als Polderland," 117–118.

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especially calamitous to deep polders, or droogmakeryen, as these cannot be drained naturally, but must be pumped out again. It is almost impracticable, however, to give great height to the levees of the South Holland polders, on account of their depth and the weakness of the underground. They are therefore usually built so as to project only 0.5 or 0.6 meter above the bosom water, after allowing for probable settling and shrinkage.*

The cross-sections of the polder levees are very variable, as might be expected. For high-lying polders they are insignificant, still more so for the division levees which sometimes separate large polders into two or more parts. Where the land inclosed, however, lies very deep and is extensive and vlauable, the levees are often larger than the average of river dikes, though not so high. These increased dimensions are partly due to the fact that the polder levees have to stand the pressure of a considerable head of water, not for a few days or a few weeks, but all the year round, without intermission. They are still more due to the poor quality of the material of which the levees are composed, which is mostly, from the nature of the situation, peat ground. Clay, of course, is scarce and valuable and lies very deep. Nevertheless, a good and tight levee may be made of peat. For this purpose it is put up in thin and even layers, thus avoiding excessive shrinkage. Such embankments are subject to the further danger of sloughing and sinking into the underground, accompanied by a rising or "bulging" of the bottom of the ring canal. This danger is met, as far as possible, by giving a great base to the embankment and leaving wide berms. Sometimes a *kleikist* or muck ditch is used, 2 to 2.5 meters wide, and extending, if possible, in depth into the clay stratum, or, if this cannot well be reached, at least 2.5 or 3 meters deep. The ditch is filled with good clay, well tamped, and brought to a height of 0.2 to 0.3 above the ground. Plate LXIX shows a typical section from Beekman, of a polder levee, with its ditch on the inside and its ring canal on the outside, used for the purpose of navigation, as is common. The flat slope is placed on the inside, as it would not be suitable for the side wall of a canal. The second figure shows the apparatus for draining. The Haarlem Lake levee is even stronger than the type represented. It appeared to have a land slope of about 10 to 1, with an immensely broad crown and a canal about 150 feet wide. Large vessels were upon it.

* Storm-Buysing, Waterbouwkunde, ii, 378.

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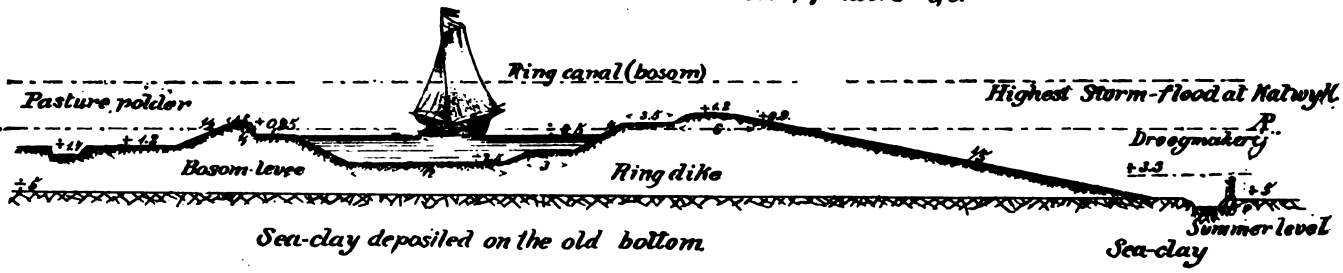
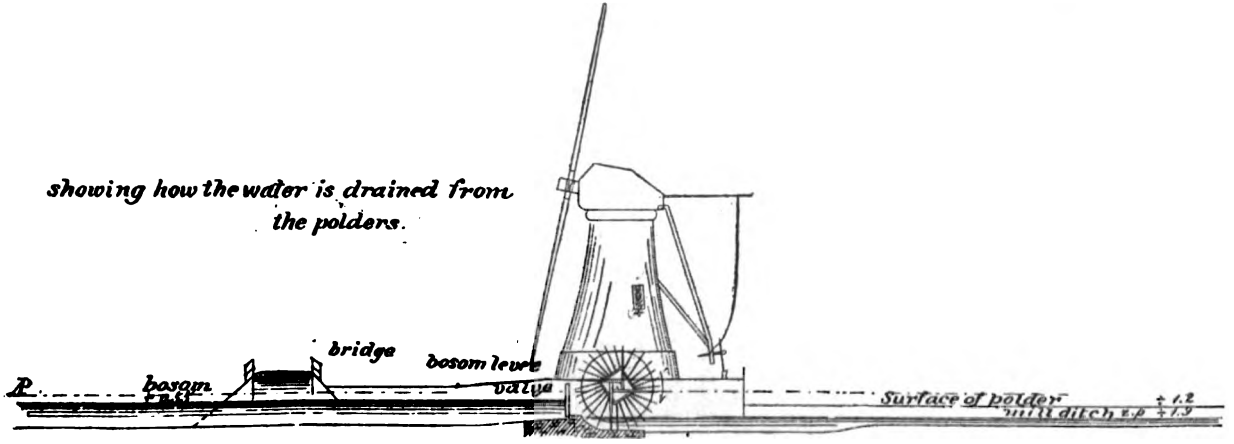
Section of polders &c.



showing how the water is drained from the polders.



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The reclamation of Haarlem Lake was so remarkable an enterprise that it deserves a short notice. It is interesting to engineers in several particulars. Accurate records of its history are in existence and are preserved in print.* It appears, then, that in 1531 four lakes occupied the space which was afterward covered by Haarlem Lake. Their collective area was 21 530 acres. They lay in the low fen lands, and every storm cut away a part of their shores, until in 1591 they formed a single sheet of 26 390 acres, and had swallowed up the village of Vyfhuizen. In 1647 the area had increased to 36 110 acres, and two more villages had disappeared down the jaws of the "water wolf," as the people termed it. In 1740 the lake possessed a surface of 41 510 acres, in 1808 of 44 440 acres, and at the time of the reclamation, 1849, 45 500 acres. The loose turf displaced by the storms was either burnt for fuel or carried to the sea through the sluices at Spaarndam. Costly works were undertaken for the protection of the banks, but the district was not equal to the task. The State finally contributed to the undertaking, and such was felt to be the magnitude of the emergency that even distant Utrecht lent its aid. The sum thus appropriated was about $720 000. So great was the rejoicing that the town of Aalsmeer appointed an annual day of thanksgiving, which was kept till 1795.

The destruction of the banks, however, was not the only evil to be combated. By the effect of the wind on this large and continually increasing body of water, the surface on one side was frequently raised during storms to a dangerous extent. As early as 1639, Leeghwater, already mentioned, had demonstrated the practicability of draining the lake, and his work had gone through many editions. Other plans were suggested; but it was not until the present century, when the introduction of steam as a motor opened new possibilities, that the project began to be seriously canvassed, and it was finally given a new impulse by the storm of 1836, previously alluded to. A law was at length passed in 1839, opening a loan of eight million florins, and appointing a commission of management. First, however, provision must be made for the great diminution of reservoir surface which was about to take place. The lake was actually 85 per cent. of the whole bosom of the Rhineland. To compensate for this loss, the sluices at Katwyk were enlarged and new facilities provided for conducting water to them. Steam mills were erected at Spaarndam, Halfweg and Gouda. There was much difficulty in build-

* Beekman, De Strijd om het Bestaan, pp. 241–249.

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ing the ring levee on the weak and treacherous, sometimes "half floating," soil, as it is described. After completion, the levee shrank and settled till it reached 4 feet below its original grade. It is about 17 miles in circumference. The pumps have been adverted to. When they were built they were among the wonders of the world, and it is doubtful whether they have been surpassed since. They worked more than three years (being stopped 210 days, on account of the bosom being too full). The quantity of water evacuated was more than 800 000 000 cubic meters, or say tons.*

The total cost was 13 920 000 florins, or say $5 500 000, of which nearly one-third went for interest; 42 200 acres were sold to individuals for $3 142 800, or nearly $80 per acre. At present, the polder does not present a very flourishing appearance. The soil after it was drained was found to be poor and sandy in places, the layer of clay being there thin or missing. It is laid out in straight lines and right angles, with the eternal double rows of trees along the roads. It seems there has been much difficulty in this extensive polder, in keeping the water to a level that is satisfactory to all—some of the lands lying much higher than others. This has caused a part of the polder to be separated from the rest by a subordinate levee.

The process of draining into the sea or the rivers in the natural way is performed through the medium of sluices or locks. Where the passage is used for purposes of navigation, the latter must of course be employed. Where this necessity does not exist, a simpler form of sluice is used, called by the Dutch an "outwatering sluice." These devices are simple in principle, consisting merely of a masonry culvert, usually brick, with folding or sliding gates; but however simple, they demand much care and skill in construction, owing to the fact that they are placed in earthen dams with a considerable head against them, and on earthen foundations, and sometimes with earth of the worst quality in each situation.

Culverts even of the ordinary dimensions are almost always placed on piles and with the same precautions that are used with lock floors. Piles are the grand resource in Holland and this the more that they have a very long life in that soil. In Belgium it is said that in many situations they decay very rapidly.† Piles in small works are usually driven

* A cubic meter weighs about 2 205 pounds.

† Gaudard on Foundations, p. 12 (Van Nostrand's Science Series).

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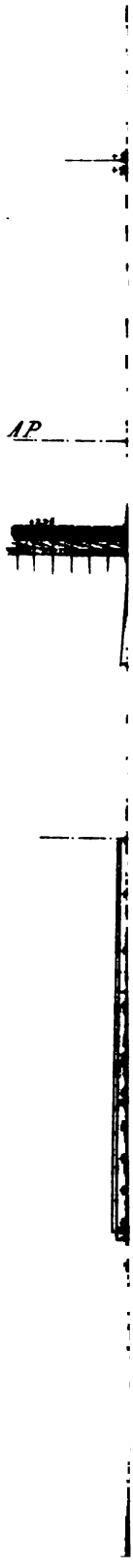

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by a hand engine, with a hammer varying in weight according to the number of men employed. Ordinarily the proportion is 15 kilograms or say 33 pounds per man. Generally about ten men work at once. The frame is a three-stick sheers, of which two sticks lie in a vertical plane. There is a tie across the two at the top, from which are suspended two sheaves, one for the pile and one for the hammer. To the tie are fixed two rings through which pass the leads—long, round, slender poles, perhaps 50 or 60 feet in length. When the leads are to be moved, they are worked by one of the sheaves as if they were piles. The same arrangement was seen used with steam power with a hammer of 450 kilos.

In laying the floors of locks or culverts, the caps are secured to the piles by mortises and tenons, the tenons being wedged so as to make a dovetail joint.* The stringers are placed and secured in the ordinary way, the caps being let into them about 2 inches. The best dry clay is then filled in between the caps and thoroughly tamped, after which the lower floor or pine floor of 3 or 4-inch plank is laid with all possible care, calked and pitched. Previously to this four or more rows of "dam plank" have been driven, being sheet piles of pine or fir, 3 to 4 inches thick, joined to one another by tongues and grooves and extending into the ground from 10 to 16 feet, their mission being of course to cut off leakage under the floor. In some important works, as in the Vreeswyk locks of the new Merwede Canal, the spaces between adjacent rows of dam plank are filled with concrete—see Plate LXX. Over the pine floor is placed a third tier of timbers or cross-pieces, perpendicular to the stringers and parallel to and exactly over the caps, projecting 1.5 or 2 feet at each end and built into the side walls. They are fastened as strongly as possible through the floor to the caps, so as to form, as nearly as may be, one whole with them. If the floor of the lock is to be built of masonry, the woodwork is completed with the cross-pieces. Otherwise the spaces between the cross-pieces are built up with carefully laid masonry, consisting of hard burned brick (*klinkers*) laid in cement and covered with a layer of the same. On this is placed the upper or oaken floor, calked and pitched.

The particular duty of the tier of cross-pieces is to protect the floor of the lock or culvert from the upward pressure of the water. Even after every precaution is taken to prevent infiltration, by dam planks

* Algemeene Voorschriften, § 200.

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and the like, it will often happen that there is a very dangerous quantity of leak water in the soil, which exerts a hydrostatic (or rather a hydraulic) pressure on the floor. This is the same influence that has been alluded to as forming one of the principal elements of danger to dikes built on bad soil, and it is the more threatening to locks or culverts on account of the comparatively narrow base which economy prescribes for such structures, whereby the pressure of the external water is not much diminished by friction. Accordingly the Dutch engineers,* in calculating the amount of the *oppersing* as they call it (up-pressing or upward thrust), assume that the pressure is purely static and equal to that theoretically due to the head. The resistance opposed to this force is composed of the strength of the cross-pieces and caps, held down as they are by the weight of the side walls and the frictional resistance of the piles to being drawn out of the ground (the floor being dovetailed to the piles by means of the caps). Particular stress is laid upon this by the Dutch, and attention is specially called to it because, it is believed, some American engineers are disposed to underrate the effect of upward thrust from soil water. It will no doubt appear that the estimate of its power by the Dutch engineers is excessive; but it must be remembered that their perhaps over-great precautions are not entirely due to theoretical considerations, but are no doubt partly the result of the experience of repeated failures. Even now, it is said, there is a bad leak in the floor of one of the great locks at Ymuiden, which has not been stopped. In place of the upper tier of cross-pieces, if the floor is to be of masonry, it may be built in the shape of an invert, but the piles, caps, stringers and pine floor are usually retained. Mr. Kemper informed the author that a departure was made from this method in the new locks now under construction at Ymuiden. They are founded on concrete alone—no piles.

Two principal types of outwatering sluice are described as being in use. In one, the dike is cut through nearly to the crown on each side, and vertical front and back walls of masonry with wing walls of the same provided. In this case, the culvert proper is only about the crown width. In the other type, the dike is cut down only partly up the slopes, making a longer culvert, but lower front walls and shorter wings. The latter is the type of which the most instances were seen. It would seem to be far preferable in bad soil and is the prevalent pattern in Zeeland.

* Storm-Buysing, Waterbouwkunde, ii, 231-237.

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Gates are either sliding, in grooves, raised and lowered by a rack and pinion or screw, or they are lock gates, arranged in pairs. They may be at the end or in the middle. In the latter case, they must be made so as to act automatically, and close by the mere pressure of the water. In most sluices there are also one or more sets of grooves to receive stop planks, if necessary, and windlasses are provided, in the larger sluices, for raising and lowering them. Stop planks are of very general use in the sluices and locks of the Netherlands. They are frequently placed in pairs, and the space between them filled with well-tamped clay. The larger outwatering sluices are elaborate structures, and indeed some of them are monuments of excellent engineering. The largest and most celebrated of them all is the great sluice or aggregation of sluices at Katwyk, to which allusion has frequently been made. This massive work, though built so long ago (in 1807), is said to be the finest of the kind in Europe, and the engineer, Conrad, has a monument in the Cathedral at Haarlem.

The Katwyk sluice belongs to the category of "flushing sluices" (*spuisluizen* as the Dutch call them), in which the inner water is allowed to accumulate to a considerable head, and is then discharged suddenly for the purpose of sweeping away aggregations of silt. In case the level of the inner water is not sufficient to accomplish this object, the outer water is admitted, at time of flood, into a large chamber, which is then opened near ebb. At Katwyk, the chamber is the space included between the outer and innermost sluices (there are three altogether), of 1 500 meters in length. See the plan, Fig. 7.* There are five open-

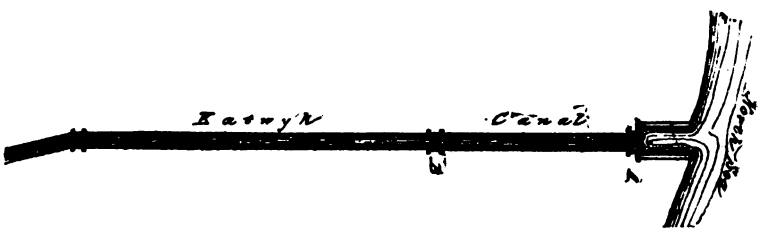

FIG. 7.

ings in the outer sluice, each closed by a sliding gate, raised and lowered by a winch and chain, the latter passing over a sheave, with a counterpoise. Besides this, there are two grooves at each end of the sluice to receive stop planks for repairs or emergencies. The outer water is

* From Storm-Buysing.

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admitted only through the middle opening, by a special appliance, designed to keep out the greater portion of the silt suspended in the sea water. For this purpose the sliding gate of the middle opening is divided horizontally into two, the upper one alone being raised for the admission of the outer water. When the chamber is high enough, the gate is closed and the water retained until the tide has run out sufficiently, when all the sluices are opened. They usually remain open five or six hours. The head under which they work is great enough to sweep away the masses of sand thrown up by the sea. Of course an elaborate spillway is required. The second or middle sluice, which lies 500 meters from the outer, is sufficient to keep out the greatest external flood, and is therefore an additional security against accident from such a source. It was originally intended also as a flushing sluice in case the canal itself became silted up within the outer sluice. It was found, however, that fears on this account were groundless, and the flushing gates were removed.* When Haarlem Lake was reclaimed, it was found necessary to enlarge the inner sluices; and in 1880 the powerful steam pumping plant previously mentioned was built.

It has already been said that the tops of the dikes, in almost all instances, are either paved or graveled, and used as roadways. If they are paved, it is always with brick, of the kind which the Dutch call *klinkers*, from the clear ring which they emit when knocked together. These are very hard burned brick, of medium or small size, obtained mostly from two sources, the Yssel of Holland and the Waal. The former are very small, yellow in color, and very hard. The material for them is gotten from the muddy bed of the Yssel, the deposits of which are admirably adapted for this purpose.† The chief depots for them are Gouda and Rotterdam. The Waal brick are considerably larger, nearly the size of one of our American bricks, and of a dark red color. They are more generally esteemed for road making. Yssel brick, however, are occasionally made Waal-size, as they term it. They are laid on a sand bed, by preference not less than 16 inches thick, on edge, with the longest side perpendicular to the axis of the dike. It was observed by a very intelligent engineer‡ that brick do not make a satisfactory road for heavy hauling, as they will not stand such hard use for more than two

* Storm-Buysing, ii, 210.
† Baedeker.
‡ Mr. Wisboom.

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or three years, but they are excellent for ordinary traffic. The author saw some brick roads which were said to have been built in the time of Napoleon. In Zeeland they prefer them to gravel, as the soil in these islands is soft and clayey, and the gravel is apt to sink.

The gravel roads, however, predominate by far, and constitute, perhaps, nine-tenths of the present highways. It may be remarked that good roads have not always been the rule in Holland. On the contrary, they are of comparatively recent origin, dating no further back than the era of Napoleon. Under his régime a great national road was built through Holland and Belgium and via Amiens to Paris, and in the reign of his brother, King Louis, the impulse thus given was vigorously followed. In fact, the Dutch people hail the memory of King Louis as the author of the present excellent system, under which the whole kingdom is traversed by admirable artificial ways. It is not to be supposed that the making of these roads is easy or cheap. The material does not lie close at hand, but must be transported from a considerable distance and handled repeatedly. It is true that water transportation is cheap and labor sufficient, but Holland has no great advantages in these regards over other alluvial regions. In other words, what the Dutch have done, the people of the Mississippi Valley may do, or any others who have the requisite energy and population. The gravel for the Dutch roads comes from three sources*—by dredging, from the upper Waal, above Tiel ; from the Maas, and from the hills of Limburg and elsewhere in the region of the gravel diluvium. The last is preferred (as similar qualities are also in America, for railroad ballast and for streets) from the fact that there is a matrix of earth surrounding and cementing the pebbles, and serving the same purpose of giving hardness, firmness and tenacity when they are put into a road. The gravel is run through one or more screens and the different sizes separated, the larger being used for the lower layer and the smaller for the upper.

The General Regulations† prescribe that first a suitable bed shall be prepared, of the proper consistency and shape, with a convex or cylindrical upper surface. On this shall be placed a layer of refuse brick, laid flat and regular, the joints filled with sand or mortar refuse. Next comes a layer of broken brick and then the gravel. According to par-

* Mr. Leemans.

† Algemeene Voorschriften, §§ 296-302.

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ticular specifications,* the gravel is to be placed in three layers, altogether 6 inches thick. Each layer is sprinkled with a layer of clay 2 centimeters thick, or say 0.8 inch, well rammed and rolled. As to this particular, Mr. Wisboom stated that there was a difference of opinion among engineers, some maintaining that the admixture of clay was detrimental, and that gravel alone was preferable.

A subject of great interest to river engineers is the experience of the Dutch with outlets. It is known that this system had a faithful trial on the large rivers of Holland, and the author made inquiries of all the engineers he met as to the results of the experiment. It was found that at one time it had been the fashion, and that some remains of it were still in existence. The branch of the Rhine called the "Gueldres Yssel" gives a somewhat shorter route to the sea level than the Lek or the Waal, and the lands which lie along its course are high, not subject to serious injury by inundation, and capable of being drained, in case of such a misfortune, by natural means. The Yssel was not originally, as it appears, an arm of the Rhine at all, but was a separate river, of which the upper part still exists under the name of the Old Yssel. The connection with the Rhine arose either from the cutting of a canal in the Roman times by Drusus, or by the enlargement by the latter of a channel which had already begun to cut itself out in time of overflow.† The Yssel, at its mean stage, carries off one-ninth of the whole discharge of the Rhine, and in time of flood a still larger proportion. It was found that this gave a great relief to the dangerous and much threatened Lek country, and it was very natural to wish to make this relief still greater and more certain by providing means whereby a still more considerable part of the high water discharge could be transferred to the shorter and less dangerous stream.

In Holland, as in America, after every serious disaster, or even a season of great alarm and expense, a variety of counsels have been offered for the prevention of future damages, and the drawbacks of the dike system are made the subject of vigorous attack. These drawbacks are the expense, always increasing as the dikes are made higher and heavier, and the ever-present possibility of breaks, with their attendant consequences of loss of property or even of life, which also become more formidable as the dikes are given a greater height. To these real dis-

* Specifications for the graveling of the Heerewaarden Dikes, August, 1888.

† Beekman, "Netherland als Polderland," p. 16.

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advantages are generally added others which have no substantial foundation. The truth is, that communities, usually of limited extent and not superabundant resources, become appalled at the prospect of the millions to be spent in the attainment of a perfect dike system, which, like a mountain peak, seemed at the beginning to be easily within reach, but, after a long and toilsome progress, is still far off. Hence there is a continual temptation to try other methods, by way of experiment at least, and some of them look plausible enough to command the attention of intelligent people, and even of some among the profession of engineers. In the earlier part of the present century there appears to have prevailed in Holland a feeling of profound discouragement. The formidable danger of ice gorges had been present with unusually great frequency, and this seemed a peril against which even increased height and strength of dikes could not assure immunity. It was held, even by the majority of engineers, that "the dikes had corrupted the state of the land," and that, at least, the system had been resorted to prematurely, and before the country was prepared for it and in the proper condition; that is, it may be supposed, before the flood plain had attained its proper development, and the banks had been built up to a sufficient height.* A commission of engineers was appointed in 1821, and in 1827 their report was published, recommending, among other things, an extensive series of outlets. The programme encountered strong opposition, and was never carried out further than to widen some of the existing outlets on the Maas. There were already several of these weirs, and they were allowed to remain until a very late date.

The principal outlet was about the upper mouth or point of departure of the Yssel of Gelderland, already mentioned as affording unusual facilities for the trial of such a scheme. In 1809 an *overlaat* was put in at this point, about 2 miles long. As the Yssel itself at this place was not considered large enough to carry the additional discharge, other overlaten were made in its banks, to allow the overflow water either to return to the stream lower down, at some more favorable point, or to seek another exit into the Zuider Zee. This series of weirs has now been suppressed, and the only remnant of the system is one of those which formerly existed on the Maas—namely, the Beers overlaat, formerly alluded to, which deserves a notice in connection with the interesting work for giving a new mouth to the River Maas.

* Storm-Buysing, II, 56.

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It has already been said that the Maas is not diked for its whole length. A few years ago there were three openings in the line—two on the southern bank and one on the northern. The former were called respectively the Beers and Bokhoven overlaten, the latter the Heerewaarden overlaat. From the two former the Maas water flowed over its banks and inundated its own valley. The last named is situated at the point where the Waal and the Maas approach one another so closely, and as the Waal is equally undiked here, it allows the water of the greater river to overflow into the less. Therefore, so far as the Maas is concerned, the Heerewaarden overlaat is not an outlet at all, but rather an inlet.*

Of the weirs on the southern bank, the higher is situated near the very beginning of the dike system, about 1 mile below the point where the Maas is crossed by the railway from Venlo to Nymegen, just above the town of Grave, and near the village of Beers. Here is a gap in the dike about a mile and a half in length. This is not left entirely open, but is partially closed by a "summer levee," of which the greater part, however, is not permanent, but must be removed during the winter, to be restored after the flood. The remaining portion is constructed for the special purpose of allowing the water to run over it. Collectively, the gap is called the Beersche overlaat, and the water which pours over it in time of flood is called the Beersche Maas.

The term overlaat is used to designate any kind of outlet or waste weir. It is rendered into French by the word *déversoir*. It is applied to several very dissimilar constructions, not only to open or undiked lands, but to sluices in the main dikes, designed, in case of necessity, to let out inundation water, and to low stretches in the same dikes, topped or surmounted by a small and easily removed supplementary levee, of which we have seen several examples. The author did not see the Beers outlet, but he saw the one at Heerewaarden, which is said to be very similar. It consisted partly of merely low and flat dike, and partly of *stortebed* or spillway. The latter was carefully faced with basalt blocks, with a moderate outside (Waal-side) slope, 2.5 to 1, and a long slope on the Maas side, 10 to 1. The crown was given a rounded form. It was 2 meters broad.† The specifications for these works‡ provide that the

* An overflow of the Maas into the Waal, of any consequence, has never occurred.

† See Plate LXIV.

‡ Waterstaat. Beteugeling der Heerewaardensche Overlaten. Dienst 1888. Bestek en Voorwaarden, etc. No. 188. Aug. 29, 1888.

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embankments shall be built entirely of clay, the borrow-pits to be distant not less than 50 meters on the Maas side, and covered with a straw mat, brick rubble and basalt in succession. The Bokhoven overlaat is merely undiked land. The Baardwyk overlaat, of which hereafter, is protected by what has been designated as *rysbeslag*, that is, by a brush dressing covered by a layer of rubble, stone or brick.

At a stage of about 4.5 feet below the grade of the Maas dikes, the water begins to pour over the Beers outlet, or, as the technical phrase is, "the outlet works." It fills the low space between the southern Maas dike and the heath lands of North Brabant (a distance of 2 or 3 miles), and soon reaches the important city of 's Hertogenbosch, or Bois-le-Duc, the capital of the province, where it encounters the small river Dieze, running nearly perpendicular to the course of the overflow water, and diked on both sides. There are two sluices and some low places (overlaten again) in the eastern Dieze dike, and through them the inundation water finds its exit into the Maas again at the mouth of the Dieze at Crèvecœur. A portion, however, of the overflow may pass to the south of Bois-le-Duc (depending on the height of the flood, etc.). Now, from Crèvecœur to Bokhoven, half a mile or so (800 meters), the Maas has no dikes. Through this gap the excess of overflow water may enter the Maas, provided the latter be low enough. If the Maas be at all high, this return flow is not possible. On the contrary, the very reverse operation may take place, and the Bokhoven overlaat may aggravate the overflow. This is an incident of frequent occurrence, especially from high water in the Waal, which river, as has been seen, at high stages, has had, until recently, direct communication with the Maas over the Heerewaarden overlaat, and has still, at the mouth of the Maas at Woudrichem, only 15 miles below Bokhoven.

Now, the Waal is a much larger river than the Maas. In fact, it is three times as great. The communication between the two streams, then, is entirely to the disadvantage of the Maas, that is, the latter never seriously affects the Waal, while the Waal may produce formidable floods in the smaller river. Both by direct flood, then, at Heerewaarden and by back water from Woudrichem, the Maas may be so raised at Bokhoven as to pour a considerable volume over the bank. This finds its way rather deviously between diked lands through the bottom called the Langestraat, corresponding, in the lower part of its course, to the valley of the Old Maasje (little

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Maas), an old, abandoned and deteriorated bed of the Maas, to the Hollandsch Diep, and thence to the sea at Hellevoetsluis. The combined volume of the overflow may be so considerable as to threaten the dikes which border the zone of inundation. Breaks not unfrequently occur and produce disastrous overflows. Mr. Wisboom pointed out the locality of a crevasse which occurred in 1880, near Nieuwkuyck. The entrance or sill of the Langestraat forms a narrow pass which is partially closed by the Baardwyk overlaat above alluded to. This outlet is large enough to give passage to the whole volume of the overflow which enters via Bokhoven.* It is a kilometer wide, say three-fifths of a mile.

By this system it will appear that a portion of the flood water of the Maas is diverted from the main stream and allowed to flow by a side channel until it either regains the parent river or enters the sea, according to circumstances. The overflowed country is mostly not directly inhabited, that is, there are no houses or dwellings in it, except in the City of Bois-le-Duc, which is surrounded by the inundation water, but raised above it.† It is not abandoned either, but turned into grazing land for the most part, though occasionally grain and other crops are planted, when the overflow goes down in time, that is, by April. This it would almost always do, did the inundation proceed from the Maas alone. But the Waal sometimes is in flood in May and June, and thus the relief of the polders of the Maas is delayed to an extent which may be fatal. Otherwise the overflow is not regarded as a calamity. The ground between the Maas and the uplands is not of the best quality, and needs the fertilizing influence of the floods. Nevertheless, the polder lands about Heusden, which are inclosed by dikes, are worth twice as much as those included in the overflowed tract, the former being valued at $800 per acre. The overlaten themselves are closed, as soon as their function is ended, every year, by summer levees, which, however, must be demolished before winter.

The evils of this situation are numerous. First, as to the Waal itself. The flow through the overlaten causes an abrupt change of direction of the current and a loss of volume, hence diminishes the velocity of the

* Notice sur les Travaux de Séparation de la Meuse et du Wahal. (Par C. Schnebbelie, Ingénieur-en-Chef du Waterstaat.) Bois-le-Duc, 1889. An abridgment of this paper is contained in the volume entitled "Les Voies de Navigation dans le Royaume des Pays-Bas," of which mention has been made.

† Mr. Wisboom says that he has skated all around the city.

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main stream, brings about deposition of silt, a rise of the bottom and a shallowing of the water, and therefore promotes the formation of ice gorges in the Waal, the evil of all most dreaded in time of flood, as endangering the dikes above it. For the Maas it is equally disastrous. The subtraction of one-sixth of the high-water discharge of the Waal through the Heerewaarden outlet would add one-half to the contents of the Maas. Even though the Waal be not high enough to discharge any considerable quantity over its banks, yet it can "back up" from Woudrichem, and thus prevent the outflow of the Maas water, facilitate ice gorges, prevent their dissolution, and obstruct the drainage of the low lands. Hence the bold project of separating the two rivers entirely, and giving the Maas a new mouth. This scheme involves the closing of the Heerewaarden overlaat, the damming of the Maas just above its junction with the Waal, and the construction of a new bed, some 21 miles in length (inclusive of the channel called the Amer, which, with improvements, will form a part of the new Maas),* and large enough to pass the high-water discharge of the river. The overlaten at Beers and Bokhoven are left to be closed by the local authorities, should they choose to do so. The affair is not regarded as of national importance. It is Mr. Schnebbelie's opinion that they will be closed. The law authorizing the execution of the project was passed in 1882. The necessary act for the expropriation of lands was passed in 1885, and the work was begun in 1886.

The closing of the Heerewaarden outlet was not designed to take place suddenly, but by degrees, so that the rivers might have time to accommodate themselves to their altered regimen. Consequently, the work there has consisted of successive raisings and enlargements of the dikes and overlaten, which are still in progress. The canal of Sint Andries, near Heerewaarden, unites the two rivers for navigable purposes, and the raising of the locks is a part of the project. This is being done to full grade, but the last enlargement of the embankments brings them, the author thinks, only within 0.8 meter of that standard. There are to be two dams across the Maas—one at Neer-Andel, about 2 miles above its confluence with the Waal at Woudrichem, the other at Slyk-Well, just below the upper mouth of the new river, and forming a prolongation of the dike which constitutes its northern bank. Both are connected with

* The part which is to be actually constructed out and out, is about 13 miles. The bed of the old Maasje will also be used for a short distance, but it will have to be tripled in volume.

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the systems of dikes on either side of the river. There will be sluices in the dams for the benefit of navigation.

The project for the dams, as shown in the figure (Plate LXXI), which is from a tracing kindly given the author by Mr. Schnebbelie, consists simply of an earthen embankment, which will be constructed after closing the river by a low dam of fascine work (zinkstukken). Zinkstukken (sink pieces) are fabrics of fine brush, very much after the fashion of the "mattresses" so familiar to engineers on the Mississippi, but much smaller, of much finer material and made more compactly and with more care. They are of extensive use in river-works, and probably furnished the model for our own mattresses, with such modifications as adapted them to American use. The brush is the same that is ordinarily seen in the market in Holland, as it has already been described. To make a zinkstuk a grillage of fascine work is first laid, upon which two or more layers of brush are placed, and then the work is completed by another grillage, laid exactly over the first and drawn tightly to it. Upon the mattress thus finished a series of square pens is built out of brush, to hold the stone or other ballast which is to sink the mat.* The grillages are each composed of long fascines or saucissons, made, or, as they express it, spun, continuously and without joints to the required length, which may be 50 meters or more. They are required to be built of Holland brush, with the tops on, and must be 5 inches in diameter. Unless otherwise ordered, they are to be three feet apart from center to center. The bottom layer is to be placed in the direction in which the greatest strain is expected, whether this be lengthwise or transverse. Upon this the second layer is placed crosswise, the two layers being firmly bound together by withes and tarred rope, the latter being used at the ends and at every alternate joint, and having long projecting ends secured to upright temporary stakes. Upon the grillage is laid the brush in two or three layers, as the case may be, each layer being placed crosswise to the preceding. The thickness is 12 inches in the one event and 17 in the other. The brush is laid in bundles, as delivered, close and even. The top grillage is now placed, one layer at a time, as a cap to the work, the crossings of the two layers being vertically over those of the bottom grillage, as indicated by the temporary stakes above mentioned. The two grillages are then firmly drawn one to the other by the projecting ropes, which have been detached from the stakes, and the latter withdrawn.

* Algemeene Voorschriften, §§ 99–144.

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The mattress thus constructed is prepared for the reception of the ballast by driving strong stakes, about 4 feet long, into it, and interweaving about them flexible twigs, forming a continuous low wattling in each direction, that is, lengthwise and crosswise. Thus will be presented a network of pens, which should be about 6 feet square each. In these is placed sufficient ballast (stone or brick) to give bare flotation to the mat. The latter is then sunk at once by rapidly and systematically throwing on more ballast. The last details of the process are almost identical with those pursued on the Mississippi, but are much simpler and easier on account of the small size of the mats and the slower current. The widest mat of the Neer-Andel dam (that at Slyk-Well is precisely similar) is 40 meters wide, the others being from 12.5 to 7. The thickness seems to be about 0.75 meter, or say 30 inches. The mattresses of the Mississippi work are frequently 100 meters or more wide and 300 meters long, and are sometimes sunk in a current of 2 meters per second. The fascine work will be built only to the height of mean tide in the Waal. Behind this, on the Maas side, the earthen dam will then be constructed.

The building of these enormous embankments in the bed of active or past streams may be supposed to be often attended with great difficulty, and such is indeed the case when the bed is composed of or underlaid by soft and compressible material. The trouble is comparatively small when the embankment is merely for a highway or railway. Then resort may be had to fascine-work or similar devices. But when the earthwork is intended to withstand the pressure of a considerable head of water, such means are frequently or generally of doubtful utility, or are even positively dangerous, as tending to permit destructive leakage under the bank. Under these circumstances it has been a puzzle to levee engineers what was the safest and most economical course to pursue, and most of them have fallen back on the simple rule of earth and more earth. The Dutch engineers have had their full share of these troubles and they seem to have come to very much the same conclusion. Of course, the principal difficulty with soft soils is that the embankment keeps sinking and displacing the ground which serves as its foundation, the latter in the meantime forcing up the surface of the surrounding land, and this sinking may be of indefinite and unascertainable extent. It is true that there are formulas by which we may calculate it to a nicety if we only know the angle of repose of the soil. That, however,

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is generally the very thing we do not know. Experiments made to ascertain it during the period of construction may be very misleading—as the soil may then be in a very different condition from that which it will assume at the time of danger. Practically, we find that an embankment may continue to sink until it has very nearly displaced its own weight before it comes to an equilibrium, and that on attempting to complete the work by adding more earth the process is likely to recommence. Furthermore, this will very possibly take place at the most inconvenient time, that is, at the time of flood, when the water stands high against the dike or dam and the soil is thoroughly permeated and softened. Consequently, various devices have been tried in the way of piles, grillages, fascines and the like, with the result that they have partially stopped the settling at great expense and sometimes at the risk of losing the dike.

Mr. Kemper, one of the engineers of the new Amsterdam-Merwede Canal, exhibited a number of very interesting profiles and sections, exhibiting the displacements that had taken place at various points of his line—sometimes amounting to a very considerable fraction of the whole contents of the bank. Mr. Wisboom, previously mentioned, assured the author that he had known instances where it had equaled twice the contents of the embankment—in other words, the work had to be built thrice before it reached its final equilibrium. Mr. Kemper said that in building some of the locks about Amsterdam, they had to throw in sand in vast quantities to displace the mud and peat, and then drive their piles into the sand. At Vreeswyk, on the Lek, in one instance, they found it cheaper and more satisfactory to dig away the peat altogether and replace it with sand. They consider river sand to be quite good soil, at least in comparison with peat. Mr. Kemper kindly had made a copy of some of the borings at Amsterdam (as well as those at Vreeswyk, previously given). They appear on the next page. *Derrie* is very similar to peat, but found in a little different situation.

In the earthen dams lately built, the practice then has been, so far as could be learned, to rely almost entirely on earth, which is usually sand. The old books recommend *zinkstukken* or other foundations, and they are still used to some extent in desperate cases. The great dam across the Y at Schellingwoude has a single layer of *zinkstukken* under its center, and several under the foot of each slope. They are best adapted to such cases as this, where the bottom is very soft; where

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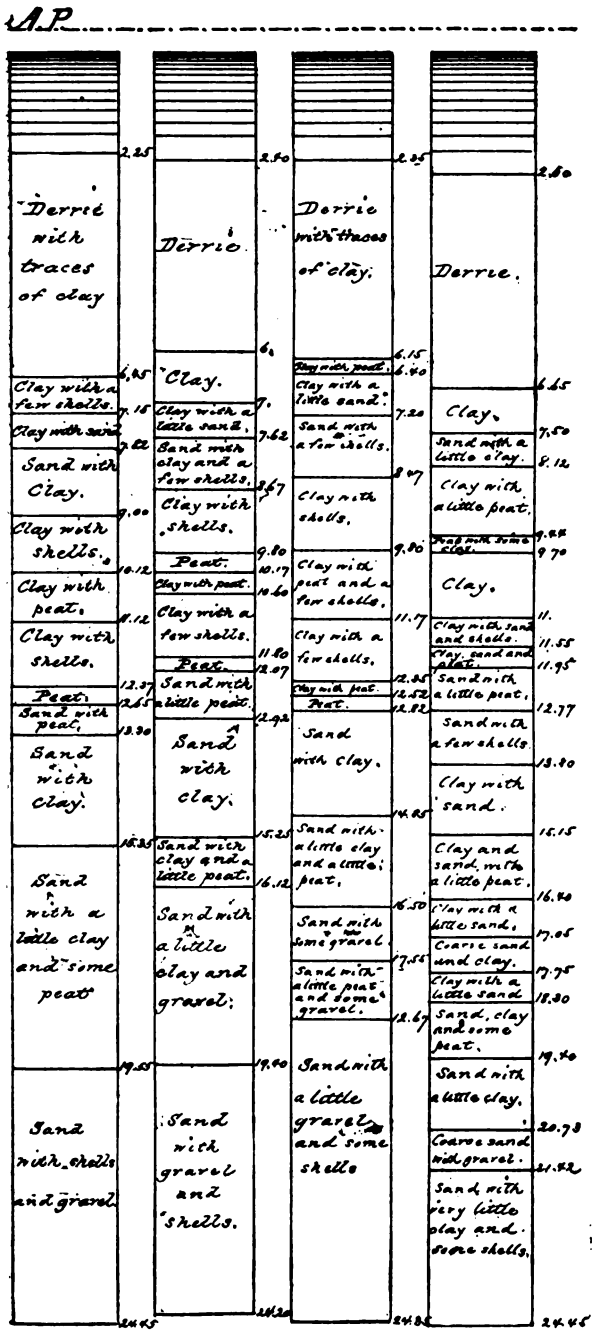
A.P.
Derrie with traces of clay
2.25
Clay with a few shells.
6.85
7.15
Clay with sand
7.22
Sand with Clay.
9.00
Clay with shells.
10.12
Clay with peat.
11.12
Clay with shells.
12.37
Peat.
12.65
Sand with peat.
13.30
Sand with clay.
16.35
Sand with a little clay and some peat
19.55
Sand with shells and gravel
24.45
Derrie.
2.80
Clay.
6.
Clay with a little sand.
7.
Sand with clay and a few shells.
7.62
Clay with shells.
8.67
Peat.
9.80
Clay with peat.
10.17
Clay with a few shells.
10.60
Peat.
11.80
Sand with a little peat.
12.07
Sand with clay.
12.92
Sand with clay and a little peat.
15.25
Sand with a little clay and gravel;
16.12
19.40
Sand with gravel and shells.
24.20
Derrie with traces of clay.
2.25
Clay with peat.
6.15
Clay with a little sand.
6.40
Sand with a few shells.
7.20
Clay with shells.
8.07
Clay with peat and a few shells.
9.90
Clay with a few shells.
11.17
Clay with peat.
12.35
Peat.
12.52
Sand with clay.
12.82
Sand with a little clay and a little peat.
14.85
Sand with some gravel.
16.50
Sand with a little peat and some gravel.
17.55
Sand with a little gravel and some shells
18.67
24.35
Derrie.
2.80
Clay.
6.65
Sand with a little clay.
7.50
Clay with a little peat.
8.12
Peat with some clay.
9.25
9.70
Clay.
11.
Clay with sand and shells.
11.55
Clay, sand and peat.
11.95
Sand with a little peat.
12.52
12.77
Sand with a few shells
13.80
Clay with sand.
16.15
Clay and sand with a little peat.
16.40
Clay with a little sand.
17.05
Coarse sand and clay.
17.75
Clay with a little sand
18.30
Sand, clay and some peat.
19.40
Sand with a little clay.
20.78
Coarse sand with gravel.
21.82
Sand with very little clay and some shells.
24.45

there is water constantly standing on both sides, and the brush consequently kept covered, and where the difference of level between the internal and external water is not very great. Since the use of railroads for building banks, with steam excavators and dredges, sand is obtained so cheaply and in such large quantities that it is now the principal resource. It is a refuse product of dredging, which is largely carried on in the river improvements and canal works. The author saw it used in enormous volumes in the construction of the new Maas bed, where there is sometimes a good deal of peat encountered; and he saw there, too, a remarkable instance of the displacement of the latter by the sand of the new embankment. The peat had been forced to the top, as though by the superior specific gravity of the sand, and it lay, or, as it were, floated there, as an irregular, foul-looking, wet mass, not unlike a manure heap in appearance. This, he was told, is a very frequent phenomenon.

FIG. 8.

The fascine dams at Neer-Andel and Slyk-Well are merely auxiliary and are not essential parts of the new constructions. The most important, laborious and expensive part of the work is, of course, the construction of the new bed. The Maas is a river which discharges, at the highest water, some 2 700 cubic meters, or nearly 100 000 cubic feet per second. This is, perhaps, half of the high-water discharge of the Arkansas or White River. While no great matter for a living river, it is a pretty formidable flow to provide for artificially. The new river bed has an extreme width of 500 meters or 1 640 feet, and an extreme depth of nearly 8 meters, or about 26 feet. It is built partly in excavation and partly in embankment. Extending entirely to the sea, it undergoes a change of condition as it progresses downward from the fluvial part of the Maas, and the bed must be conformed to this change. The Maas at Slyk-Well, like other rivers, has a considerable range or oscillation, namely, about 17 feet. This oscillation depends almost entirely upon the supply of water from above. It has therefore its high-water bed and its low-water bed. The water-level in the former case extends nearly to the top of the dikes. In the latter it is far below the level

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of the natural surface of the ground. The following, then, is a typical section of such a river:

FIG. 9.

Now, at the mouth, where it discharges into the sea or the estuary, there is no such condition. There are high waters and low waters, but they are dependent on the tides and not on freshets. The oscillations are daily and not annual or of long period. In short, the river has undergone a change, from the fluvial to the maritime regimen, and instead of two beds, one for summer and one for winter, we now have one only. In designing the new bed for the Maas, the engineers have had due regard to this condition. The low-water bed, in their project, keeps on increasing from the upper to the lower mouth. At the origin, the width of the "minor bed" is 170 meters. Eleven kilometers below it is 210 meters. At 22 kilometers, it is 265 meters. At 25, it is 315 meters. At the mouth, it is 495, or coincides substantially with the high-water bed. Not all of the "new river" is included between dikes. These extend for about 22 kilometers, or 13.6 miles, after which they cease, the new bed then using the antiquated channel of the Old Maasje to the estuary called the Amer, through which the stream flows into the Hollandsch Diep. The bed of the Old Maasje is only one-third large enough to accommodate the discharge of the new river, so it must be dredged and enlarged. The Amer is sometimes too wide, sometimes too narrow, sometimes too shallow, so it must be "regularized" by contraction, by widening or deepening, as the case may be. The bottom will have a slope of about 0.08 in 1 000, or about 0.42 foot to the mile.

The summer bed is made by excavation, the winter bed by embankment. As the quantity of material furnished by the former process is greatly in excess of the requirements for ordinary dikes, it follows that the enormous banks which are being thrown up are not, to use Mr. Schnebbelie's language, dikes properly so called, but partake of the nature of spoil banks. As has been before stated, for one-third of the distance the dikes have muck ditches under them. They are being constructed entirely by steam trams, standard gauge, with large iron ditching cars which hold 7 to 8 cubic meters each. The cars are filled by steam excavators. When the author saw the work they were excavating

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the muck ditch by steam dredges. The trace of the new bed consists of several gentle curves, which change direction to the right and left alternately. The slopes of the summer bed are regulated according to these changes so as to conform to the practice of Nature, the steeper slope being on the concave bank. On this side the inclination is 5 to 1, on the other 10 to 1. The dikes have a slope of 2 or 2.5 on the land side and 5 to 1 on the water side. The crown varies from 10 to 114 meters—say 33 to 374 feet. The berm is mostly 15 meters. The summer bed commonly lies close to one of the dikes, northern or southern, leaving a wide interval between its bank and the toe of the other dike. It is intended to use this interval as pasture land, protecting it from small summer freshets by a summer levee on the margin of the low water bed. This adjunct, by the way, is extremely common, and, indeed, universal, where the foreshores are wide, on all the rivers. Outside of the dikes again are canals, intended to receive the drainage of the polders adjoining the new river, which formerly emptied into the Old Maasje. These canals will discharge into the new river by sluices, or, if necessary, will be drained by steam.

The cost of this novel though not unprecedented enterprise is estimated by Mr. Schnebbelie at 32 000 000 francs, or something over $6 000 000. This includes rights of way and everything except the closing of the Heerewaarden overlaten and a bridge at Heusden. Of this sum the Province of North Brabant contributes $200 000 and the polders interested $400 000. The rest of the expense is borne by the State. Of course great consequences must be expected to justify such an outlay. The ends designed to be attained are :*

First, by closing the Heerewaarden outlets the principal cause of ice gorges will be removed. Second, by the exclusion of the Waal water and by the greater slope given to the lower Maas there will result such a depression of the water surface, that even after the suppression of the outlets at Beers and Bokhoven, the Maas will never attain its former heights. Thus the dikes will no longer be menaced. Furthermore, the lands adjacent to the river may be drained. It is not believed that the Waal dikes will have to be raised in consequence of the closure of the Heerewaarden overlaten.†

It is very interesting to the American engineer, especially to those

* Travaux Séparation de la Meuse et du Wahal.
† Beekman, "Nederland als Polderland," p. 23.

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engaged in the Mississippi service, to see what results Mr. Schnebbelie looks for and how high up stream the influence of these changes is expected to extend. It will be seen that his claims are very moderate:

Distances above origin of new river.	Reduction of high-water line.
0.3 mile	3.7 feet.
4.0 "	2.3 "
8.0 "	2.1 "
13.5 "	1.7 "
18.6 "	1.6 "
24.8 "	0.3 "

It may be mentioned here that the traffic on the Maas is inconsiderable above St. Andries, hence no injury in this respect is to be feared.

On the subject of outlets the testimony of the Dutch engineers was unanimous. The author gives such of their opinions as he made notes of:

"It is an axiom of river engineers that every river must carry its own water to the mouth. * * * * The Waal is injured by the overlaten from Heerewaarden to Loevestein. At high stages, as the river is loaded with silt, this latter is dropped by the sudden loss of velocity, so that recently considerable sums have been spent in restoring the depth by dredging. The outlet is also dangerous as regards ice gorges. Breaks in dikes have often the same consequences as overlaten—that is, a diminution of velocity below the crevasse, and hence ice gorges and new dangers.*

"The Dutch experience with outlets is all against them. They cause shallowing of the river, and hence (especially) ice gorges.†

"The overlaten were dangerous and detrimental on two grounds. First, they caused a slackening of the current, and, consequently, deposit and shallowing; and, second, by the same slackening the movement of ice was impeded and gorges formed. The principle is now utterly condemned.‡

"Formerly along all our large rivers we found such overlaten, which were made to allow the water to escape sideward, especially by an ice gorge. This system is generally out of practice at present, as it is stated to be no good at all. The river must be capable to carry off its own water. Therefore since 1861 we have been at work to normalize the rivers. * * * We may safely say that our wishes have been complied with in every respect, and the danger of ever having a flood again has vanished. The overlaten are filled up to the height of the dikes, and at present only two more exist in the southwest Maasdyk, namely, the Beersche Maas and the Bokhovensche Overlaat; but they will disappear as soon as the new mouth of the River Maas is ready."§

* Beekman, De Strijd om het Bestaan, pp. 49, 54.

† Notes of conversation with Messrs. Schnebbelie and Wisboom.

‡ Notes of conversation with Mr. Leemans.

§ MS. communication of Lieutenant Wenckebach, of the Dutch Engineers, 1889.

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The work of building and repairing dikes or waterways, and, in fact, public work of any kind, seems to be done almost exclusively by contract. The work is carefully measured and staked out beforehand, cross-sectioned, and an accurate estimate of it made, such as we make for a final estimate. Persons intending to bid may employ their own engineers and remeasure the work if they please, for their own guidance. They then bid, not so much per cubic yard, but a gross sum for the whole. The specifications are of two kinds, general and special. For work under the civil department, or Waterstaat, the general specifications are those to which allusion has already often been made (the Algemeene Voorschriften). This is a volume of 248 pages, and contains standing regulations for the execution of all kinds of work—earth and dredging work, the construction of straw mats and of brushwork, of stonework, of pilework and timber-work, of work in metals, in masonry, calking, glazing, stucco, paper-hanging, road making, etc.; the requirements for materials, the duties and rights of the contracting parties, the responsibilities of bondsmen, etc.; a table of weights and measures, the lawful hours of work, and a quantity of miscellaneous instruction. This volume is for sale for one florin (say, forty cents), and is made a part of every contract, except so far as it may be superseded by special provisions in each instrument, in which case the modifications are particularly noted. The general specifications for work under the military engineers are contained in another volume entitled "Algemeene Voorwaarden," which is altogether different in arrangement, but covers nearly the same ground. The particular specifications in each case describe first the nature of the work to be done, with the grades, slopes and measurements, the materials to be employed, and, in short, every detail which is required for a thorough understanding of the work. They are accompanied by accurate drawings. One feature of the Dutch specifications is a list of uniform prices, as it is called, which is to regulate any bills for extra work. A few of these prices may be interesting to American engineers:

For digging, carting or loading 1 M^3 of ground.....	4	cents.
" carrying with wheelbarrow 40 M. level or 25 M. not steeper than 10 to 1.....	5.6	"
" carrying the same distance further.....	3.2	"
" carrying with cart over 100 M further.....	6	"
" " " " each 100 M.....	3.2	"
" 1 M^3 coarse screened gravel, rolled.....	80	"

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For one ship's ton ballast stone, delivered and placed in position	92	cents.
" 100 pieces perkoenpalen, oak, 1.60 M. long	$5.40	
" one stone setter, krammer or brush worker, per day of 10 hours	60	"
" one earthworker, or common laborer	50	"
" one cart with horse and driver	2.00	
" carriage and team per day	2.80	

The laborers are all natives, and form a peculiar class, called familiarly "polder-Jans," or, as a Mississippian might say, swamp-Jacks.*

Steam power is used a great deal on large works.

The construction of new dikes is generally or always undertaken by the Waterstaat of the kingdom, at least on the mainland. In Zeeland, which maintains an isolated existence, the several polders bear all their own expenses in this way if they are able; if not, they seek assistance from the state, and are then called *calamiteuse* polders. After the dikes are once built, however, their maintenance and enlargement are committed to the local authorities. The organization of the Waterstaat is said to be somewhat complicated, and effort was made to ascertain only its prominent features. The head of the department is the Minister of the Waterstaat, Commerce and Industry. He has an advisory staff, consisting of an inspector-general (Mr. Caland) and two inspectors (Mr. Conrad and Mr. ——). These are chosen from the chief engineers, not by seniority at all, but by selection. There is a chief engineer in charge of the great rivers—Mr. Leemans—and one each for very great works, as for the new Maas Mouth (Mr. Schnebbelie) and for the Merwede Canal (Mr. Wellan). Besides these there is a chief engineer for each province, and these have subordinates, one for each arrondissement, or more or less, according to necessity. The chiefs report directly to the minister, the inspectors being the latter's professional advisers. The chiefs themselves are not necessarily chosen from seniority, though this consideration has a great deal of weight. The inspector-general has his own particular sphere of action in the assignment to duty of subordinates, etc. The department of river gauges, etc., is his peculiar province. The two inspectors divide the kingdom between them territo-

* Jan is a common term in Holland for any waiter, driver, or other person in a low or menial station. It is usual to summon a waiter by calling him Jan.

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rially, one having the northern and the other the southern department.* By the Constitution of 1848, the Crown has the supervision of everything that concerns the Waterstaat, including roads and bridges, whether the costs be paid by the general government or otherwise. The provincial estates have control, within their provinces, of all waters, bridges, etc., and are authorized, with the approval of the Crown, to make changes in the regulations appertaining thereto. The administrations of the waterschappen may propose such changes. The Estates have authority over all stripping of turf, in dikings, droogmakeryen, mines, etc., reserving the right of the Crown to delegate the immediate supervision of the same to agents appointed by it.

Local organization is somewhat irregular. In some provinces the polders are independent, and each has its own administration, with a presiding officer under one name or another. More commonly several or many polders, having the same interests, have united themselves into a *waterschap*, and maintain an active and powerful organization, with authority to impose taxes, etc. These bodies attend to the matters of general importance to the district, such as the drainage of the bosoms, defense against the outer water, etc. All the mainland of South Holland, all North Holland south of the Y and a part of Utrecht are divided into such great waterschappen, which also are found in other parts of the kingdom. They bear all sorts of names, *hoogheemraadschappen*, *polderdistricten*, etc., mostly derived from the middle ages. The powers and duties of these bodies are very varied, some having control of all matters relating to the common interest, the draining and letting in of water, the maintenance of the river and sea dikes, the levying of taxes for these purposes, etc. Sometimes the tie is much looser, and the polders retain these affairs within their own jurisdiction, only sending representatives to annual meetings of the waterschap administration for the discussion of some specially important subject. The individual polders, of course, have their own management, which attends to their local interests and economy, under general regulations prescribed by the Provincial Estates.

There is another kind of body charged exclusively with the supervision of the dikes, and called accordingly *dijksbesturen*. The jurisdiction of these corporations is very different in different places. One part of them which exists in nearly all the provinces is called the "Daily Boards," generally appointed by the Crown for a term of years. With

* Notes of conversation with Mr. Leemans.

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these are other members, either elected by the freeholders or appointed, by virtue of ancient rights, by corporations or even by individuals. Freeholders generally have votes in proportion to their possessions. The administrative board of a polder usually consists of a president or secretary, called bailiff, burgomaster or what not, and several members who go by the names of poldermasters, dike reeves, or the like.

The expenses of management, maintenance, repairs, etc., are usually borne by the landed proprietors in proportion to their property. In addition to the general charges of the waterschappen, the polders also have their separate budgets, which are sometimes considerable. As an example of these taxes, it may be mentioned that the acreage tax of the Rhineland waterschap from 1844 to 1867 averaged 1.40 florins per hectare, or say twenty-two cents an acre. The polder taxes were very irregular. In the grazing lands (the so-called cow polders), which lie comparatively high and have not expensive drainage, they amounted to from thirty-two to eighty cents per acre. In the deep droogmakeryen they sometimes went as high as $4 per acre. In the Haarlem Lake polder they were $1 per acre. The whole budget of the Rhineland district ranged from $100 000 to $120 000. That of the polders collectively was $160 000. For the whole province of South Holland, including the islands, the yearly contribution, in 1884, was about $1 300 000.* According to Baedeker, the annual expenditure of the whole kingdom for dikes is about $2 400 000.

A word should be said about the remarkable system of military inundations, by which it is proposed to lay certain portions of the country under water, in case of necessity, to obstruct the progress of an enemy. To give any intelligent exposition of this plan would require much more careful study than the author has given it, and much more information than he possesses. Suffice it to say that it is not designed, as might be supposed, to drown the enemy, but merely to make the roads impassable and the country untenable, and that it is necessarily applicable only to the low lying polder land, that is, North and South Holland and Utrecht. The inundations are to be produced by sluices specially contrived for the purpose, either so-called fan sluices † or sluices closed by stop plank, or

*Beekman, "Nederland als Polderland," pp. 125-132. Id. De Strijd om het Bestaan, 249.

† Waaijersluizen, an ingenious contrivance, with a gate composed of two leaves, each of which itself consists of two gates, at something less than a right angle to one another, and joined firmly together at the quoinpost. The second gate works closely in a sector-shaped recess in the wall of the head-bay. It is evident that the head against the one gate will neutralize that against the other.

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simply by cutting away certain overlaten. These works are placed under the guns of powerful forts. Of course, the inundations are to be accompanied by vigorous military and naval operations.

The sea-coast of Holland is said to be about 1 500 miles in extent. Probably about 800 of it is protected by dikes. The author has seen somewhere an estimate of the length of the river dikes, placing it at more than 800 kilometers, or say 500 miles. The dikes are only a part of what Holland has found it necessary to do, to build up a country. Of enterprises recently completed or still in progress, the North Sea Canal, which gives Amsterdam its access to the ocean, cost $14 000 000; the New Waterway from Rotterdam to the sea, through the Hoek van Holland, cost $13 000 000; the Merwede Canal, from Amsterdam to the Waal, will cost $10 000 000; the new Maas Mouth will cost $6 000 000. The State which has accomplished all this has a superficies of 12 738 square miles and a population of about 4 600 000. It is impossible to visit Holland without an increasing admiration of the plucky, free, intelligent people who, by sheer force of industry and genius, have created for themselves a country almost against the will of nature. Surely the Dutch indulge in no empty boast when they say: "God made the sea and man made the land."

DISCUSSION.

A. F. Shars, M. Am. Soc. C. E.—I think Mr. Starling is entitled to our most cordial recognition of his energy and ability in the preparation of this paper, as well as for the industry and earnestness displayed in learning a foreign tongue that he might possess himself of all the necessary data. The paper is the more interesting to me, as I am about to proceed to the construction of important works of a similar nature to these dikes, of which it treats. I hope I shall not be considered hypercritical if I venture on one or two suggestions on behalf of myself and older members of the Society as well as for the benefit of younger members who look to our published *Transactions* as textbooks of both example and authority.

First, no document presented here can be made too clear. There should be no opening left for any misunderstanding. I think it a mistake not to have translated all the Dutch words that he has given us. All through the paper words are encountered which are, indeed, eventually explained, and of which the meaning is secured. But it would have

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been better if, throughout the text, the author had used the English equivalents and exhibited the Dutch in brackets.

We have Hoofden, Zinkstukken, Droogmakeryen, belonging to no tongue that is classic to the American engineer; enclosed in brackets they might be interesting; the word "Perkoenpalen," we are told, is untranslatable, the reason being that even the Dutch engineers, themselves, are ignorant of its derivation. Now, the truth is, whatever we may say of the word, the American engineer has made use of the article ever since our profession has had a name in America. He has used it to anchor rafts in shallow water; to secure entanglements, whether of wire or brush, on the glacis or elsewhere in front of works; and to fasten sods on the face of dry sandy slopes; in short, for a hundred purposes, and as yet I have not heard that he ever encountered difficulty in securing them, if he called for anchor-stakes, or, more briefly, anchors.

Few engineers are permitted time to study rigid literary accuracy, but we may always keep in mind a remark of our own Hawthorne, "There is no word nor thought in any language that cannot be fully and accurately translated into the language of the American"—a thought of value to the engineer.

I beg your indulgence in making another criticism; I refer to the conglomerate statement of measures, the mixture of the American system of feet and inches with the metrical system of the European continent. If we wish to make our papers of the greatest use to our profession, we should, when using the foreign system, accompany it with the equivalent American value in brackets.

There is a doctrine of construction suggested in this paper that, I confess, surprises me. If it were simply an historical statement, I should have nothing to say about it. But, by the manner in which the statement is made, the author has conveyed what seems to be an opinion of his own so completely at variance with all experience and written testimony, that I think it extremely dangerous to leave it uncorrected. It is stated on page 577 as follows: "The material of the dike should, if possible, be clay, and should be taken from the outside. If clay cannot be had in that situation, then resort must be had to the inside. Sand has little cohesion and does not make a strong and water-tight dike." Farther on, the writer says: "There are examples of dikes that consist of very sandy material, with a dressing of only one meter of clay, and yet they turn water excellently. But it is easy to be seen that such a covering must be carefully treated and protected from injury, as, if the poor material be exposed, it cannot be trusted to exclude the water."

Now, as I have said, to an historical statement of the Dutch custom I have no objection; but I think Mr. Starling has misinterpreted the fact of the outside dressing of clay, in supposing it is put there for the purpose of making a water-tight bank. There can, I think, be no doubt

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that it is there for a very different purpose. It is there because, by reason of its unctuous nature, it presents a lubricated surface, when wetted, to the action of the current, and thus impedes the bank's erosion more effectually than a mixture of sand and clay. But beyond all doubt for purposes of a water-tight work, there must be with clay a mixture of sand or gravel, or, what is better still, of both.

I have never yet seen a satisfactory analytical statement, accounting for the fact that clay is of use in a puddle bank, though I know it is almost universally so employed. Is clay used to fill the voids of the grosser material, or are the other materials used to fill the voids of clay? Both doctrines are taught with equal positiveness by the oracles. Every engineer's experience has taught him this interesting fact, that a bank of hard pan, which is largely sand and gravel cemented with clay, is vastly harder to excavate than a bank of pure clay can ever become.

My own experience in very early life furnished me a useful lesson in this direction. It was the custom, forty years ago, to leave all the details of coffer-dams to the contractor, on whom rested the responsibility of raising the work. The contractor in the case to which I refer was the late John Duff, who afterward became the Vice-President of the Union Pacific Railroad, and died a millionaire. Mr. Duff took charge of all the details, and gave his personal attention to the construction of a coffer-dam which it became necessary to build in Codorus Creek, on the line of the York and Harrisburg Railroad, in Pennsylvania. The structure consisted of sheet-piling enclosed in the customary yoke timbers, and forming a chamber 6 feet wide in water at that season about 12 feet deep. The bottom of the creek was a thin layer of silt overlying solid rock. The chamber of the coffer-dam was filled with pure clay from the banks of the creek, and the pumping began. It was soon discovered that a useless and expensive structure had been erected.

About this time the waves of revolution had stranded upon these shores an accomplished engineer who, under the patronage of the Appletons, undertook the publication of a book that should be authority on the subject of bridges. I had become a subscriber to this work of George Duggan, and received the number containing the chapter on coffer-dams while Duff and the rest of us were scratching our noddles over the puzzling problem. And here it is proper to remind you that in 1850 there was very little engineering literature in these States. I think the *Engineer and Architect's Journal* of London and the *American Railroad Journal* of this city were the only papers that professed any interest in our work. Later came the Appletons', edited by Julius W. Adams, Past President Am. Soc. C. E. But the work of Duggan solved the difficulty for us. We read: "However general may be the opinion, it is certain that one more erroneous was never entertained than that clay alone is a proper material to make a good coffer-dam." "All clays, when used in a coffer-dam, require a mixture of sand and gravel." These statements

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are a summary of the entire treatise, which is carefully elaborated. It also mentions the fact that "in specifications for these works the usual expression is that a good retentive clay must be used."

I suppose that in very shoal water and in positions like those of the Dutch dikes, where the duration of exposure is not continued very long at any one time, it may be safe to use pure clay as a water-tight material; but certainly not in any considerable depth of water. To members who are interested in this subject I recommend this chapter from Duggan, the book having been presented to the Society by the late Mr. Trautwine. Many writers on this subject have given us the result of their experience since the time of Duggan, and none with more decided tone than the late William J. McAlpine, Past President Am. Soc. C. E., whose operations in sub-aqueous works we are all familiar with. Very lately—in November, 1890, and in August, 1891—Herbert M. Wilson, M. Am. Soc. C. E., presented papers to this Society on the Indian and American irrigation systems. Interesting and valuable statements on the subject under discussion may be found at pages 251–2 in the Indian paper, and pages 206 and 220 of the other, and doubtless also in other parts of the documents, though I do not happen to recall them at this moment.

Now, it would seem that in this day experienced professional men should not be found who can make the mistake of requiring the construction of a water-proof bank with pure clay. Not more than two years ago, however, I went to South America with an English engineer, a member of the Institution of Civil Engineers, who recommended a dam of masonry because there was not a mass of pure clay in the vicinity, the trouble being that all the clay he could discover was unfortunately mixed with sand and gravel. Important and disastrous facts like this must be my excuse for inflicting on you so considerable a discussion of what ought to have passed beyond discussion.

I observe a disposition to throw large cobble stones out of banks intended to be water tight. If the work is intended to carry a water conduit, and therefore subject to leakage, there may be reason in the precaution, though I very much doubt it; but an earth dam, which has been properly designed, will be improved by leaving the heavier material in the heart of the work, provided a suitable amount of good water-proof front has been left on the water face.

I observe with interest a statement on the 589th page of Mr. Starling's paper that may be adopted as an argument in favor of dams built of clear sand, notwithstanding the previous condemnation that material has received. He says, "At Domberg the encroachments of the sea have been so extensive that they are about to build a dike there to back up the streak of dunes that has already become too thin for safety, while the further inroads of the waves are prevented by strong revetments and a formidable series of hoofden [spur dikes], extending, it was said, all the way to Westkapelle. The dunes are justly regarded as of such

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value, that extraordinary pains are taken to preserve them. Where there is a high beach, extending as far as the flood line, this, of itself, will protect the sand hills from everything excepting very high tides. Where the beach is not sufficiently high, sometimes efforts are made to raise it artificially by means of brush or reed screens, which will collect the loose sand, as it is blown about by the winds, and cause it to lodge under their lee until the screens themselves are buried." Here, then, we have the sand dike, one of the most common contrivances of nature all over the earth, for protecting the interests of her other higher contrivance—man.

These sand dikes are more common on the Pacific slope of this continent than on the Atlantic. I have observed on the Atlantic coast of the United States that the dunes are universal, but not so the dikes, which seem to exist only on the steepest shores. The Pacific shores have a steeper declivity, and there we find the dikes in great perfection; but the dunes are farther inland, where they exist at all, and attain less height than on the Atlantic coast. A remarkable instance of such a dike extends along the beach north of the Columbia River, to Shoalwater Bay. The waves and heavy surf along that shore have pounded the sand into a most compact and water-tight mass, some 10 feet higher than the adjacent land on the inner side of the beach, and no water passes through this dike during the highest flood tides. I lived three years behind such a dike as this on Ferrol Bay of Peru, and had a remarkable opportunity to see its water-proof capacity; for a river, which should have flowed into the Bay of Samanco, having burst its banks, came down over the pampas of Chimbote, and was held in a great lake by this dike, and remained there until a channel was cut for its outlet.

I presume it is safe to say that every member of this Society, who has lived in the hemlock region of New York, can recall instances of the resort to sand for stopping leaks in dams, because the miller said that "sand would get there when nothing else would."

C. B. Comstock, M. Am. Soc. C. E.—Unfortunately for most of us, a book in Dutch is a closed book, so that a debt of gratitude is due to any one who will give, as Major Starling has done, the results of his studies and of his personal investigations on the ground, in reference to hydraulic works in the Netherlands. Italy and the Netherlands have been the pioneers in, and almost the inventors of, hydraulic processes, and despite the American tendency to think most highly of what we have ourselves done, no one is well equipped unless he also knows what others have accomplished.

In the levees on the Mississippi, a common type has been one with a crown 8 feet wide and with side slopes of 3 to 1, banquettes not being habitually used. In Europe, on the other hand, river levees usually have somewhat steeper slopes, and often have greater top widths and banquettes, whenever the levees are high. Thus on the Po below the

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Ticino the normal width on top for levees close to the river bank, established by the Superior Council of Public Works, is from 8 to 9 meters, with an external or land side slope of 2 to 1, and an internal slope of 3 to 2, above ordinary water, and of 2 to 1 below that level. At a meter and a half below the top of the levee there is a banquette from 6 to 10 meters wide. If the levee is well back from the river, the top width is 5 meters above the Ticino and 7 meters below, while the banquette is 3 meters below the top of the levée, and 5 to 7 meters wide whenever the levee is 6 meters high. The aggregate length of levees on the Po is 822 kilometers.

What is really needed for levees, is that the surface of the ground water as one follows it from the river side of the levee, at the high-water level, through the levee and the subjacent ground, to the ground water surface at some distance from the levee on the land side, shall everywhere be below the surface of the levee and of the adjacent ground. Now, where the levee is of good material, and below its foundation there is very sandy material, the above condition can be more cheaply fulfilled by a wide banquette on the land side than by a heavy levee without a banquette.

In this connection, the following notes as to German levees and levees on the Theiss, may be of interest.

Levees on the Rhine, in Germany.—The principal continuous levees begin at Cologne on the left bank and on the right bank below Cologne, at the boundary of the District of Düsseldorf. From this boundary the river valley increases from 1 to 10 kilometers in width, and near the Netherlands frontier is 25 kilometers wide. In this area are many portions of the old Rhine bed. These deserted portions in the course of centuries have been leveed, either against all floods or against summer floods, thus forming polders whose aggregate area is about 1 100 square kilometers. The levees are of three classes:

First.—Summer levees, which protect only against summer and not against winter floods.

Second.—Winter levees, which protect only against ordinary winter floods.

Third.—Bann levees, which protect against floods of all heights, save those produced by ice gorges.

The aggregate length of bann and winter levees is about 520 kilometers, or four times the length of this part of the river, and of summer levees is about 260 kilometers.

Almost everywhere the summer levees have bann levees or winter levees behind them.

The highest water at Cologne since 1816 was			9.52 meters, in Dec., 1882.	
" " " Düsseldorf	"	"	8.93	"
Summer levees are raised to.....			5.90	" on Düsseldorf gauge.
Winter " "			7.90	"
Bann " "			9.50	"

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The flood discharge of the Rhine is 315 000 cubic feet per second at the Holland frontier.

The levees are 2 meters wide on top as a rule, this being increased to 3.5 or 4 meters when used as a road. River slopes are one-third and land slopes the same where not exposed to water. Where they are arranged for overfalls the slopes are from one-sixth to one-tenth.

Elbe Levees in Prussia.—The aggregate lengths of the Elbe levees in Prussia, from the Saxon frontier to the mouth of the Seeve, just above Hamburg, is 445 kilometers. They rise to from 0.6 to 1.0 meter above the highest floods. In the upper districts they have usually top widths of about 2 meters, with slopes of 3 to 1 on the river side and 2 to 1 on the land side.

Where the levees are 2.5 meters or more high, a banquette from 3 to 5 meters wide is placed on the land side at 2 meters below the crown. In the lower districts the top width is at least 2.9 meters, or 4.7 meters if used as a road; the river slopes are from 2 to 1 to 5 to 1, according to the material, and the land slopes from 3 to 2 to 2 to 1. Where it has not already been built, a banquette both outside and inside, 5.9 meters wide, is aimed at.

Vistula Levees in Prussia.—On the Vistula in Prussia, the aggregate length of winter levees is 366 kilometers, and of summer levees 30 kilometers. The levees have a top width of from 3.8 to 5 meters, and in the upper district usually a river slope of 3 to 1 and a land slope of 2 to 1 or 3 to 2. The strongest levees are below the forking of the Nogat. The normal profile for these levees has a top width of 5 meters, the slopes being those named above; 3 meters below the crown there is a banquette of 5 meters width. These full dimensions as respects slopes and banquette have been rarely reached. It will be seen that banquettes and steeper slopes than 3 to 1 on the land side, are used on the Elbe and the Vistula.

The Theiss is 1 025 kilometers long from the point where it enters the Hungarian plains to its mouth in the Danube. On it a large number of cut-offs have been made shortening the river by 478 kilometers. The plain of Hungary subject to inundation is about 600 kilometers long. In 1879, in consequence of breaks in the levee above Szegedin, that city of 70,000 inhabitants was nearly destroyed. The flood was 9.60 meters above lowest water. Szegedin was from 2 to 4 meters below the flood level. In 1872 there had already been built 1 200 kilometers of levees, and since the destruction of Szegedin in 1879 the levees have been greatly strengthened and improved.

Between the Körös and the Danube, the following is the general type of levee adopted:

Height, 1.50 meters above high water.

Top width, 6.0 meters.

River slope, 4 to 1 below high water and 2 to 1 above.

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Land slope, 2 to 1.

Banquette on land side, 4 meters wide, and 1.0 meter below high water. Here, as on the Po, much greater top widths are given to levees than is usual on the Mississippi.

DESMOND FITZGERALD, M. Am. Soc. C. E.—The writer of this paper is entitled to great credit for the care and labor expended in presenting to the Society the results of his researches into the details of dike building in Holland. It enforces the magnitude and variety of the engineer's work, and the widely different conditions under which it is carried on in different portions of the globe. The principles, however, which underlie all public work are the same the world over, and must be met in one way or another, or failure will ensue.

I notice in the first place that Mr. Starling has called attention to the fact that slopes of less than 2 to 1 are considered unfavorable for the growth of grass in the Netherlands, and that therefore this rate is prescribed. This accords closely with my own experience in the case of the slopes of the embankments on the Sudbury and Cochituate Aqueducts of the Boston Water Works. I have found it extremely difficult to maintain grass slopes on inclinations of 1½ to 1. It is a constant fight with the elements, no matter what care is used. On northerly exposures the troubles are of one class, and on southerly slopes of another, but the final result is pretty much the same.

The Boston Water Works has been a fruitful source of experience in the line of earthen dam construction and of high embankments for the support of masonry structures. In regard to the latter, earth embankments have been carried to a height of nearly fifty feet by Mr. Davis and Mr. Fteley, with a settlement of less than 1 inch, but it is needless to say that they have been built with great care, of excellent gravel, rolled in thin layers. The dams have generally masonry cores, carried down to the rock, or to fine sand or other good bottom, at a considerable depth. In a dam now building we have carried the concrete wall down to rock, at a depth of 40 feet in one place below the surface. For permanent work, whenever the damages from failure are likely to be great, or loss of life comes into question, it does not pay to take any risks.

For one, I particularly appreciate the reference to the practice of the Dutch engineers in allowing plenty of margin against the upward pressure of water. I have seen works designed with an utter disregard to this law. It is no uncommon thing to see gate-houses, for instance, built with massive side walls but with hardly any protection against the blowing up of the floors, and, perhaps, it may not be out of place to emphasize what Mr. Starling has so well said on this point.

In sinking core walls or puddle trenches to considerable depth, great care should be exercised not to disturb the fine material in the natural soil adjacent to the trenches; in other words, not to pump too much

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muddy water; but how this is to be accomplished in many situations may well bother the engineer.

Bolton W. DeCourcey, M. Am. Soc. C. E.—Mr. Starling's "Notes on the Holland Dikes," is most interesting reading for members of the profession in the State of Washington.

The large area of the "tide lands and mud flats" belonging to the State and soon to be offered for sale, when properly reclaimed, will afford western Washington a large amount of level acreage of the richest alluvial soil for farming purposes, and will require a proper system of dikes, both maritime and fluvial, before it can be made properly available.

On page 580, where Mr. Starling speaks of tree protection, I would like to ask him if any use has been made of the matting tendency of the roots of the yellow or basket osier? I have seen them successfully used in cases of sliding banks, also as an excellent protection against the erosion by currents.

On page 582, regarding the ends of spurs, he says, "This is not according to our practice." I would ask what is the practice on the Mississippi and which does he consider the better? I would also ask if the problem to be worked out by the Mississippi civil engineers be not more difficult, preservation and improvement of the river navigation in that case entering largely into the question?

I was engaged on the Illinois Central Railroad, South, in 1882, and saw the floods in Mississippi and Kentucky, and also the high water of the following year, and remember the difficulties entailed by the tremendous force and the long duration of the flood.

The diking of the mud flats of the estuary of the Chehalis River will be probably entered upon in the near future. This is a large estuary with a total rainfall per annum of 5 798 707 200 000 cubic feet, completely protected from evaporation by the densest growth of timber and underbrush, and what may be called a supersoil of partly decayed vegetable matter as porous as a sponge, in which a person will sink from 18 to 24 inches as he walks. In making a reconaissance of seventeen days' duration in the autumn of 1889, through the Olympics, this so retarded progress that I found it impossible to do better than 5 miles per day.

The consequence is that freshets are not great, but the river carries silt all the year round, and what is called Grays Harbor, at the embouchure of the Chehalis, is a delta, the areas between the channels, which are perfectly defined at low water, being composed of this silt, there being only 2 to 3 feet of water over the greater portion at high water. There is a large acreage, which in the memory of parties living near was some years ago nothing but soft mud, acquiring its first coat of grass. The proper diking of these mud flats will concentrate the stream, and so improve the navigation. The system for providing the funds for this improvement and the personnel of the staff to be engaged in the superintendence of the reclamation, will have to be provided, and

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it is to be hoped that the best talent and skill will be procured for the task.

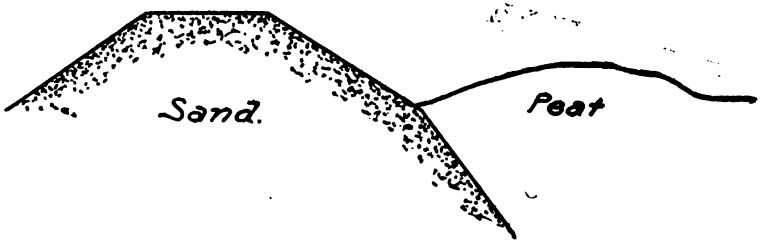
From the success secured in Florida by the use of bituminous rock in sea walls, it should be given a good test in maritime dikes and jetties.

W. HOWARD WHITE, M. Am. Soc. C. E.—Mr. Starling's interesting paper fills a considerable gap in the engineering literature of this country.

From his comment on the Dutch experience and maxims as to river overflows, I judge that he fully endorses the policy of confining the outflow of the Mississippi to a very limited number of mouths, and that he would certainly not advocate additional lateral outlets above or below New Orleans. His opinion on this point would be very interesting.

I note in the section Fig. 8, page 648, that the curving upwards of the sand under the peat seems to be an oversight. I take it that the section in such cases is rather as given below, the sand being prevented

FIG. 1.



by the lateral pressure from taking as great a slope as the natural one. The mud or peat being lighter, can flow or rise up, but the sand being the operating force, could never run out under the peat at a flatter slope than its natural one.

I take it that the use of banquettes for preventing the blowing up of the top stratum inside the dike must be limited to cases where the surface of the country inside the dike is generally (viz., except for short periods of time) above the water surface outside the dike.

Referring to Mr. Starling's Fig. 1, page 569, it seems evident that if the water surface were maintained, as shown, for a sufficiently long period, no banquette that could be built, either inside or out, would be of much utility, since the water pressure would gradually reach beyond the banquette, however wide, and blow up the soil at the first weak point found. If I am correct in this view, it would seem that the long periods of high water in the Mississippi must be unfavorable to protection by banquettes. It would appear in such cases that where the bottom muck is too deep to be cut down to impervious bottom by a puddle bank, the reliance must be on a secondary levee, such as Mr.

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Starling describes on page 571, in which a body of water takes the place of the banquette. Presumably the "sand boils," Fig. 6, page 603, act as safety valves in such a case, relieving the subterranean pressure to such an extent as to prevent the blowing up of large areas.

An interesting question somewhat connected with the dike problem, though really on a different basis, is the utility of berms on dam slopes as shown by the full line, Fig. 2. The argument for these is that they tend to check slides. At least this is the only one I have heard. I argue

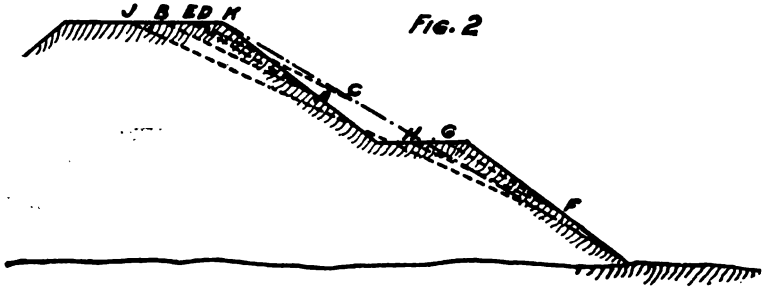

FIG. 2

that the same material put into the form shown by the broken and dotted line on the right-hand side of Fig. 2 would be less liable to slide and therefore less liable to cause damage than when the same material was formed into a berm, with steeper slopes above and below, largely on the principle that an ounce of prevention is worth a pound of cure.

A slide takes place ordinarily because the material becomes sufficiently saturated to make the angle of repose flatter than it originally was. Once started, it is extremely difficult to stop; first, because the sliding of the mass forms a more or less smooth surface for the highest material to slide down on; second, because the sliding rubs off more and more material; and third, because the removal of the material which has slid down tends to still further reduce the angle of repose by the very removal of the material on top of the sliding joint. We see the reverse of this process in the case of the pressure against a revetment wall, where the prism of pressure is only half as large as the mass above the angle of repose.

The possible slides in a dam with a berm divide themselves into four cases: 1st. When the sliding surface is steeper or parallel to the broken and dotted line and above the berm as shown by *AE*. It is obvious that in this case the amended profile is the best, since it would be unaffected, while with the berm profile a good deal of the crown would be lost, apart from the danger that overloading the berm would cause a slip below. 2d. When the sliding surface is flatter than the broken and dotted line and above the berm, as shown by *AB*. Here, the amended profile still has an advantage, since the part of the dam removed by the same angle of slope would be *CDK*, which is much less in amount. 3d.

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When the sliding plane is steeper than, or parallel to, the amended slope, and below the berm, as shown by *FG*. Here, the amended profile still remains more advantageous, since no slide would take place. 4th. When the sliding is flatter than the amended slope and below the berm, as per *FH*. Here, for the first time, the berm profile has an advantage, since the sliding plane on the amended profile would come inside of *FH*, as shown by *IJ*.

An important factor, then, is in what position slides generally occur. Since the lowering of the water in the reservoir is the most frequent cause of such slides, it seems pretty certain that they are mostly to be expected in the upper portion of the dam, which is more frequently laid bare. It is this portion, too, which is most liable to the action of frost.

Hence, as I said above, I see no advantage for a reservoir dam in using berms on the slopes, in preference to putting the same amount of material into a dam.

J. T. Fanning, M. Am. Soc. C. E.—Mr. Starling says in opening his paper, that "the most remarkable sea-works which he visited filled him with admiration, but also with despair, as there seemed hardly a possibility of ever being able to imitate them." The alluvial districts of Holland until within a few centuries represented the resultant of a long continued struggle between the rivers Rhine and Meuse and the North Sea. The power of the rivers was steadily and persistently transferring the foot-hills of the Alps into the bed of the sea and advancing the shore. The sea as persistently dashed back the deposits or spread them aside.

Then came man to possess and use the delta lands thus formed, and then began his efforts to wall out the sea and to confine the rivers in their many channels. First came the Batavians, then the Danes, the Normans, and then the Saxons.

After a moderate success in diking portions of the delta, there came a great storm, destroying dikes and giving victory to the waters, most disastrous to humanity. This victory was not final, however, for then the engineer, as a skilled designer and director, was called to be a leader in the contest. The works of the engineer in the Netherlands may well fill a student of them with admiration. He may be conscious of the improbability of ever being able to "imitate the most admirable of these works," but if he appreciates their true significance and receives their broad teachings, they will be an inspiration when some duty, that is great to him, comes in his way.

Venice is a city of canals, with the sea flowing and ebbing through at will. Amsterdam, on the other hand, is a city of canals with the sea shut out, or flowing and ebbing only at the pleasure of the engineer. Amsterdam was at first diked off from an arm of the sea, and then it was made a crescent city, with concentric canals that admitted maritime commerce to its heart, or permitted distribution of commerce near its circumference.

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The harbor of Amsterdam was, later, shut off from the Zuider Zee by a greater and more admirable dike, exposed to a long reach and powerful effect of storm waves. This great Schellingwoude dike is an illustration of many of the principles adapted to sea-works which Mr. Starling has industriously compiled and presented in his paper. It has five locks, the largest of which are adapted to sea-going vessels of heavy burden; it has inflow and outflow sluices in admirable masonries, and steam pumps of modern design. This great dike shuts out the eastern waters of the sea and creates a harbor protected from the waves, where a multitude of ships and steamers lie at anchor, moor at wharves, or enter the network of city canals in safety. It is also the eastern outwork of the modern North Sea Canal which gives Amsterdam commercial prominence among Dutch cities.

Here, again, in and near the canal and its Ijmuiden Haven, many principles which the author has stated are amply illustrated. This canal, 15 miles in length, extends westerly from Amsterdam to the North Sea. On either side of the channel may be seen canal embankments; polder levees; railway and highway embankments; canals for internal commerce and travel and for drainage; locks for the entrance and exit of craft; sluices for drainage and irrigation; steam and windmill pumps; meers lying lower than the ocean; dunes of sand deposited by the winds near the sea shore, and along the shore a great dike of sand thrown up by the sea. At Ijmuiden are locks, the greatest seventy-two feet in width, that pass ocean steamers as well as coasting packets and the fleet of fishing smacks that have quickly made Ijmuiden a fish mart. These locks represent, in foundations, masonries and gates, the condensed hydraulic experiences and skill of centuries in the Netherlands. The locks are founded in a broad and high ridge of sea-thrown sand. At Ijmuiden is also an artificial haven made by projecting two remarkable breakwaters, each three-quarters of a mile long, from the sand hill into the open sea.

This is all a field full of varied and interesting examples of hydraulic constructions, and the influence of its teachings now shows in several directions; for instance, in larger locks between Manchester and the Mersey Inlet, and at our own Sault Ste. Marie between the lakes.

The Haarlem-mer Meer Polder extends nearly to the North Sea Canal, and is seen on the left of the railway from Amsterdam to Haarlem. This polder has, perhaps, a larger place in the imagination of the American schoolboy than any other of the wonders of the Netherlands. He has read of the great lake of 70 square miles area that was surrounded with dike and canal and drained by three enormous pumps working nearly three years continuously, a half century ago, and of farms and homes thus made for ten thousand people.

The description of the first of these great steam pumping engines, with its largest steam cylinder 12 feet in diameter and smaller steam

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cylinder 7 feet in diameter, with 10-foot stroke, and the eleven great pumps arranged in a circle around, and all worked by this one engine, has often excited the interest of young mechanicians, as they have read that these pumps could lift 63 tons or nearly 17 000 gallons of water per stroke out of the polder drain. The writer, when in Holland, remembered that when the sea broke into the lands now forming this polder, it washed as far as the rear sluices of Amsterdam on one hand and Haarlem on the other, and endangered those cities. Naturally, he inferred that the famous "Leeghwater" pump must still be an object of interest among the generation it had protected. Alas! on diligent inquiry in a leading hotel in Amsterdam for directions to reach the "Leeghwater," no one connected with the hostelry had seen it or could tell where it was located. Soon after, in the City of Haarlem, inquiry was again made for the Leeghwater machine, first of several merchants near the railway station and then at the hotel, with fruitless results for a time; but at last, a guide was found who had once taken a party to a great steam pump some miles distant in the meer. This machine proved to be one of the two mates of the Leeghwater, the "Cruquis." These three ponderous pumping machines are at separated points in the circumference of the meer, the Leeghwater being in the south, the Cruquis on the west, near Haarlem, and the "Van Lynden" on the east.

Had Mr. Starling referred the "despair," mentioned in the opening of this paper, to his hopes of gathering in a foreign land so much valuable technical information as he has presented to us, he would have touched many a sympathetic chord among those who have tried with less success. Undoubtedly he could often give to those of whom he inquired more of the history of the specialties he went to study than they could give to him. This monograph will, next to the personal inspection and study of the interesting hydraulic works of the Netherlands, be rich in instruction, suggestion and inspiration to the engineers who have to direct works of importance such as are now frequently projected in America.

We still have opportunities for the best efforts in confining the Mississippi River floods and adapting the river channels to commercial needs; checking the meanderings of the Missouri River back and forth across its broad valleys; deepening the channels and constructing locks between our great lakes, and between the lakes and the ocean; transporting commerce between our Gulf and the Pacific, the Bay of Fundy and the St. Lawrence, and other equally grand works, for which this monograph offers suggestions in preliminary studies.

The surprising extent and success of the difficult hydraulic works accomplished in Holland may give us courage in projecting, and our capital confidence in aiding, great works that will be of much benefit, as it is now giving Dutch engineers and capital, courage to project the reclamation of 700 square miles additional of the shallow Zuider Zee. The les-

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son for us in this monograph is not only how dikes and sluices are made in Holland, but that these dikes and sluices, in their best forms, are the summations of thoughtful, earnest experience of several centuries in works whose failures, if they fail, lead to dire disasters. The monograph teaches also that the rolls of the storm wave and the floods of the river may be stayed and guided by "mud" if the mud be "mixed with brains."

Mr. Starling closes with the Dutch saying: "God made the sea and man made the land." The impression comes to a studious examiner of these lowland hydraulics, that God has there permitted the powers of the river and of the sea to long struggle together, and then has permitted the learned skill of the engineer to overcome them both and take the spoils, which are the Netherlands.

HENRY B. RICHARDSON, M. Am. Soc. C. E.—Mr. Starling, in this paper, has given the Society a condensed body of information relative to the dikes of Holland, checked by personal observation, which cannot fail to interest the profession, and to be of special service to those of us who are occupied with similar constructions.

The Dutch earthworks, designed for protection against inundation by river floods, appear to closely resemble in all material features the levees of this country on the lower Mississippi and its tributary and effluent streams. The form and proportions of the sections used under like conditions, the character of the material entering into their structure, and the dangers and defects that threaten their integrity, seem to differ less than the climates and customs of the countries they serve to defend. In both countries the systems of dikes and levees are plainly evolutions from small beginnings—probably as yet not fully developed in either.

Some of the most obvious points of difference seem to be the following: (1) In form of section and proportions, the Dutch works have generally wider crowns than ours—presumably due to the longer time during which additions to their width, or general enlargements of section, have been carried on. A large and increasing proportion of the levee work on the Mississippi River for the past six or eight years has been the enlargement of cross section, involving width of crown as well as other dimensions, so that in course of time it is probable that the width of the Mississippi levees may become as great as those of the Dutch levees.

The digging of "muck ditches" of some sort, as Mr. Starling notes —though not greatly favored by the Dutch engineers—is an almost invariable practice in the construction of levees on the Mississippi.

They are of undoubted service in cutting off roots and minor subsurface cavities caused by decayed roots or by burrowing animals, and their omission under any levee would be regarded with astonishment and alarm by most of the "oldest inhabitants" of the Mississippi Valley—

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who, by the way, are mostly hydraulic engineers of the practical school, with whom the "muck ditch" is a cherished institution—"tongueing and grooving," as it were, the levee to the solid earth, and among whom there is a prevalent belief that in Holland they cut their "muck ditches" at least as deep as the levee is high.

But it is not always apparent that such ditches as are usual—say, 3 feet deep—have any appreciable effect in cutting off "sipe-water," which always, to a greater or less extent, filters through the natural pores of the soil—and it has not been observed, in Louisiana at least, that in the few cases where much deeper ditches—say 10 feet and over—have been cut and carefully filled with fairly good puddling earth, that the total "sipe-water" was materially reduced, though it might show itself somewhat further away from the levee. But in numerous cases where old levees, dangerously leaky from perforations of burrowing animals, have had a muck ditch of ordinary dimensions cut along the foot of their front slopes, and have then been enlarged on that side so as to cover the ditch with the earth of the new section, the trouble has generally been reduced to insignificance, if not completely cured.

(2) In the character of the material of which dikes and levees are constructed, it would appear that little difference exists in the earth near at hand, of which they must be mostly formed, except that true clay is perhaps of more frequent occurrence in Holland than here, and that the Dutch have peat bogs to contend with, which are not to be found in the valley of the lower Mississippi. Those parts of the structure that consist of a protective covering of the earthen embankment, are naturally more elaborate in Holland than in this country, where the use of stone and brick for revetment has never been attempted—presumably on account of their larger first cost, and of the fact that few levees here are yet considered in such a state of completion that it is not expected they will at some time be further enlarged and improved. To undertake in this country, and upon levees which, within a few years, it is expected to enlarge and regrade, such careful and costly work in straw, brick and stone, as is applied by the Dutch to many of their embankments, would plainly be uneconomical, and the less expensive—if also less durable—wooden revetments in use here, are, perhaps, under the circumstances, better adaptations of means to ends than could be effected with less perishable materials.

(3) There seems to be a greater difference between the kinds of danger—if not in their degree—to which the river dikes of Holland on the one hand, and the levees of the lower Mississippi Valley on the other, are subjected, than in almost any other aspect in which they can be compared or contrasted; in fact, so great a difference in one particular—at least so far as stated in this paper—that it is to be wondered at that the author has failed to give it special notice.

Of the many dangers that menace the integrity and efficiency of levees

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in the lower Mississippi Valley, no one will deny that the tunneling operations of the various burrowing animals whose habitat is there, are among the most persistent and difficult to deal with. The crayfish, the fiddler-crab, the musk-rat, and sometimes the beaver and two or three other rarer burrowers, are certainly to be held responsible for great numbers of leaky places in and under the levees of the lower Mississippi, and several disastrous crevasses have been caused by their mining work. No doubt they have been charged at times with evil acts of which they may be innocent; but there are evidences enough that they should be classed among the dangerous and suspicious classes. If such pests are not to be found infesting the Dutch embankments, these are surely exempt from one of the perils which constantly threaten some of the dikes of this country.

In another respect the Dutch seem to be fortunate. The rivers of the Netherlands evidently do not eat away their banks in such an enormous and insatiable way as does the Mississippi, and their appetites in that direction are more easily restrained than that of the Father of Waters. Hence, with them, the necessity of falling back to new locations with the lines of levees is less frequent and less in amount than in this country. A few years ago official records showed that three-fourths of the levee work done in Louisiana, within the previous seven years, had become necessary on account of caving river banks, which required levee lines to be moved to new locations. The necessity for such extensive changes of location from this cause is undoubtedly rare in Holland, and it should be added, is becoming less frequent on the Mississippi, though the causes that may require it are still active in most concave bends of the stream.

Mr. Starling has noted the great peril caused to the Dutch river dikes by ice and ice gorges, and the fact that danger from this cause is unknown in the lower Mississippi Valley.

As regards the dangers due to soft foundations, and loose and porous "underground," it would appear that on account of the numerous heavy dikes built across old channels in Holland, the Dutch have probably had more difficulties, from the sinking and displacement of foundations, to contend with than have been encountered in this country, and it is instructive and encouraging to find that the experience of their engineers has led to greater reliance in such cases upon earth and earth alone, which is in accordance with the general practice here, rather than upon grillages of fascine work or like devices.

A precaution that should tend to diminish or utilize even the displacement of soft material underlying the site of heavy embankments, that seems worth taking, though often neglected, to their cost, by contractors, is to begin the bank in two parallel and separate lines near the edges of the base as designed for the main embankment. By this means a part of the displacements set up in each direction from the two banks,

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as begun, will meet in the middle of the main embankment, and, for the time at least, become either neutral, or only upward, in a useful direction.

The dangers resulting from pressure transmitted through loose and porous soils underlying tougher and less pervious strata, as illustrated by Fig. 1 of this paper, have not heretofore been generally considered by the levee builders in the Mississippi Valley of such a threatening and alarming character as it appears, from the extraordinary precautions taken against them by the Dutch, they are regarded in Holland. Were it not that the author states that five breaks, which took place in 1890 in one levee district of the State of Mississippi, are believed to have been due to this, it might, so far as previous recorded experience goes, be supposed a danger almost confined to the Netherlands.

It is true that opportunities for accurate determination of the immediate causes of crevasses are somewhat rare. Those breaks that occur unexpectedly in levees that have previously presented a safe and sound external appearance, are not often seen at the instant they break out, and usually they cannot run very long without so washing out the adjacent soil and flooding their entire surroundings, as to leave few visible indications of the causes producing them. It may, therefore, be possible that many breaks in levees have been due to this cause which have been attributed to other causes, or recorded as of unknown origin; but it may be stated that in the official records of the past quarter century relative to the hundreds of crevasses that have occurred in the levees of Louisiana, there is no mention or indication of the "blowing up" of a superficial layer of strong soil by pressure transmitted through or by an underlying weak soil. We may conceive an experimental illustration where an enclosed sand filter having its discharge orifice covered with a tough integument, is put under pressure till the tension on the covering membrane is sufficient to cause rupture. But in such an experiment we should hardly expect to see the sand blown out also, to any such extent as to produce a free flow of water; that is, to have the explosion of the membrane followed or accompanied by a crevasse in the sand behind it.

That such dangers as the author describes are possible must be admitted; but in the absence of direct proof that such "blow-ups" have occurred from pressure transmitted by semi-fluid soils, or by water pressure through the natural interstices of the soil, is it not a more reasonable hypothesis that in cases where such "blow-ups" have seemed to show themselves, the pressure that produced them has been transmitted directly through some vacancy or conduit of larger calibre than the pores, or natural voids, in any kind of earth?

However, if the danger exists, it should be guarded against; and the method employed in Holland of building inner berms or banquettes seems to afford at least a palliative remedy and is to be commended. In

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Louisiana it has long been the practice to build such banquettes across all considerable slough bayous and other undrained depressions. They have perhaps "builded wiser than they knew"; but it is not supposed by anybody that these structures were intended by their designers to meet exactly the same dangers for which it seems they are used in Holland.

The Dutch have unquestionably greater difficulties to overcome in reclaiming their country from the sea and in its protection against river floods than will ever be encountered in the protection of the valley of the Mississippi from overflow, but the situation and the opportunities for reclamation of both are sufficiently similar to make the methods and experience of the older country most interesting and instructive to those engaged in like undertakings in the younger. Our thanks are due to Mr. Starling for the carefully selected practical details of the Dutch practice as presented in this paper.

ARTHUR HIDER, M. Am. Soc. C. E.—The elaborate description of the dike work in Holland presented by Mr. Starling, is of great interest, especially to those engaged in levee construction along the Mississippi River, where the conditions are somewhat analogous. All the engineers engaged in this particular line of work are greatly indebted to him for his carefully prepared and complete description of these interesting works, the experience and methods in use there in many cases being very similar to our own.

In comparing the methods of the Dutch in dike work with the present practice of levee building on the Mississippi River, there seems to be a difference of opinion among the engineers in Holland, as well as those engaged in similar work on the Mississippi—this is the value and necessity of an interior core of earth or "muck ditch" below the natural surface of the ground under the base of the levee. The almost universal practice for years on the Mississippi has been the construction of these ditches in all new levees; they have been made of varying dimensions, from 3 feet on the bottom and 3 feet deep with side slopes 1 to 1, to the size stated in the original paper of 6 feet on the bottom, 6 feet deep and side slopes ½ to 1, and even larger.

Before discussing the merits of this ditch as an additional safeguard to the stability of the embankment itself, the nature and condition of the soil upon which the levee rests, as well as the method of its construction, should be understood. The soil is alluvial, and is composed of layers of sandy loam, sand, clay and silt of different degrees of tenacity and porosity; these layers vary in thickness from 2 to 25 feet or more, and occur in irregular order; at some points the top layer is sand, at others clay or silt, the relative position of the layers depending on locality. As a general thing, the lighter strata, sandy loam and sand, are found on the highest grounds, or "ridges," as they are termed, and the denser clay and silt in the swamps and low ground. As a matter of

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economy, the ridges are generally selected as the locations for the levees wherever practicable, and the result is that the levee is frequently upon an underlying stratum of extreme porosity which is several feet in thickness.

The present method of levee construction usually followed is, briefly: first, to clear the base of all undergrowth and trees, grub out the stumps and roots, plow the base of the levee, dig the muck ditch, and refill it even with the surface of the ground with the best material at hand, preferably by the use of teams and scrapers, and then complete the embankment to grade either with wheelbarrows or scrapers; no particular care being taken to build it up in layers, or to compact it more than would occur by teams passing to and fro. The embankment is then sodded with tufts of Bermuda grass 2 feet apart. The section for levees of heights not exceeding 15 feet now generally adopted is 8 feet crown, side slopes 3 to 1. In the older levees no effort was made to grub out the stumps; they were cut off at a convenient height and left standing.

The utility and necessity of an interior core or "muck ditch," where the top stratum is of clay of any considerable thickness, is questionable; the fact that the stratum of clay itself is impervious to water and reaches far below the interior core, would seem to indicate that the digging of the ditch would be unnecessary. Again, where the top stratum is of sand, the building of a core 6 feet or less in depth of clay into a layer of sand which perhaps extends 20 feet or more below it, would have little effect in preventing leakage due to hydrostatic pressure. Instances are common where driven wells taking water 30 to 35 feet below the surface become flowing wells during high water. A levee line of 4 miles in length is now being repaired, built a few years ago with the usual muck ditch, the foundation of which is about 8 feet of loam and clay, below this a layer of about 3 feet of sand, then again a layer of clay. This levee was considered dangerous, as "sand boils," some of them of large dimensions, developed on the land side, being more frequently close to the base, and occasionally as far as 150 feet beyond. Innumerable other instances could be cited where the "muck ditch" failed to prevent leakage.

Another fact that renders the construction of these ditches expensive or inefficient in many locations, is the difficulty of getting proper material with which to fill them. On sandy locations there is frequently no suitable material to be obtained nearer than a mile or more and the result is that the best of the material close at hand is used, or that the expense of this particular part of the work is increased from 200 to 300 per cent. Usually, the best of the material close at hand is used, but this is often ill adapted to the purpose. It is admitted that where the ditch is properly constructed of good material and extends through porous ground into a thick stratum of clay, it is a desirable adjunct to the stability of the levee, as well as the most economical method of preventing leakage, but it is

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believed the instances, taking the whole line of Mississippi levees, are few where these conditions exist.

The practice of adding banquettes to the Mississippi levees to give additional security on weak and bad foundations has not been nearly so generally followed here as in Holland, although within the past two or three years this method has come into more favor. From an observation of several years, it appears that the addition of a banquette 20 feet or more wide to the land side of all levees of over 6 feet in height, would afford greater security than the construction of the "muck ditch," which under the peculiar conditions of the soil is of doubtful utility for the purpose intended, the cost of the "muck ditch" as now constructed should be put into the banquette. In exceptional cases the "muck ditch" might be used to advantage. This would afford a larger section where most needed, preventing sipage and leakage near the base, and the danger of sloughing off of the back slope, which occurs often when the levees have become saturated after the water has stood against them for a long period. The additional earth would reduce the danger from percolation and sipage of water through the levee near the base, and the added weight of the banquette would overcome the tendency of the water to force its way up on the inside near the base, due to the head on the other side. The banquette would afford a roadway at all seasons of the year without injury to the main levee, an important consideration during high water, when the surface roads due to sipe water are practically impassable to vehicles.

The cost of adding a banquette 20 feet in width to the main levee with 8 feet crown, front slope 3 to 1, back slope 2 to 1—compared with one of standard section mentioned above—would be:

For an	8-foot	levee.....	85	per cent.	of	cost of	standard section.
"	10	"	94	"	"	"	"
"	12	"	99	"	"	"	"
"	14	"	101	"	"	"	"
"	16	"	102	"	"	"	"
"	18	"	102	"	"	"	"
"	20	"	102	"	"	"	"

It will be seen that for levees up to 12 feet in height the cost would be somewhat less; between 12 and 20 feet in height, from 1 to 2 per cent. greater (putting the cost per cubic yards of the muck ditch the same as the embankment in the levee proper), but in practice the muck ditch is more expensive, so that really there would be no addition to the cost by the change.

It is believed that the modifications suggested would add to the stability of the levees and reduce the danger of crevasses. What the levees generally need is "more dirt."

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J. FRANCIS LE BARON, M. Am. Soc. C. E.—I have read with a great deal of pleasure the very full and interesting paper on the "Holland Dikes," by our esteemed member, Mr. Starling. This is a subject of much interest to engineers in many parts of this country, and especially in Florida and Georgia.

In those two States some beginnings have been made in reclaiming large tracts of swamp lands and many more projects are receiving attention. There are vast areas in both States which if properly drained and diked will be rendered the richest and most valuable lands now covered by water. I have lately been employed as consulting engineer, in preparing designs and formulating plans for the reclamation of the great Okefinokee Swamp, in Georgia, and also the Holpati Saw Grass, situated at the source of the St. John's River, in Florida.

Okefinokee Swamp comprises an area of 676.35 square miles, and the swamp and water shed together is 1 262 square miles. It is the source of the Suwanee and the St. Mary's rivers. The lowest part of the swamp is 107.5 feet, and on the northern and eastern edges it is 125 feet above the sea.

It is proposed to drain this swamp into the St. Mary's River, and the work is now in actual progress. The swamp being some 10 to 15 feet higher on the edges than in the center, it is possible to drain a large portion, about one-third of it, eastward into the St. Mary's River, without drawing off the water from the center, which can be drained into the Suwanee River, or later the whole swamp can be drained into the St. Mary's River by simply deepening the canal, provided the drainage of the first portion, which is now being done, proves a success, and the fertility of the soil sustains the expectations of the projectors.

This swamp contains a large amount of valuable cedar and cypress timber, and the projectors desired to so arrange the effluent canal that this timber could be floated out and return a profit, long before the land could be made available for cultivation.

The swamp is bounded on the eastern side or toward the St. Mary's River by a rim of sandy land, underlaid with clay at about 10 to 15 feet, and there are some half dozen small branches, tributary to the St. Mary's River, that take their rise in the eastern base of this enclosing rim or mound.

The St. Mary's and the Suwanee both issue from the southern edge of the swamp, and the first named swings round and runs almost due north for about 33 miles, nearly parallel to the eastern edge of the swamp, until about opposite its northern quarter, when it turns abruptly eastward and flows into Cumberland Sound. By cutting the canal through this surrounding rim, nearly east into the St. Mary's, the distance is found to be 6.98 miles. If the drainage of the swamp was the only object, the effluent canal would naturally be located through the narrowest part of the ridge into one of the several creeks before men-

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tioned, which would carry the drainage into the St. Mary's; but as it is desired to float out the logs as well and deliver them in the St. Mary's River, this location was found impracticable, as the creeks are all quite crooked and have very little fall, and the work required to straighten and clear them out so as to form a clear channel for the logs would have been very expensive. Furthermore, these creeks having so slight a fall for a long distance from their mouth, there would have been too feeble a current to carry the logs or prevent the deposit of sediment.

It was therefore considered advisable to locate the effluent canal on as true a grade from the swamp to the river as the nature of the ground would admit, and it was decided to locate it on the summit of the ridge or water shed between two of the nearly parallel creeks, and through this a grade was established of 5 feet per mile. Thence a grade of 33 feet per mile for 1½ miles, and from that point to the river a grade of 12.2 feet per mile were secured. These grades would give a strong current computed by Hagen's formula, $V = 2.425\ R^{\frac{1}{2}}\ I^{\frac{1}{3}}$ (meters) of 10.77 feet per second for the steepest part, and 6.55 feet per second for the remainder to the river, which would allow of the delivery of 1 440 logs per hour in the St. Mary's.

The bottom of this swamp is found to consist of 5 to 7 feet of rich vegetable mould, somewhat peaty, and below this a stratum of sand and then clay. The dip of the swamp appears to be about 1 foot per mile, falling from the river toward the center.

Feed gates for regulating the discharge are provided at the mouth of the canal where it leaves the swamp. Hagen's formula for V has been tested very carefully on the Seine and Memel rivers, and found to be more nearly correct for such rivers than even Kutter's.

By constructing a flume 4 x 4 feet, 3½ miles long, to the St. Mary's River, on trestles, at a cost of about $30 000, we have available a water power of over 6 500 horse-power, by means of which the logs can be sawn into lumber as fast as delivered by the canal. Of course, a much larger flume might be made, and the horse-power increased.

The Holpati Saw Grass is an impassable morass of 112 000 acres, elevated 22 feet above the tide-water in Indian River, a salt-water lagoon on the east coast of Florida. The soil consists of black, rich muck, about 14 to 18 feet deep. The drainage of this immense tract is now entirely by the St. John's River, but it is proposed to cut the effluent canal east into the San Sebastian River, which flows into the Indian River. The St. John's River flows over 240 miles in a direct line, and probably over 450 miles by the sinuosities of the course, to fall 22 feet to its outlet in the Atlantic. There is no elevated ridge of more than a few feet surrounding this morass, and the length of the effluent canal will be 18 miles. Just north of this morass and adjacent to it, is a large saw-grass lake of some 12 800 acres, whose drainage is into the St. John's River. This whole region is so flat that its reclamation would

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make it necessary to provide for the drainage of 468 square miles of adjacent water shed, and when the water is lowered in the Holpati Saw Grass the large saw-grass lake would also drain into it. In fact, the whole upper part of the St. John's River and bordering swamps would have to reverse their flow for a distance of 80 miles, or at least down to Lake Poinsett.

It will, therefore, be necessary to enclose the whole immense tract with a dike 60 miles in length, or deepen and clear out the natural channel of the St. John's River for a long distance, and proportion our drainage canal to carry all the drainage of the water shed, or, in lieu of clearing out the river, dike off all of it immediately below our works by a dike not less than 20 miles long. These are the problems presented in this work. In the former case the dike must cross the mouths of three streams—Podgett's Branch, Podgett's Creek and Fort Dunn Creek, which are from 800 to 1 000 feet wide, but shallow—which now discharge into the Holpati Saw Grass. I have estimated the cost of this reclamation at $1 100 000, allowing 20 per cent. for contingencies.

The reclamation and drainage of the Sorasota Saw Grass, in this State, was completed in 1886, and has been reported by me in the Proceedings of the Southern Society of Civil Engineers. This work has proved very successful, and the land produces excellent crops. The cost was $6.30 per acre. Area drained, 500 acres; water shed, 3 500 acres; fall to outlet, 6.5 feet in 7 000 feet to tidewater on Sorasota Bay.

The drainage operations of the Disston Company in this State have proved very successful thus far in the upper portions, and large quantities of sugar, besides rice and other crops, are now being raised on some of their drained lands. They state that they have raised as much as thirty tons of cane per acre, equal to 6 210 pounds of sugar, twenty bushels of rice, worth $3 to $4.50 per bushel, and sixty to eighty bushels of corn.

Many years ago hundreds of acres of salt marsh in the town of Ipswich, Mass., my early home, were reclaimed from the sea by diking, with which work I was connected and thoroughly familiar. No engineer was employed, and the work was done by the farmers. Large areas were successfully reclaimed and the outlets provided with sluice gates of the flop pattern set in the dikes. The mistake was made, however, of not building the dikes strong enough and of suitable material. The marsh sods, cut from trenches just outside the dikes, were generally used. These sods are too light, as they float in water when only partially dry. The muskrats also proved deadly enemies to the dikes, burrowing into them and filling them full of holes. These dikes were about 10 to 12 feet wide on the top, with slopes of not more than 1 to 1, the same on both sides, and heights of from 6 to 15 feet. Complaint was also made of the flop gates becoming clogged by grass and driftwood, and in one case the gates were taken out, and a shutter gate, sliding up and down

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in vertical grooves in the flume, and worked by hand, was substituted. This was tended for a time by a man specially hired for this duty, but, as was to be supposed, he neglected it at times, and the result was several tides of salt water flooding the land, which, of course, killed all the sweet water grasses and crops. This work was done about 1855 to 1860. The result was that the dikes gave way and the land was flooded with salt water, and has been allowed to relapse into its original condition. The salt water killed the sweet grasses and plants, and for a long time the marsh was in a far worse condition than before anything had been done. This result may prove a lesson in such cases, and emphasize the fact that such works should not be undertaken except under the supervision of a skilled engineer, and that the details must be carefully attended to and the gates effectually guarded from fouling and made perfectly automatic in their action.

I have been especially interested in that part of Mr. Starling's paper which treats of the "hoofden" or spur jetties, as I have had some experience in these works, having been in charge as United States Assistant Engineer under the late General Q. A. Gillmore, Major J. C. Post and Captain W. F. Russell, Corps of Engineers, United States Army, of the construction, extension and repair of the spur jetties built on Amelia Island for the protection of Fort Clinch, Fernandina, Fla.

These jetties are situated on the northwest point of the island at the narrowest part of the gorge of Cumberland Sound, where the width to Cumberland Island is 5 900 feet. This width is increasing. In 1843 the surveys show that the shore line at Fort Clinch was fully 100 feet further out. In 1882, I made a complete triangulation and topographical and hydrographical survey of Cumberland Sound, under the direction of the late General Gillmore, and found that the gorge had widened several hundred feet since the last coast survey had been made. There used to be a lighthouse on the southeast point of Cumberland Island, the site of which has been completely washed away.

These spur jetties are 3½ miles from the outer edge of the bar, and are exposed at high water to the full sweep of the sea from the northeast, but at low water they are partially protected by the long north jetty on the north shoals and by these extensive shoals themselves, which lie 1 mile north, or just across the sound. There is 24 to 30 feet of water 200 feet beyond the ends of the spurs, which deepens rapidly to 60 feet. The mean rise and fall of the tide here is 5.80 feet and the mean velocity of the ebb tide is 2.00 feet per second and of the flood 1.80 feet; and the greatest surface velocity on the edge of the channel opposite the jetties was found by Mr. Paret (M. Am. Soc. C. E.) to be 5.00 feet, and the bottom velocity at the same place 2.80 feet per second. These latter velocities, being taken in the concave bend, are somewhat greater than obtain at the jetties, which are located on the convex side.

The jetties, as built, extend along the shore here for 4 000 feet, the

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fort being nearly in the center. The erosion of the bank has been so great here that the walls of the fort were endangered, and in 1881 these spurs or groins were built, and located about 570 feet apart and normal to the shore line, which gives them a radial projection, as the shore is here a convex curve of about 3 000 feet radius.

The jetties were constructed of log mats, 40 feet in width, the logs not less than 9 inches at the small end, and laid transversely to the axis of the jetty. The "mat" was simply a raft of pine logs, with binders spiked on 8 feet apart, with 20 to 24 inch spikes. Saw-mill slabs were spiked on the logs between the binders. They were covered with 1 foot of broken quarry rock, which was afterward increased to about 1½ feet. The rock was principally granite and gneiss brought out from the north in schooners. Only one course of these mats was laid, and they were about 3 feet in thickness, including the stone. They extend from high water to a few feet beyond low water, and the top has the same grade or slope as the beach, between high and low water. The cross-section is flat on top. Those near high-water line were built in place, and those near low water were built elsewhere and floated into position and sunk by throwing on rock.

They have withstood the sea well, and answer the purpose intended. At first only four, stopping at low water, were built, but it was found necessary to increase the number to seven and to extend them a few feet beyond low water. Since their completion, the erosion has practically ceased, and the beach remains normal. The sand piles up in the seaward angle, near the high-water mark, to the top of the jetties and the high-water line now extends about 100 feet further out than the original line on the east side of each jetty.

GEORGE W. RAFTER, M. Am. Soc. C. E.—Several years ago I was in the City of Cairo, at a time when the Ohio River was at full flood, the water in the Mississippi also being high. There was some apprehension that the levees of that water-locked town were not equal to the emergency. The water in the river stood several feet above the general level of the business portion of the town, and at a number of places "sand boils" were throwing out considerable quantities of water. To add to the general feeling of discomfort, the drainage machinery refused to work just at the time of highest water, though luckily repairs were soon accomplished and the inflowing water easily kept at proper level. I noted with surprise the large amount of water a "sand boil" would throw out without sensibly enlarging the crater or materially increasing in volume. Such observations as I could make seemed to indicate that the outflowing stream was the result of the confluence of a considerable number of very small ones, rather than a single direct connection from the point of outflow to the river, as otherwise it appeared difficult to account for their maintaining a uniform section at the crater.

The portions of Mr. Starling's paper which treat of the methods of

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administering public work in Holland are, perhaps, as useful as any to the working American engineer, in that they serve to enforce the proposition that good work is only secured by sound administrative methods. His remarks in reference to the regulations governing the excavation of borrow-pits and the dressing of slopes of the same by experienced slope dressers, is a good illustration.

The feature of the Dutch specifications of including a list of uniform rates at which extra work will be paid for, is one which could well be generally adopted in this country without injury to the interest of the parties for whom work is done. In railway construction extra work is usually of such a character that it can be classified under some item of the contract, and accordingly becomes, so far as final settlement of the account is concerned, practically contract work; but in national, state and municipal work (and especially in the latter), the most diverse practice prevails. In the bulk of new construction in cities, such as laying pavements, building sewers, extending water mains, etc., all the elements are known, and competition quickly reduces the profit to a narrow margin. The tendeney is, under these conditions, for the contractor to put as large a price on all extra work as the nature of the case will allow, and considering the unsatisfactory way in which municipal work is frequently prepared for letting, I am not prepared to say, absolutely, that municipal contractors are not, to some extent, justified in getting all they can.

In some cases, railway specifications in this country have called for the payment of a fixed percentage on the actual cost of all extra work, 10 per cent. being, so far as I am informed, the usual figure. Municipal extra work also is sometimes paid for in this way, though it is believed frequently at a somewhat greater rate than 10 per cent.

It may be of interest to note that the most complete account, in American engineering literature, of the drainage of the Haarlem Lake, is that contained in the chapter on drainage in Holland, in Colonel George E. Waring, Jr.'s, "Sewerage and Land Drainage." The history of the project from its inception, two hundred and fifty years ago, to its completion in 1858, is there given, together with many notes on the methods of executing the work which are not found in Mr. Starling's brief reference. The large pumping engines are also briefly described, and some illustrative sections given.

Mr. Starling's interesting statement, however, respecting the necessity for the construction of a subordinate levee, thereby forming a secondary polder inside the primary one, is a useful supplement to Colonel Waring's more extensive discussion of this phase of the subject, and may be considered as bringing our information in reference to the Haarlem Lake drainage down to date. One farther fact of some value in the present connection may, however, be noted, namely, the size of the pumping engines employed in this drainage. The original pumping stations were those mentioned by Mr. Starling at Spaarndam, Halfweg

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and Gouda. According to data given in Wheeler's "Drainage of Fens and Low Lands," published in 1888, the actual horse-power of the original machines may be stated to average, at Spaarndam, 280; Halfweg, 125, and Gouda, 360. In 1880 an additional machine was erected at Katwijk of 570 actual horse-power. The lift at this station varies from 0 to 7 feet, and the entire plant, consisting of six drainage wheels, can handle 2 000 long tons of water per minute to a height of 4 feet or 1 200 long tons per minute to the extreme limit of the lift of 7 feet. The combined machinery at Katwijk is further stated by Mr. Wheeler as being the largest installment of drainage pumping machinery yet erected in either Holland or England. Mr. Wheeler also gives the details of the mechanical features of these four large drainage plants somewhat more completely than they are elsewhere described in the English or American literature on the subject, his chief source of information being "Stoombemaling Van Polders en Boezems," by A. Huet, C. E., The Hague, 1885.

The Canal Pumping Works in Chicago may also be mentioned. These are designed to assist the outflow from the south branch of the Chicago River into the Illinois and Michigan Canal, and have a total capacity per minute of lifting 60 000 cubic feet of water to the height of 8 feet. This work is accomplished by four sets of centrifugal pumps (two pumps to each set), placed in a dug well below the surface of the water to be lifted, and driven by a compound, vertical, condensing engine set between the two pumps and coupled direct. The actual horse-power required when all are working at full capacity to the extreme limit of the lift of 8 feet is 910. On this basis we may conclude that this drainage pumping plant is much larger than anything in either Holland or England.

In Egypt, however, much larger plants than the foregoing have been erected and are in successful operation, the largest being those for assisting, during low water, the supply to the irrigating canal for the west district of the Nile delta, and the water supply of Alexandria. For this purpose two plants have been provided, one at Atfeh and one at Katatbeh ; the latter, erected in 1882, consisting originally of ten Archimedean screw pumps, each 39 feet long and 10 feet in diameter, with cores 4 feet in diameter. The Archimedean screws being found unsatisfactory, seven of them were removed the following year, and five vertical action centrifugal pumps substituted. The mean lift is about 8½ feet, with a maximum of 10 feet, and the entire plant is capable of discharging 2 500 long tons of water per minute to this height, yielding, when so doing, an actual net horse-power of about 1 700. So far as I have been able to learn, this is the largest pumping plant in use anywhere.*

* Further detail with references to the several sources of information may be also found in Wheeler's "Drainage of Fens and Low Lands."

The recently published "Special Consular Report, Canals and Irrigation in Foreign Countries," contains brief references.

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In conclusion, the brief references in Mr. Starling's paper to many of the cognate heads may be taken as indicative of the difficulty which the author has found in compressing all the detail within the limits of such monographic writing as is suitable for papers to be read before this Society.

ROBERT CARTWRIGHT, M. Am. Soc. C. E.—Major Sears has taken exception to the statement by Mr. Starling that pure clay is the thing for puddling. This Mr. Starling has evidently overlooked in his paper, because I am sure that he knows that pure clay does not make the best puddle. The best natural puddle we have is in hard-pan, and if any of you have worked in hard-pan, you will have noticed that a great deal of gravel is encountered in it; it is also pretty hard to work, while pure clay can be easily worked. My experience has led me to work about 3 to 1; that is, I take pure clay, cut it up, and to every three barrows of it dumped down an embankment, or anything of that kind, I dump down a barrow of gravel and sand.

I have built thirty-four gas holders, and to have a leaky tank is a serious matter in a gas-works. They are located in various sections of the United States and Canada, and that means a great many different kinds of soil. I would like to ask Mr. Herschel whether or not clay will saturate; he has said that it would dissolve down. In that case it must necessarily saturate—a fact which I deny. Certainly, the bottom of the great lakes, being clay, would have had ample time to saturate or absorb the water since the creation of them. The tunnels at Cleveland and Chicago have demonstrated that fact.

CLEMENS HERSCHEL, M. Am. Soc. C. E.—This touches on a subject upon which I have some strong feelings and opinions. We have just heard that pure clay does not make a good puddle, and instead of it is recommended a mixture of clay and of some other material, say of gravel. Now I desire to go a step farther; I do not want any clay at all. What in the world anybody wants to cart clay on the field of operations for, as is so frequently done, is something I have never found out. I have been at work handling water for a considerable length of time now, and I have never had any use for clay. Pure gravel, just as it comes from the gravel pit, will make a water-tight stop, when used between planks or in any other position for which puddle is used, as far as my experience goes, better than clay, or a clay mixture, ever did. Now, perhaps the reason that others do not say the same thing is, because they have never tried it. At all events I give you that as the result of my experience.

I would like to have my position clearly understood. It is simply this—that anything that can be done with clay, can be better done with good gravel. Clay will melt away just like sugar or salt; gravel will not. Clay is a dirty, slippery, disagreeable and treacherous substance to have around the premises; gravel is not. I have seen clay imported from a great distance in order to be put in bags and used to make coffer-

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dams, or something of that sort, water-tight, when three or four days afterward fifty per cent. of that stuff had disappeared. If, instead of getting that expensive clay, in the instance I have in mind, had gravel been put in those bags, it would have done the work better and at less cost. Now, I understand that the art of puddling is to deposit material in such way that every particle has been touched or permeated by water, and that water is used to make it settle down in a mass. Whether puddle be made of one thing or another, if it is handled in that way it constitutes puddle. I am a great deal like my friend, Mr. Worthen—I have not been brought up "that way"; we have got along pretty well without it, and don't want any clay, unless we are forced to build upon it.

In answer to Mr. Cartwright, I should say that "saturate" is hardly the word.

WILLIAM E. WORTHEN, M. Am. Soc. C. E.—In my practice I have never had occasion to make use of a clay puddle ditch in an embankment; there has always been some binding material like clay, loam or hard-pan in the excavation which answered my purpose, and the bank was made of a uniform material for its full width, in layers of about 6 inches and compacted by the travel of the carts which came up on one side of the reservoir dumped its load where required, and returned to the same side, each cart making a complete travel around the top of the work. When necessary, the earth was sprinkled as deposited and occasionally rollers were employed.

When the reservoir made by damming a stream, or hillocks break the continuity of the bank, the weakness lies in the natural earth and its connection with the dam at the sides and bottom, and if the dam be made of the same kind of earth, it will retain water better than the earth which has not been moved, and will be stable if there is an embankment sufficient to sustain the load and wide enough to make the water which might percolate through traverse so long a distance that its flow is checked to a mere ooze.

Phineas Ball, C. E., constructed a dam at Clinton, Mass., with a concrete wall in the center, but nothing but sand and gravel at the sides and beneath. In such a construction I have found that the water from the reservoir percolates through the gravel and stands at a head very nearly equal to that of the reservoir, at the wall, and close to the wall and on the opposite the side the head falls off, diminishing toward the foot of the slope.

At Lowell, where it is necessary very often to put in sheet piling to cut off the water of the canals from the excavation for new construction, the canals are drawn off and sheet piling is usually set and slightly driven in trenches about 4 feet deep, which are filled with puddle on the canal side. This puddle consists of a glacier till from the disintegration of granite rocks, which is scattered in water of very little depth, merely enough to wet the till. The mass becomes compact very quickly,

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and is always stanch, although it is subjected to the full head of the canal within a few hours.

ESTEVAN A. FUERTES, M. Am. Soc. C. E.—I have had a peculiarly fortunate experience in this matter. In about the year 1875 I built a system of water-works under the control of a literary corporation. I designed to pump something like 100 000 gallons per day into a reservoir, built on the French system; that is, without a puddle core or wall, which was in this case impossible on account of the fact that the site of the reservoir was a stratified clay and sand hill for an indefinite depth below the surface. Also there was not enough money for any other than the cheapest possible construction. The entire volume of the surrounding bank or enclosure was built from the material excavated from within the reservoir. Its banks have an internal slope of 2 to 1; a pleasure drive on the top, and sodded external slopes 1½ to 1. The specifications called for a surface puddle lining, of three volumes of brick clay (near by) and one volume of sand, laid 2 feet thick perpendicularly to the slope, or 4 feet horizontally. This puddle was to be paved with stones 10 inches deep, the joints in a vertical plane perpendicular to the top line of the bank, and grouted with Portland cement 2 to 1. The bottom of the reservoir had likewise a layer of puddle 2 feet thick covered by concrete 10 inches thick. This concrete was made up of one volume of Portland cement to three of sand, mixed dry, with five volumes of broken stone and enough water to need considerable and rapid pounding to make it sweat. The water supply was admitted, distributed and wasted through two 6-inch, one 8-inch and one 12-inch iron pipes cutting through the bank and terminating within a circular tower 9 feet in diameter. This tower was built at the toe of the inner slope, provided with gates, two separate chambers, an ornamental circular railing, and connected with the bank by a rustic bridge.

The committee controlling the works, though with the best intentions, harassed me not a little in the usual way; and when the reservoir was completed, in so far as form, puddling and paving, there was a breach on its north bank 37 feet deep, waiting for the pipes which should have been laid through the bank long before, at the time of its consolidation in layers of a small and uniform thickness. The pipes ordered by fast freight were at last found stranded on the Erie Canal at a great distance from the works, late in December, when grouting was deemed inadvisable. Now commenced the vicissitudes of the works, which if recited in detail could be made quite amusing and even of professional interest. The reservoir was filled with 1 000 000 gallons, and at once leaked at the center of water pressure on its south and tallest new bank. Despite my advice to let matters alone until the spring, the reservoir was patched up, by the advice of some Western practical man, whose professional dictum was always: "Plug her up, gentlemen, till she is tight." Thus the reservoir was riddled with holes in six successive fillings and an equal number of leakages.

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Though at this time I desired much to give up this job, I did not feel warranted in doing so for many important reasons, although even a practical farmer succeeded in having an order given to line the reservoir with straw during the winter months, now well advanced. When the spring opened and the straw was removed, I found, as predicted, that innumerable rats had made their home in the straw, and the stone pitching was covered with a thick coating of slime produced by the filtering of rain through the rotten straw.

The leaks were stopped completely as follows: sand in small amounts was sifted into the crevices, over the whole surface of the pitching. This was done by four men in six hours. Then, beginning at the highest point, water was played upon the sand through a hose, the sprinkler gradually working downward, till the stones appeared clear of sand on their external surface and embedded in about 2 inches of wet sand. On the next day grouting was poured into the joints by beginning at the lowest point, upon a strip 3 feet wide, along the entire bottom of the slope. Then the next higher band, 3 feet wide, was also grouted; by this method insuring complete filling of the interstices left in any one course by the downward leakage of the grout of the next higher courses; and so on till the entire internal surface was grouted. Two days thereafter, Saturday and Sunday intervening, the grouting had set, water was let in, and the reservoir has neither leaked nor needed the expenditure of one cent for repairs to date.

I may also mention that a filter had been built within the reservoir, made up of various layers of stone and gravel and topped by sand. This material was enclosed in a huge box receiving at the top the water to be filtered, and delivering the filtered water through a pipe at the bottom into the west chamber of the tower, and thence to the distributing main. The sand employed was of such a nature that it immediately became clogged and no filtration was possible.

We have in this interesting case a compact example of a puddle that would not hold water, and a sand that would become water-tight; but it would be hazardous to generalize upon these facts without reference to the physical nature of the materials and the wide variety of mechanical conditions under which they may be used. To my mind, the McAlpine axiom just quoted, that "water abhors angles," explains fitly the advantages of clay with sand when its use is properly indicated by favorable conditions. It is a fact that cannot be denied that a thick layer of homogeneous plastic clay exposed to moderate water pressure is impermeable as long as it is confined and cannot crack. The softening of the clay surface in contact with water helps the impermeability, by preventing cracks though capillary action, without giving rise to what is technically called a leak. In the reservoir in question, the puddle was confined on one side by the earth upon which it rested; and on the other, by the weight of the stone pitching. If the pressure of the

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water on the clay is great enough in comparison with its thickness and with the softness of its backing, the water pressure will punch a hole through the clay at some specially weak point. Under these conditions, the power of suspension of the water may become zero; but its transporting power will grow to be equal to that due to the head acting on the hole and punched through the clay. Erosion must take place. The leak becomes turbid or roily by the transported material carried off from the bank; and if left alone, the leak will continually grow. But if, when the leak starts, a pebble is placed in the hole, the area of delivery will be so greatly diminished and the resistance to the flow so largely increased, that the transporting power of the water may be reduced to zero and the power of suspension will again become correspondingly active; so that the slightest thread or smallest particle of clay will become clogged in the narrow orifice of delivery, which will evidently cease to be an area by contracting into a mere line, and finally into isolated points, easily closed by the feeble current into which suspended solid particles, each smaller than its predecessor, are drawn to cork the leak.

If these considerations are correct, and they seem to be, for clean gravel, or clean quartz sand, cannot oppose the passage of water on account of the inevitable presence of vacuities, no matter how compactly the material may be condensed, then the result recited to have been obtained with sand and gravel dams must have been secured by the adventitious circumstance of the presence—in the sand used—of a variety of small sizes of sand down to impalpable dust, which filled up and finally clogged up all the vacuities in the face of the dam exposed to leakage.

The question then seems to resolve itself to this: In the absence of actual experience with the sand or gravel of an unknown locality, is the engineer warranted in running the risk of leaks (which are generally serious in earth banks), when the use of clay can exchange the chances of failure for the certainties of success? It may be said that if the engineer has had any experience, he will not use improper gravel for this purpose; but I am inclined to think that only the engineer of limited experience or of great experience in a restricted area or locality will advance this argument. Although "dirty gravel" of all kinds may be found over extensive areas, embracing several of our States, there are, however, many places where only clean gravel or clean sand can be obtained, and the only way to make such material impermeable is to fill up its vacuities with material of varying sizes comminuted down to at least one size smaller than the size of the smallest particle of water likely to wend its way through the material of the bank. That such is the mechanical condition of tight banks may be also proved by the instance just mentioned in reference to the clogging of filters, which is a familiar experience. In these cases care is taken to regulate the de-

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livery of the filters by suitable choice of the sizes of the material employed, placing the finely broken material on the water or loaded side of the filter, and the coarser material on the delivery side. These filters invariably get clogged by the process above mentioned, which renders impermeable the water side surface, and the usual remedy is either reversal of the current to dislodge leaves, fibers, and their cementing silt, or by removal of the silt-compacted coating. It is from the formation of this silt-compacted coating that immunity against leakage is obtained. The recommendation to throw gravel over a leaky area, omitting the clay is, I fear, successful only because of the sand, clay or loam almost always present in a large number of gravel products, which is there in spite of the injunction to keep it out. The coarse gravel stops the outward motion of the finer gravel; the latter prevents the running of the sand; and the vacuities of the sand are clogged by the loam, clay or silt, which prevent the escape of the water.

CHARLES B. BRUSH, Vice-President Am. Soc. C. E.—Mr. Starling's paper is, I think, one of the most valuable and interesting that has been presented to the Society. I am surprised at the suggestions of Mr. Herschel and of Mr. Worthen to the effect that clay is unnecessary at all times in assisting an embankment to hold water. While it is true that under certain conditions very excellent results are obtained with the use of gravel, still equally good results are obtained with a proper mixture of clay puddle. For instance, I do not think it would be wise where rock had been excavated in the bottom of a reservoir to attempt to seal that bottom with gravel. In such case a clay puddle would be preferable.

In the case of a subterranean water supply, water is generally obtained from strata composed of water-worn or rounded stones. In making examinations for a water supply, we are almost sure to find it in such strata. Water is also found in large quantities in sand strata. Probably the most remarkable case of a subterranean water supply from sand strata is found at Memphis. The water-bearing stratum there is about 700 feet thick, and is composed of white sand of various sizes. Most of this sand will pass through a screen of 2 500 meshes per square inch. Eight million gallons of water were actually drawn from this stratum in twenty-four hours.

EDWARD P. NORTH, M. Am. Soc. C. E.—I think there might be some difference in the action of clays when exposed to water. I was employed on what is now a part of the West Shore Railroad in 1864–66.

On that road a bank some 60 feet high was thrown up from a wet clay cut. Shortly after it was built it commenced sliding, and in 1866 the base had gone fully 1 000 feet; the bank was still sliding in 1874, and, I believe, had not stopped in 1880. It is not clear what agitated this bank so that it would not set, and it is submitted that it must have been saturated with water.

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Another smaller bank about 30 feet high was built with wet clay, and there was a pool of water on the upper side of it. When gravel ballast was put on this it bulged on the lower side about half way down from the top. Any additional gravel placed on the road-bed apparently drove semi-fluid clay out from that bulge until the pool was drained, though at that time gravel enough had been put on the road-bed to make a gravel core nearly to the original surface of the ground. None of the banks on that road which were built of dry clay gave any trouble.

J. FOSTER CROWELL, M. Am. Soc. C. E.—I think my friend, Mr. North, has confused the question slightly as to the suitability of clay for puddle, by introducing another of its defects as a material for embankments. The kind of trouble that came from the clay in the railroad embankment he describes is not the same kind of trouble that has been under discussion. In the railroad embankment it is a question of gravity, and a wet clay bank that slumps under the weight of the track can sometimes be effectually cured by the use of sand or cinder (not slag) for ballast, which, being lighter, remains on the surface instead of sinking into and displacing the clay.

As to the relative value of clay and sand in an embankment to restrain water, a case in my own experience may be interesting. I was building a railroad across a corner of the Dismal Swamp, where it was desirable to avoid culverts wherever possible; to meet all the conditions, a water-tight embankment for the roadbed was essential. There being a good supply of fine clay near at hand, I used it in preference, but soon found that the bank was not water-tight and was rapidly and continually being dissolved away, so to speak, by the water, notwithstanding that there was seldom any current. There was fortunately a deposit of sea sand near by, and I put it on top and on the slopes, about 6 inches deep on the top and somewhat thicker at the toe of slope. The rains soon compacted the sand and drove it into the clay, forming with the water a practically impervious coating, and after that there was no trouble whatever. It was not, you will observe, the sand alone that did that; by itself the sand would have been too unstable even against the winds. I found, too, that clay and sand artificially mixed made excellent ballast for track, for which use neither alone was suitable.

So I think it is not exactly correct to condemn clay as a material *sui generis*. There are all kinds of clays, all kinds of gravel and all kinds of sand. As the artist said of his colors, they should be mixed "with brains."

JOHN BOGART, M. Am. Soc. C. E.—It occurs to me that in this discussion the word "gravel" is used by some of our members as meaning quite a different material from the material which is spoken of as gravel by other members, and I think that this difference of meaning exists in different parts of the country. At many places the word gravel is un-

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derstood to mean a mass of rounded stone of varying sizes. This sort of gravel occurs in very large deposits at many places and it is similar in the form of its constituent parts to the gravel of the seashore.

The other sort of gravel is made up of stone not rounded, but rather of flat shape, and with many particles of very small size, but still not rounded. It is with this latter sort of gravel that tight puddle can be made without the admixture of clay, and it is to this sort, doubtless, that reference is made in speaking of gravel used for puddle and without the admixture of other material. The method suggested by Mr. Worthen of determining whether a gravel will puddle would apply to this latter sort of material, but the gravel composed entirely of rounded pebbles of varying sizes will not make a tight bank alone, and in many places where clay occurs, and only this sort of gravel can be found, an excellent puddle is made by a suitable admixture of the two. This is the case in the banks of the canals of the State of New York at many points, in which a section of such puddled material is formed in the center of the bank, and which, when cut into afterward, is found to be compact and impervious.

W. R. Hutton, M. Am. Soc. C. E.—I find clay to be the very best material to make a water-tight embankment. I have not known it to crack—it has this disadvantage, that it is liable to be perforated by muskrats. A certain amount of clay is almost indispensable in making a water-tight bank, although a very small proportion in sandy or gravelly material will be sufficient, if well puddled.

Mendes Cohen, Pres. Am. Soc. C. E.—I have had some experience with just such slippy embankments as Mr. North has described, and have found the trouble due not so much to the material of the bank, as to the lack of proper preliminary drainage of the slopes on which it rested. As soon as this was effected the slipping ceased.

J. J. R. Croes, M. Am. Soc. C. E.—The relative merits of clay and gravel for restraining the passage of water through an embankment were discussed very fully by Mr. William J. McAlpine in a paper read by him in 1868, and published in Volume I, page 57, of the *Transactions* of the Am. Soc. C. E.

A favorite axiom of the author was thus enunciated in that paper: "Water abhors angles, and by compelling it to make a sufficient number, its head can be entirely destroyed, and prevent any damage." He continued: "Many young engineers fill their piling trenches with clay puddling, but the author greatly prefers fine gravel with a little loam mixed with it. The particles of clay are cohesive, and a vein of water ever so small which finds a passage under or through clay is continuously working a larger opening. The particles of fine gravel, on the other hand, have no cohesion. Such a vein of water as has been mentioned first washes out from the gravel the fine particles of sand, and the larger particles fall into the space, and these small stones first intercept the coarser sand and next the particles of loam which are driven in

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by the current of water, and thus the whole mass puddles itself better than the engineer could do with his own hands. An embankment of gravel is comparatively safe, and becomes tighter every day; one of clay is much tighter at first, but is always liable to breakage from the causes already mentioned."

Mr. McAlpine stated that he presented this paper to show his practical experiences, such as the substitution of gravel for clay to resist the escape of water in dangerous places, and the method of preventing the passage of water along smooth surfaces by fatiguing it with angles. This latter principle he illustrated by his use of sheet piling placed by hand instead of being driven, and with battened joints.

The doctrine of Mr. McAlpine with reference to puddling is, I think, sustained by the experience of engineers, in the Eastern States at least.

By the term gravel is not meant a collection of clean, round, water washed pebbles of nearly uniform size, but a combination of small stones, sand and loam so proportioned that all the interstices between the stones will be thoroughly filled by finer material. In certain proportions clay is valuable in such a mass, provided that it is so situated that water cannot get at it so as to wash out the clay, which consists of very fine particles capable of being washed away by the action of running water.

A good illustration of the behavior of clay when used for puddling material in such proportions that its removal from the mass by water very seriously disintegrates the whole mass, occurred on the Ridgewood Reservoir of the Brooklyn Water-Works, built in 1857-60. The reservoir and embankment were built of material taken from the excavation and well rolled and rammed. This material was the Long Island drift. Where the reservoir was excavated below the natural surface of the ground, the banks were dressed to a slope of 1.5 to 1, and a puddle facing 18 inches thick at right angles to the slope was put on, consisting of clay and gravelly earth in about equal proportions. This material was wet, and cut with spades. Above the natural surface, the puddle wall was carried over the natural surface to the center of the embankment, and then carried up vertically in the center of the embankment. These banks were faced with a slope wall of split boulders about 12 inches thick, with the interstices well filled with pinners. When the reservoir was built, the water dissolved the clay out of the puddle and the slope wall slid in some cases and settled back in others, and it was necessary to empty the reservoir and relay the whole of the wall in cement mortar. Since that time I have never attempted to use clay in puddling to which water could find access, and I think in general the less of such material that there is used in puddling a wall, the better the wall is.

WILLIAM STARLING, M. Am. Soc. C. E.—I believe in maintaining the purity of the English language, and am altogether opposed to the intro-

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duction of foreign words where there are equivalent expressions to be found in the vernacular tongue. Where these do not exist, it conduces to precision and brevity and avoids awkward circumlocutions to use the foreign term, having once explained its meaning. How will you render into pure English such words as gondola, kayack, howdah, samovar, diligence, boomerang? These words represent things which do not exist in England or America. Consequently, we have no names for them. You may, if you choose, call a gondola a barge, or a samovar a teakettle, or a howdah an elephant-saddle, but you will thereby convey an altogether imperfect and erroneous idea of the things alluded to. How shall we render the word *polder?* Shall we adopt the Indian plan, and call it a diked-field-which-has-been-pumped-out? and shall we use this paraphrase whenever we wish to speak of this thing? A *droog-makery* is something which has no existence outside of Holland, so far as I know, and there is therefore no equivalent for it in any other language. I suppose a *perkoenpaal* might be called an anchoring-stake, or a *zinkstuk* a mattress, or a *hoofd* a spur-dike, but those English expressions would convey only misleading ideas of the things themselves, which differ widely from the types of the same constructions which prevail among us. The outlandish words used are not more than half-a-dozen in number. The above list comprises nearly all of them. To learn that much Dutch cannot be a serious strain to a moderately robust intellect.

Metrical measures are used in the paper when the dimensions are derived from foreign sources—feet when given as the result of personal observation or estimation. It is to be presumed that every member of this Society is more or less acquainted with the metrical system. I have no special familiarity with it myself, but I am not puzzled when I am told that a bank is four or five meters high. I cannot think that is necessary, every time the word meter is used, to put after it, in parenthesis (3.28 feet). To render metrical dimensions which are given in round numbers into feet gives an appearance of spurious particularity. Suppose it is said that a berm should be 32.8 feet wide, the question immediately suggests itself, why this precise distance? But if the measure be given as 10 meters, we readily understand that no special exactitude is required.

The remarks on the use of clay as a material for dikes have led to a very interesting and instructive discussion. Some gentlemen seem to have interpreted them as a general declaration on the part of the Dutch engineers or myself in favor of the exclusive use of pure clay in water-tight bank of all kinds. It ought hardly to be necessary to say that this is a grave misconception, which could only have arisen from a very hasty reading of the paper, and a failure to understand the conditions of dike construction as they exist in the Netherlands and also in the Mississippi Valley. The remarks in question are a nearly literal translation of a passage in Storm-Buysing's "Manual," supported by the

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evidence of engineers of the present day. It was thought best to give these opinions on their merits, without the expression of any individual views, as was plainly indicated by the statement that they had been "compiled" from Dutch authorities. In the matter of dike construction, at least, the Dutch engineers are the masters of the world, and their expressions are entitled to great weight, even when they differ from our own notions. In order that it may be seen whether I have caught the meaning of the author correctly, I give a literal rendering of the whole passage from which the words of the text were extracted:

"The earth of which the dike is to be composed must be such as to cohere readily with itself and with the soil beneath it. The more cohesion the soil has, the more it is to be preferred; for the more will its different parts unite and form a compact mass, which can oppose a resistance to the water, and thus furnish a tighter dike. Clay is thus the most suitable earth for dikes, and for the most part is to be found along our coasts, where dikes are to be built. It is to be procured, by preference, from the outer side, but when the foreshore is scanty or wanting, it must be taken from the land side. Sand has very little coherency, and does not afford a water-tight and strong dike (belooft geen waterdigten en sterken dijk). Peat and swamp soil have too little specific gravity, often less than water itself, and must thus, as well as sand, be rejected from the dike. Mould or arable land, though far inferior to clay, is still much better than peat or sand, packs closely, by reason of the smallness of its particles. and is especially suitable for dressing slopes that are to be sodded, as grass grows very well upon it. Clay cannot always be had in a pure state or in sufficient quantity, so that inferior earths must sometimes be mixed with it. But if the precaution be taken to work the best and purest clay on and near the outside, and the inferior sorts in the body of the dike, such sorts may be used without great danger. There are examples of dikes that consist of very sandy soil and have a covering of only one meter of clay on the outer slope, yet they furnish very satisfactory dams. It is easy to be seen, however, that such a dressing of clay must be treated with the utmost care, and the slightest injury to the outer slope must be immediately repaired, for if so much of the clay be removed that the water comes in contact with the sand or peat, very little confidence can be placed in such a dike."

It will be seen that these opinions are expressed in the most emphatic terms, and they are confirmed by the General Regulations * and by information obtained from individual engineers.†

* "The least cohesive and least clayey soils are worked into the inner slope and under the crown of the dike; the heaviest and most clayey soil in or next to the outer slope."—"Algemeene Voorschriften," page 6.

† "At nearly every point where dikes are necessary along our rivers, we find an abundance of clay, which is produced by the river itself, and is of a very heavy quality. Of this material the dike is constructed. If there is only sand to be got, the dike is made a great deal larger, and the outside is covered with a thickness of clay amounting to from 0.50 to 1.00 meter. * * * Puddle walls or diaphragms of any kind are never used. An invariable rule is *that the best soil must be worked on the outside of the dike.*"—Lieutenant Wenckebach, of the Dutch Engineers, MS. Communication, 1889.

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As has already been intimated, these remarks are not to be understood as applying to water-tight banks in general—a subject which did not come within their scope at all. They are applicable only to dikes, and Dutch dikes at that, and must be understood with due regard to the limitations which govern that class of construction and the conditions which prevail in their own and similar localities. It seems that it is necessary to reiterate that the rules which are prescribed for the building of ordinary dams or reservoir walls are not necessarily or even usually applicable to the construction of dikes. How little I have succeeded in making this understood appears from the fact that my friend Mr. Cartwright supposes that I have advocated the use of pure clay as a puddle—an error which is the more difficult to understand as there has been no allusion to puddle in the whole paper—that method of construction being expressly excluded. It cannot be made too plain that dikes are necessarily very cheap constructions, comparatively low in height, and exposed to the action of water only for a limited time. They must be built of natural soils only, no mixture by artificial means being admissible. The choice of material is very narrow, being confined to such as can be obtained within the limits of an ordinary haul. All material must be put up dry—the levees being often half a mile or more from the river, and water being usually as scarce in the dry season as it is superabundant during the floods. As to gravel, it might as well be proposed to build a levee of diamonds. Neither in Holland nor in the Mississippi Valley is any gravel to be found in the alluvial region, except such as is brought in barges, hauled by rail, or mined at a depth of fifty feet or so beneath the surface. We are thus confined to clay, sand, and mixtures of the two in variable proportions. Often one class of soil predominates, but sometimes two or more are found together. In such a case, it is possible to keep the classes separate, though with some trouble, and in Holland, as we have seen, this is done, the clay being thrown upon the front or water slope, and the sand and loam constituting the body of the dike. In America we have not yet attained to these refinements.

Now, "clay" and "sand" are themselves very indefinite terms. I am a little surprised to see from some experienced engineers such hasty and sweeping declarations about clay that it is treacherous, slippery, that it melts away like sugar or salt. Surely, these gentlemen do not require to be reminded that there are innumerable earths embraced under the general head of "clay," possessing widely dissimilar qualities. The plastic clays of the Mississippi Valley do not in the least deserve these reproaches. On the contrary, they are justly regarded as the most valuable soil that we have for the construction of dikes—and I suppose, from what little I have seen and read of the clays of the delta of the Rhine that they are very similar to the Mississippi clays. The latter are bluish or blackish in color, tough and waxy in consistency,

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and resist the action of water like hard soap. When dry, they break up into innumerable small fragments—whence they are called by the general name of "buckshot" soil. When wet, these fragments swell and unite into a compact mass. With clay, as much depends on the method of handling as upon the nature of the material. The Mississippi clay shows to the best advantage when massed together. Its tenacity and cohesive power then come into play; whereas, if the particles become separated from one another and intermixed with particles of other soils, they lose this distinctive and valuable quality, and may then become unstable and treacherous. Banks of clay are most to be depended on if put up with scrapers or wagons, especially wheeled scrapers, where it is closely compacted together. Under these circumstances it will not slip or "slump." When put up with wheelbarrows, it is very granular in texture, and while still green is apt to become pasty under the action of water, and consequently to slip, that is, on the land side, for I have never known it to slip on the water side. After settling a year or so, it becomes pretty well compacted, and seldom slides, even when wet. When well packed, a "buckshot" levee constitutes a firm and tough mass, almost impervious to water, resisting the wash of rain or waves admirably, and without any disposition to slip, even though standing at a pretty steep slope.

The power of resisting the action of water is, of course, an extremely valuable quality, and it certainly is one of the principal reasons for placing it on the outer side of the bank, as Mr. Sears supposes, though not the only reason, as the Dutch engineers have assured us. Its usefulness in this regard is shown by the fact that it constitutes the outer layer of the hoofden, and of the bank-protections of different kinds.

Clay has one principal defect. It does not stand well at grade, but settles for an indefinite time and very irregularly, so that "buckshot" levees have to be repaired occasionally. However, such levees are very strong and trustworthy. Leaks in them, when they occur, are not usually dangerous, for they hardly "cut" or enlarge at all. Water may run over such levees for hours without destructive effect. A dike of this material, if the subsoil is good, may be of comparatively small dimensions and yet be tight and safe.

Pure clay is rarely to be met with. The clays of the Mississippi Valley are described by experts as being "largely siliceous" in composition, and in Holland it is known that the clay of the forelands, or recent deposits outside the dikes (the *battures*, as they would be called in Louisiana) contains a considerable quantity of sand, for which reason it is preferred for dike-building to the stiff or "fat" clays of the old sea-bottom, which are harder to work and more liable to crack and split.

I myself read with some surprise the emphatic condemnation by the Dutch engineers of sand as a material for dikes, and their declaration

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that it would not make a water-tight bank. I have had to build many sand levees, and have had very little trouble with them. I am inclined, therefore, to think that the words above alluded to are not to be taken too literally, but rather in the sense that a much greater quantity of this material must be employed to effect the desired purpose. Sand levees, when of sufficient size and thoroughly sodded, answer very well, always stand at grade, and often, though not always, appear to be perfectly dry upon the back slope. It is possible that some of the discrepancies of opinion and observation here, as elsewhere, may depend upon the looseness of our nomenclature. There is very little pure sand to be found in the alluvial soil of the Mississippi Valley. It is mostly mixed with sufficient soil to bind its particles together. The defect of a bank of this material is that even when sodded it will not long stand the cutting effect of waves or overfall. Should a breach occur, it widens with fearful rapidity.

The Dutch likewise have frequently been compelled to make use of sand for their dikes. In such cases, when the dikes are important, they have been made of enormous dimensions, and covered, as already stated, with a dressing of clay on the front slope. The great Petten-Hondsbosch and Westkapelle dikes, mentioned in the text, are examples of sand dikes.

To sum up the case: dikes can be built only of natural soils, such as are found on the spot without artificial mixture. Of these soils there are only two varieties, namely clay and sand, mixed in various proportions. Of these, clay, of the kind prevalent in the alluvial regions of Holland and the Mississippi Valley, is the heaviest, toughest, tightest. and least subject to erosion. It requires far less of it to answer the same purpose. Therefore, it is preferred. Sand, however, can be made to do, if used in sufficient quantity.

These remarks refer only to dikes, and have no application to elaborate works, designed to stand great heads, where expensive methods are admissible, and where other material, such as gravel, can be easily obtained.

In answer to Mr. De Courcey, I would say that so far as I know, no use has been made of the roots of the basket osier as a protection against erosion or sliding. We are not usually troubled much with the latter, and as to the former, our levees are generally too far back from the river to have a strong current against them. Where the velocity of the current is dangerous, we have sometimes used spur-dikes.

As regards the alignment to be given to the ends of spurs, I think no particular attention is paid to it. The spurs are made nearly of uniform length, and the line joining their ends is consequently about parallel to the bank. In "caving" reaches, the latter is usually concave in trace, and hence the line of spurs has the same conformation. This is contrary to the practice of the Dutch, as laid down in the text, which prescribes a convex plan.

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In Holland, as in America, the questions of channel-improvement and preservation of the banks have been closely conjoined with that of protection against overflow. However, those problems are immeasurably more difficult in the case of the Mississippi than in those of the branches of the Rhine.

Mr. Richardson refers to the burrowing of various animals as forming a serious difficulty in the way of maintaining a system of levees, and thinks that this matter demands special notice. So far as Holland is concerned, I have never seen any mention of this trouble in any of their books or specifications. I asked one of the most accomplished of the Dutch engineers, Mr. Leemans whether they had any difficulty in this particular, and he answered, very little—that occasionally they were somewhat annoyed by moles. I think that lower Louisiana must be peculiarly infested with pests in the shape of boring animals, from what I have heard, in the strong clay soil of that region. In Mississippi, public opinion is a good deal divided as to the part played by these creatures in producing leaks in or under the levees. Holes often appear, spouting clear or milky water, and sometimes continue unchecked for years. As long as they "run clear water" and are not formidable in volume or number, they are not regarded as specially dangerous, and generally nothing is done except to keep a close watch on them. Should they enlarge, or the water become discolored, they are usually "hooped," as the term is, that is, surrounded by a low semicircular levee, connected with the main levee at both ends, on the land side. This contrivance is exactly on the same principle as the Dutch device described on page 571. The "hoop" is not generally brought up to the level of the external water, for then the pressure would simply be transferred to the "hoop," which would not be desirable—but it is built 2 feet or so below the external level, so as to diminish the head, lessen the flow through the hole and, hence, the tendency to erode, and yet not excite a disposition to break out again elsewhere in the weak underground. Popularly, the crayfish are held answerable for most of the holes that appear, but I do not think a case has been made out against them. I have had many hundreds of so-called "crayfish-holes" explored and dug out, and usually a decayed stump or root was found to be the prime cause of the mischief. Where levees have been built through timbered land, though the stumps may have been closely cut down or even grubbed out, yet the lateral roots cannot be extracted. These penetrate to great distances, perhaps entirely under the base of the levee. Eventually they decay and form conduits for the water. It is true that crayfish are often or usually found in them, but it is doubtful whether they have any agency in producing or enlarging them. These leaks are generally found in levees known to be of faulty construction, or insufficient in size, and most of them disappear when the levee is enlarged. There are various animals that burrow more or less in levees, and elsewhere, such

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as field-mice, rats, etc. I am told that musk-rats are particularly troublesome in Louisiana. One bird even—the kingfisher—has been detected boring horizontal holes as much as 8 feet deep. It is thought, with much probability, that beaver have caused several breaks. These animals are becoming rarer and rarer. In Mississippi, I do not consider the danger from boring animals as serious when the levees are otherwise sound and strong.

The interesting question raised by Mr. Richardson as to the transmission of hydrostatic pressure through weak and porous soils under the dike, is worthy of careful consideration. It may be, as he says, that where crevasses have resulted apparently from this cause, cavities of large extent existed in or under the bank. This is certainly possible, though in my opinion not likely, at least in all cases, for many crevasses of this nature have occurred in new levees, where there has been no time for the formation of such voids. However this may be, the provision against such a possibility would be of the same nature as has been suggested for the other case, and the frequency of breaks from underground pressure of some kind demands such a provision.

Both Mr. Richardson and Mr. Hider seem to agree with the Dutch engineers in attributing small importance to a "muck ditch." I think they have the strength of the argument on their side. Nevertheless, it appears to me that in those cases where I have used large and well-constructed muck-ditches, they have cut off considerable leakage.

Mr. Hider's remarks on banquettes I think perfectly just. There is another point of view from which the employment of banquettes appears to be correct practice—and that is the altogether faulty cross-section of levees built on the ordinary model, as regards hydrostatic pressure. The standard section of the Mississippi levees has a crown of 8 feet and slopes of 3 to 1 on each side. Such a levee, with the water standing at its very top, shows a proportion of breadth of base to height of external water as follows:

At the water surface	8 to 0, or ∞ to 1.
1 foot below surface	14 to 1, or 14 to 1.
2 feet "	20 to 2, or 10 to 1.
3 " "	26 to 3, or 8.7 to 1.
4 " "	32 to 4, or 8 to 1.
8 " "	56 to 8, or 7 to 1.
12 " "	80 to 12, or 6.7 to 1.
20 " "	128 to 20, or 6.4 to 1.

It thus appears that the greater the depth of the water, the less the strength of the levee, the weakness being manifested at the most dangerous of all places—at the base. The greater the width of the crown, the more conspicuously does this defect show itself. Now, a banquette gives strength where it is most needed, and in an economical manner. For this

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purpose and for most others, the shape shown in the newest Dutch constructions is the best, namely, with a cross-section of a long segment. In the case of the Mississippi levees, the banquette is intended to be used as a roadway; hence, it must have a regular crown. It is usually given a slope of 5 to 1.

In practice, it has been found that standard levees of 8 feet high or less never break; hence, the grade of the banquettes has been fixed at that distance below the crown. The Dutch usage is very similar.

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